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THE ULTIMATE STANDARD OF LENGTH

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Dr. Meggers has been Chief of the Spectroscopy Section of the National Bureau of Standards since 1920. He received his Ph D. from Johns Hopkins University in 1917. Figure 4 in the article below won first prize for Dr. Meggers in the black-and-white section of last year's Photography-in-Science Salon, an annual competition sponsored by the Smithsonian Institution and THE SCIENTIFIC MONTHLY.

IT NOW appears that the ultimate standard of length has been found in a wave length of radiation emitted by mercury 198, an isotope transmuted from gold by neutron bombardment. It will be shown that the homogeneity, reproducibility, and convenience of this standard cannot be surpassed by any other. It is, therefore, inevitably the ultimate standard of length, basic for the definition of all other units, including the meter.

The meter, as everyone knows, was designed about 1790 to represent one ten-millionth of the earth's quadrant. In 1827, some natural philosophers, meeting in Paris, speculated that the meter could not be reproduced if the form or size of the earth were changed by collision with a comet. This inspired Sir Humphry Davy, the English chemist, to propose as a natural standard, independent of terrestrial form, the diameter of a capillary tube of glass in which water would rise to a height exactly equal to the tube's diameter. Recognizing the defects of this standard, Jacques Babinet, a French natural philosopher, suggested that a wave length of light in a vacuum would be a better one. The same suggestion was later made independently by German, Dutch, and British scientists, but the first practical results must be credited to two Americans, A. A. Michelson and E. W. Morley, who, in 1887, outlined "A method of making the wavelength of sodium light the actual and practical standard of length." Their method involved the use of the optical interferometer, which they devised for their celebrated experiments on the relative motion of the earth and ether; it consisted of the measurement of a length by counting an equivalent number of interference fringes.

Although the meter was originally intended to be the 10,000,000th part of the earth's meridional quadrant through Paris, a platinum end standard (*Mètre des Archives*) of approximately this length was accepted as basic for the metric system until superseded in 1889 by the present International Prototype Meter, which is defined by the distance at the temperature of melting ice between the centers of two lines traced on the platinum-iridium bar deposited at the International Bureau of Weights and Measures. Within the limited accuracy of comparison the present Prototype is identical with the original meter, but its legal definition does not refer to any natural standards or to physical constants.

In 1889 Michelson and Morley described in detail a method of measuring the meter in light waves and stated that the brilliant green mercury line would in all probability be the wave to be used as the ultimate standard of length. When Michelson applied his interferometer to a study of the homogeneity of spectral lines, he discovered that atomic radiations, in general, were not strictly

monochromatic. In particular, he found that the green mercury line was one of the most complex in nature, and that the red light of cadmium was most nearly homogeneous.

Michelson, in 1892, went to the International Bureau of Weights and Measures and made the first accurate determination of the relation between the meter and the wave length of cadmium red radiation. This experiment was repeated in 1905 by three French scientists, and their value, 6,438.4696 A, or 10^{-10} meter, was adopted in 1907 as the *primary standard of wave length* and definition of the angstrom unit. During the next forty years the most precise measurements of length were made with this standard.

During World War I there was great anxiety and fear that a bomb might accidentally destroy the world's Prototype Meter. The best way to insure the meter against accident or change is to define it in terms of an indestructible but accurately and easily reproducible wave length. In 1927 the Bureau of Standards recommended that the International Conference on Weights and Measures do this, but a conservative Conference defined, *in a provisional manner,* 1 meter equals 1,553,164.13 wave lengths of red radiation from cadmium and explained that it was not a question of giving a true relation between the meter and the wave length, but only a metric value of the latter which could be modified by future experiments. In the next double decade the meter-wavelength experiment was repeated seven more times, thus making nine determinations in all. A summary, recently published by H. Barrell, of the (British) National Physical Laboratory, is shown in Table 1. The average deviation of one of these values from the arithmetical mean of all of them is one part in seven million, which is truly remarkable considering that each ruled line on the meter is ten to twelve wave lengths wide. Notice that the final average of all is identical with the value measured in 1905. This may be regarded as proof that in a period of forty-eight years the meter did not change its length beyond the limit of accuracy of these measurements. However, metal end gauges, susceptible of greater accuracy of wavelength measurement than ruled scales, have been found generally to change with time, and there is no a priori reason to believe that any material standard of length is strictly immutable. It may be assumed, on the other hand, that a wave length of monochromatic light, produced and observed under specified conditions, represents a permanent, reproducible, unchanging, and sharply definable unit of length; all

TABLE 1

VALUES OF THE WAVE LENGTH OF THE CADMIUM RED LINE IN TERMS OF THE INTERNATIONAL METRE (UNIT = 1×10^{-10}m)

DATE OF DETERMINATION	OBSERVERS	ORIGINAL VALUES	CORRECTED AND ADJUSTED VALUES IN NORMAL AIR	DIFFERENCES FROM MEAN	
				10^{-10}m	Parts per 10^6
1892–93	Michelson and Benoît (B.I.P.M.)	6,438.4722	6,438.4691	− 0.0005	− 0.08
1905–06	Benoît, Fabry and Perot (B.I P.M.)	6,438.4696	6,438.4703	+ 0.0007	+ 0.11
1927	Watanabe and Imaizumi (Tokyo)	6,438.4685	6,438.4682	− 0.0014	− 0.22
1933	Sears and Barrell (N.P.L.)	6,438.4711	6,438.4713	+ 0 0017	+ 0.26
1933	Kösters and Lampe (P.T.R)	6,438.4672	6,438.4689	− 0.0007	− 0.11
1934–35	Sears and Barrell (N.P.L.)	6,438.4709	6,438.4709	+ 0.0013	+ 0.20
1934–35	Kosters and Lampe (P.T.R.)	6,438.4685	6,438.4690	− 0 0006	− 0.09
1937	Kosters and Lampe (P.T.R.)	6,438.47C0	6,438 4700	+ 0.0004	+ 0.06
1940	Romanova, Varlich, Kartashev and Batarchukova (Leningrad)	6,438.4677	6,438.4687	− 0 0009	− 0.14
		Mean	6,438.4696	± 0.0009	± 0.14

experience supports this assumption. Although a wave of green light is only 1/50,000 inch in length, it can be reproduced within 1/100,000,000 of its length, and length measurements with light waves can be made with this accuracy. Even if the ruled lines on meters could be bisected to 1/100 of their width, length measurements with homogeneous light waves are capable of tenfold greater accuracy.

In order to attain the maximum accuracy of length measurements with light waves, it is necessary to employ the most homogeneous or monochromatic waves that can be found. Sixty years ago it was generally assumed that all spectral lines were monochromatic and invariable. In 1892 Professor Michelson devised an interference method of testing this assumption and found that most of the lines examined were complex; that is, instead of being single they consisted of two or more close components with unequal intensities. Michelson proved that the effective wave length of such a group of lines would depend on the phase relations and relative intensities of the components when applied to length measurements. He found the green mercury line the most complex, and the red cadmium line the most homogeneous, of any that he tested. Consequently, he discarded the green line of mercury as an ultimate standard and determined the number of waves of red radiation from cadmium vapor equivalent to one meter.

Michelson's discovery, in 1892, of spectral line complexity led to intensive investigation of this phenomenon, but an acceptable explanation of it was delayed for nearly forty years. Even though chemical isotopes were discovered in 1913, it was necessary to await the development of the quantum theory of atomic spectra before the complex structure of spectral lines could positively be ascribed to atomic nuclei. The actual phenomena are somewhat complicated, but they are simply illustrated by the mercury green line whose structure was first accurately observed and interpreted in 1931. Theory and experiment agree that this line has sixteen components as shown in the left half of Figure 1.

Natural mercury consists of a mixture of seven isotopes with mass numbers (relative to oxygen = 16) 196, 198, 199, 200, 201, 202, and 204. All are characterized by the well-known spectrum of mercury, consisting, in the visible range, of two close yellow lines (5,791 and 5,770 A), a bright green line (5,461 A), a strong blue line (4,358 A), and a violet line (4,047 A). To these (and other) mercury lines each isotope contributes one or more components of which not any are exactly coincident. The component displacements have two different causes, isotope shift and nuclear spin. Mercury isotopes with even mass numbers (but no nuclear spin) contribute single components shifted in accordance with mass as shown in Figure 1. The two isotopes with odd mass numbers, 199 and 201, have nuclear spins (of 1/2 and 3/2 units, respectively) which interact with the valence or optical electrons to produce close groups or clusters of lines called hyperfine multiplets. Thus the green line of natural mercury receives three components from Hg^{199} and eight from Hg^{201}, which, added to five from even-mass isotopes, totals sixteen.

The objectionable complexity of mercury lines could be eliminated if one even isotope could be separated from the rest, but until recently it has not been practicable to isolate or concentrate an isotope of natural mercury in sufficient

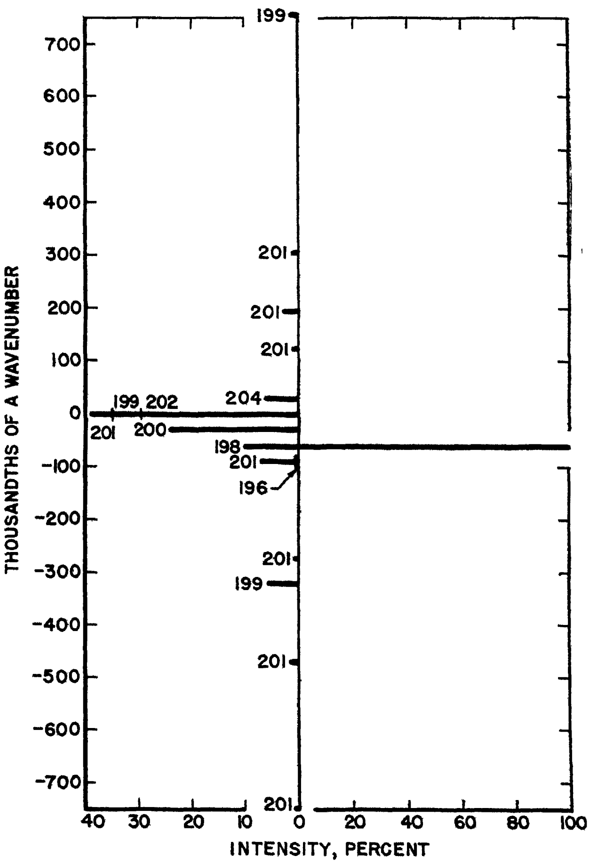

Fig. 1. (*Left*) The thick horizontal lines represent the 16 components of green radiation from natural mercury; their lengths are proportional to intensities (or isotopic abundances), and their isotopic origins are indicated by mass numbers. (*Right*) Green radiation from mercury 198

quantity to make any lamps. This most desirable end has now been attained, however, by transmuting gold Au^{197} into mercury Hg^{198}, thus reversing the alchemist's age-old experiment for the simple reason that mercury 198 as the ultimate standard of length is worth infinitely more than gold. The nuclear reaction that transmutes gold into mercury was first reported in 1934 from the University of Rome by E. Fermi *et al.*, who used a mixture of beryllium powder and radon as a neutron source to bombard gold. Neutrons that attach themselves to nuclei of gold atoms produce a highly radioactive isotope of gold which decays rapidly (half-life, 2.7 days) and becomes a stable isotope of mercury. Before 1940 such experiments yielded only infinitesimal amounts of transmuted elements, which could not be seen, weighed, or detected except by radioactive effects. Late that year L. W. Alvarez, of the University of California, boldly suggested that neutrons from a cyclotron might transmute sufficient gold into mercury to be detected spectroscopically. Exposure of one ounce of gold to neutrons for one month yielded enough mercury to produce a tiny electrodeless lamp that shone for about five minutes, during which the first interference spectrogram was made, thus demonstrating that the green line of mercury could be emitted entirely free from complex structure. This is shown diagrammatically in the right half of Figure 1.

Recognizing the importance of the experiment reported by J. H. Wiens and L. W. Alvarez, and wishing to increase the production of Hg^{198} so that one or more durable lamps could be made, the National Bureau of Standards purchased forty ounces of proof gold and requested the University of California to bombard it with neutrons for one or more years. Unfortunately, World War II interfered with this project, and only submicroscopic quantities of artificial mercury were made. The prospects were very discouraging until, near the end of the war, there were rumors of a secret source of neutrons thousands of times more effective than the largest cyclotron. In 1945 the National Bureau of Standards' gold was transferred from California to Tennessee. A year later the Bureau distilled from this gold about sixty milligrams of mercury, which was tested with the interferometer and mass spectrometer, and found to be highly pure Hg^{198}. Some of this Hg^{198} has been used in the preparation of several types of lamps. These are being studied to determine which will be best for length measurements.

The ultimate in simplicity of lamp construction has been found in an electrodeless discharge tube,

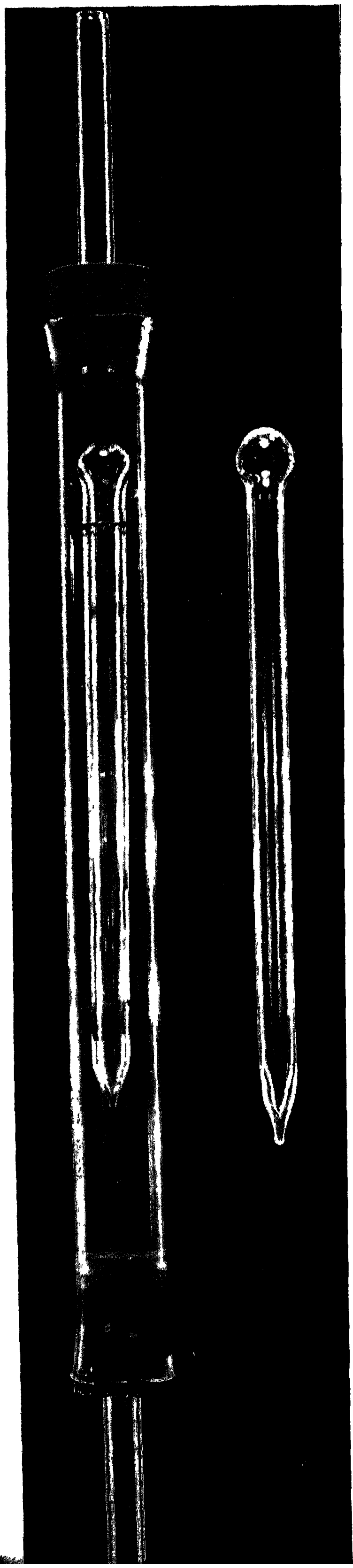

FIG. 2. Mercury lamps, either with or without water cooling, emit light when excited by high-frequency radio waves. A gentle stream of cold water around the lamp insures that the lamp is at a constant low temperature and that it will emit sharp lines without any self-reversal. (Lamps shown are about ¾ natural size.)

made by sealing small amounts of gas or vapor in evacuated tubes of glass or quartz (Fig. 2). When such tubes are held in a high-frequency electrostatic field, the inclosed gas or vapor emits its characteristic atomic radiations. By this method intense light emission can be obtained from mercury at extremely small vapor density and low temperature, which are necessary conditions for sharp lines. Several lamps of this type have been made, each containing a few milligrams of natural mercury or of artificial Hg^{198}. Green light from one of the lamps containing natural mercury was selected with colored glass filters and collimated to illuminate a Fabry-Perot interferometer, which consists simply of two silvered flat planes adjusted accurately parallel as shown in Figure 3. Multiple reflections occur between the planes, and when the transmitted rays are collected by a lens they form at the principal focus a system of circular interference fringes. Such a set of fringes was photographed for natural mercury; the photograph was bisected, and one half is reproduced in Figure 4. The lamp containing natural mercury was next replaced by one containing artificial mercury 198; the same interferometer was illuminated by filtered green light, and the fringes were photographed. One half of the interference pattern of the green light from Hg^{198} is reproduced in Figure 4 juxtaposed to that from natural $Hg^{196, 198, 199, 200, 201, 202, 204}$. Is there any doubt about which is which?

No matter how sharp and single-valued the Hg^{198} fringes may be, they are of no value whatever for length measurements until the lengths of the Hg^{198} light waves themselves have been accurately measured. This is being done by comparing them with the primary standard of wave length, 6,438.4696 A, provisionally adopted as the metric value of the red radiation from Michelson's type of cadmium lamp. Because of the relative coarseness of ruled-meter lines compared with light waves it is possible to measure one light wave in terms of another ten times more accurately than either can be measured relative to the ruled meter.

Comparisons of wave lengths by interferometer methods are among the most beautiful experiments in physical optics; in simplicity and precision they are outstanding among physical measurements. The experimental arrangement is shown pictorially in Figure 5 and diagrammatically in Figure 6. A mercury 198 lamp is imaged inside a cadmium lamp (or vice versa), and light from both lamps simultaneously illuminates an interferometer. The transmitted light is collected by an achromatic lens that images interference patterns on the slit of a spectrograph, which in turn forms a spectrum (by prismatic dispersion), and focuses monochromatic slit images with interference patterns superposed but without the confusion of overlapping. A portion of such an interference spectrogram is reproduced in Figure 7.

The interferometer itself is extremely simple; as stated above, it consists of two perfectly flat glass or quartz plates separated by a certain distance and adjusted accurately parallel. The adjacent faces of such a pair of interferometer plates are coated with thin films of silver or other metal to reflect 80–95 percent of the incident light. When such an interferometer is illuminated by monochromatic radiation and viewed from the other side, a system of perfectly circular interference fringes is seen in the transmitted light appearing to come from infinity. Bright areas result from constructive interference, and dark areas from destructive interference, of the successive reflections and transmissions of light waves. Of particular interest is the illumination at the center of each pattern; if the center exhibits maximum brightness it means that the number of light waves in the double distance (to and fro) between the interferometer plates is an integer, or whole number, because the successive components are all in phase to interfere constructively. If the center is dark it means that the number of waves in the double distance between the plates is an integer plus a fraction which is 1/2 for maximum darkness occurring when the crest of one wave coincides with the trough of another; that is, they are 1/2 wave length out of phase. In general, the fraction will have a value between zero and unity because successive fringes represent a change of one wave length in retardation between the plates. With highly homogeneous waves this fraction is determinable to one one-thousandth of a wave, and herein lies the unique advantage of measuring lengths with light waves; the scale division is ten thousand times finer than the lines ruled on a meter bar.

In terms of any particular light wave the double distance (to and fro) between two parallel interferometer plates is the product of the wave length and the number of waves, which, in general, as just stated, consists of an integer and a fraction. This number of waves in the double distance is called the *retardation*, or the *path difference*, or the *order of interference*. Assuming that the double distance between the interferometer plates is the same for all wave lengths, it can be expressed for each as the product of wave length and appropriate order of interference. The ratio of two wave lengths is, therefore, equal to the inverse ratio of

their orders of interference. Thus, the wave length of the green wave of mercury is determined from that of the red wave of cadmium by multiplying the latter by its order of interference and dividing this product by the order of interference of the former.

In principle, the orders of interference, whatever their value, may be determined by starting with the interferometer plates in contact and then counting fringes which flow out as the plates are separated. This was the procedure actually used by Michelson when he determined the number of cadmium waves in a meter, but to avoid the tedious labor and risk of blunder in counting 1,553,164 waves he counted only about 1,212 contained in 0.39 millimeter and then doubled this number eight times in succession to reach a decimeter, finally comparing this wave-measured decimeter with the meter by displacing the former ten times its own length.

Now, *by using the green and yellow lines of Hg^{198}, it is possible to determine orders of interference or measure lengths without counting any fringes at all.* The only requirements are measurements of a few interference-ring diameters, accurate values of the wave lengths, and an approximate value of the distance between the interferometer plates.

The fractional part of the order of interference is derived directly from measurements of ring diameters. It is seen (Figs. 4, 7) that the intervals between successive rings are not constant; the separations decrease with increasing ring number counting from the center. But theory and measurement agree that these rings are a quadratic function; that is, the squared values of successive ring diameters differ by a constant. Because the squared values of these ring diameters are a linear function of ring number, the fractional order at the center of the pattern is simply obtained by dividing the square of the first-ring diameter by the constant difference between squares. Obviously, the unit of length employed in measuring ring diameters is immaterial because the unit cancels out in the quotient.

Assuming that the fractional orders of interference have been thus measured for the green radiation 5,461 A and for the two yellow radiations 5,770 A and 5,791 A, the next step is to determine the whole number or integral order of interference for each of these radiations. Starting with a scaled or calipered approximation of the distance between the interferometer plates, double that distance is divided by 5,791 A to get the nearest whole number of waves. To this trial number is added the measured fractional order, and this sum is then multiplied by the wave length to give a double distance that includes the correct fraction but possibly an erroneous integer. If the trial number is correct, division of the double distance by the wave length 5,770 A will yield a corresponding whole number and fraction, and this computed fraction will agree with the measured fraction for this wave length. If these computed and measured fractions disagree, the divergence shows how large an error exists in the trial whole number, because the rates of change of wave numbers are inversely proportional to the wave lengths. This yellow pair of mercury radiations differs in wave length only 21 A, which is 1/275 of a wave. If the order of interference for 5,791 A is reduced by exactly one wave the order of interference for 5,770 A will be reduced by one plus 1/275 wave (because it is 1/275 shorter), and any given configuration of their interference patterns cannot recur except at intervals of 275 waves. Therefore, dividing the difference between the computed and measured fractions by 1/275 indicates at once how many waves to add (or subtract) from the first trial whole number. Then the corrected double distance is divided by the wave length 5,461 A of the green wave to check the correction and get its integral order of interference (without counting any fringes). In terms of green mercury waves the double distance is this integral order plus the

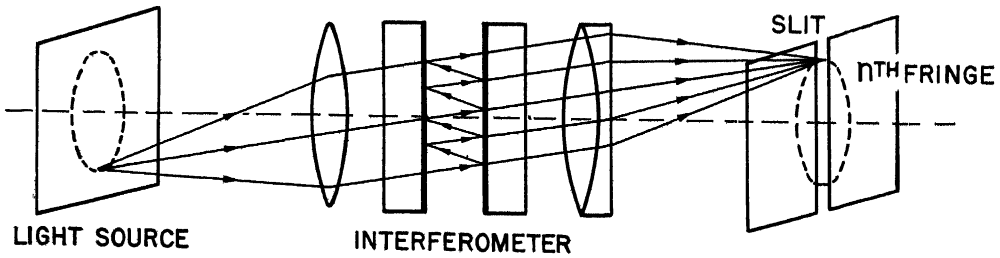

Fig. 3. Light from one point of an extended *light source* is traced through a Fabry-Perot *interferometer* to its image on a *slit*, or screen. This image will be either light or dark, according as the retardation of successive reflections and transmissions is an even or odd multiple of half wave lengths. The symmetry of this condition about the optical axis forms circular fringes.

measured fraction. If the interferometer plates are in optical contact with the end faces of a plane-parallel end gauge, the gauge length will be half of the observed order of interference. Indeed, this is the method actually used since World War I for the absolute measurement of all precision end gauges, "Johansson blocks," and the like, except that mercury lines could not be employed. Heretofore, such measurements with cadmium lines have been restricted to lengths of the order of four inches or ten centimeters, but with Hg^{198} waves it will be possible to measure directly more than ten inches or one-quarter meter.

The advantages of measuring lengths with three radiations of Hg^{198} may be summarized as follows: The yellow pair is happily heuristic for the order of interference since coincidences can recur only at intervals of 275 waves. Any distance known within half of this interval can then be measured to one one-thousandth of a green wave without counting any fringes. Coincidence rates and measured fractional orders lead uniquely to the correct integral orders. Theoretically, with Hg^{198} waves, orders of interference exceeding a million waves can be applied to length measurements. Since the integral order is determinable without error, and the fractional part can be measured to one one-thousandth, uncertainties in length measurements may be reduced to one part in a billion, or thousand million.

Certainly, for purposes of length measurement, the waves of mercury 198 are an improvement over natural mercury; they are also an improvement over waves characteristic of cadmium atoms. Now one may ask the question "Why is a wave length of mercury 198 the *ultimate standard of length?*"

The most nearly ideal wavelength standard must satisfy certain conditions as to monochromaticity, reproducibility, intensity, and convenience.

At low pressures and moderate electrical excitation the monochromaticity of atomic radiations varies as the square root of the atomic mass divided by the absolute temperature. The residual fuzziness is fully explained by the random motions of the radiating particles: those moving toward the detector appear to emit slightly shorter waves and those moving away appear to emit slightly longer waves. (This is an example of the well-known Doppler principle.) Naturally these atomic motions are least for heavy particles at low temperatures. Since mercury atoms are nearly twice as heavy as cadmium atoms, and radiate strongly at less than half the absolute temperature, mercury waves will be less than half as fuzzy as cadmium waves, other things being equal.

Cadmium consists of a mixture of eight isotopes, and most of its lines exhibit hyperfine structure. Although the isotopic structure of the red radiation from cadmium has never been resolved, it is very likely present. Natural mercury consists of seven isotopes, but this isotopic complexity has been removed in the manufacture of mercury 198

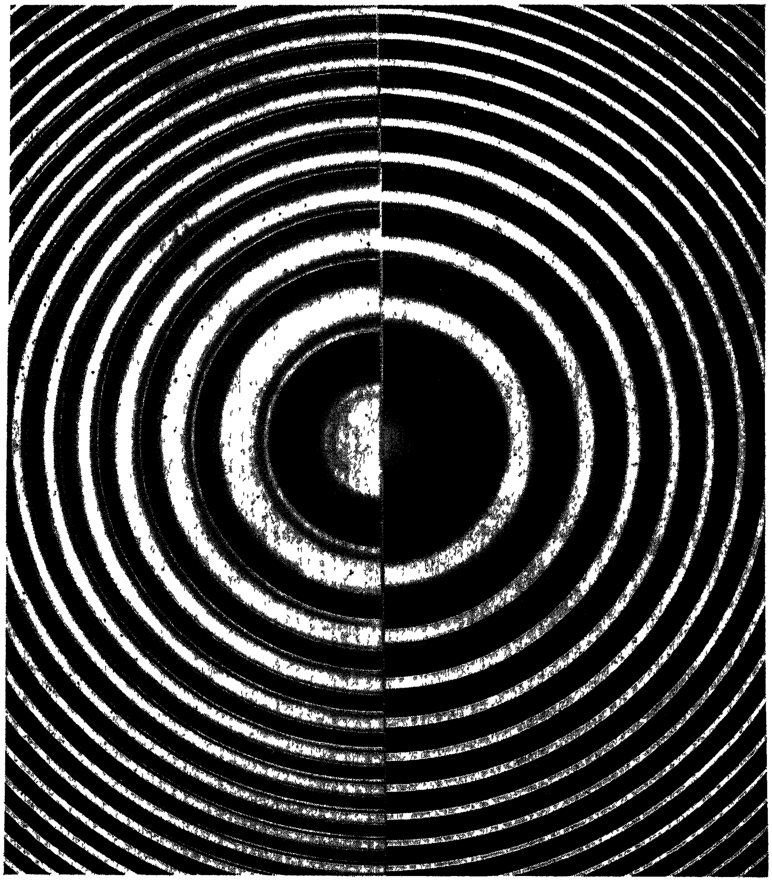

Fig. 4. Circular interference fringes formed by a Fabry-Perot interferometer illuminated with green light from lamps containing (*left*) seven isotopes of natural mercury and (*right*) a single isotope, mercury 198, made by transmuting gold. Faint eccentric fringes on the right are caused by reflections from the last, unsilvered, slightly inclined interferometer surface.

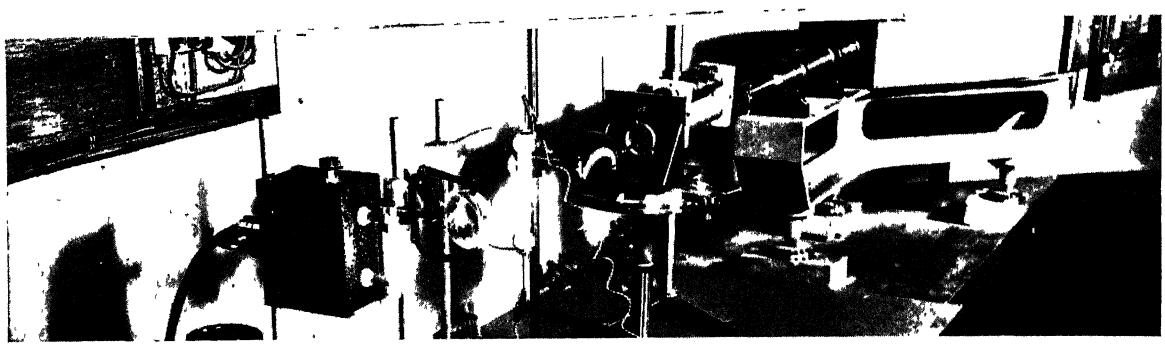

Fig. 5. An optical train for measuring wave lengths emitted by mercury 198 relative to the cadmium standard. The mercury lamp is imaged inside the cadmium lamp so that light from both sources passes simultaneously through a Fabry-Perot interferometer and into a prism spectrograph

from gold 197. Being a single isotope, the mercury 198 radiations cannot exhibit isotope shifts, and because the atomic nucleus has an even mass number and no detectable angular momentum there can be no hyperfine structure in its spectral lines.

Although cadmium and mercury are divalent chemical analogues, and therefore exhibit relatively simple and similar atomic spectra, whatever differences exist are almost invariably in favor of mercury. For example, the brightest line in the cadmium spectrum occurs in the blue-green (5,086 A), whereas the mercuric analogue is in the green (5,461 A), almost exactly coincident with the wave length for which the normal human eye is most sensitive. The red wave of cadmium (6,438 A) is intrinsically only one-tenth as energetic as the brightest line (5,086 A), and it is further handicapped by the fact that the eye is only one-seventh as sensitive for red as for green. Consequently, for the visual adjustment of interferometers the green line of mercury is seventy times as intense as the red line of cadmium. The mercuric analogue of the cadmium red line (6,438 A) is a yellow line (5,791 A) which is always accompanied by another yellow line of slightly shorter wave length (5,770 A) but nearly equal intensity. This yellow pair of mercury lines produces interference coincidences at intervals of 275 waves and therefore facilitates (without counting any fringes) the determination, from a first approximation, of the uniquely correct order of interference; it has no equally useful counterpart in the spectrum of cadmium or of any heavier element.

Mercury has the lowest freezing point (–39° C) of any metallic element, but even at that point it has a vapor pressure adequate for the excitation of its spectrum by high-frequency electric fields. At room temperature its vapor pressure is about one micron (of Hg), thus insuring the reproducibility of wave lengths emitted by mercury lamps at low or moderate temperatures because pressure displacements and all other effects associated with high vapor density will be inappreciable.

Cadmium melts at 321° C, and to insure enough vapor to produce a spectrum the Michelson lamp must be enclosed in a furnace kept at a temperature between 300° and 320° C. This high temperature adds fuzziness to the waves, and heat convection and radiation from the furnace may disturb the adjustment of the interferometer or change the wave length by altering the air density. Owing to the inconvenience of operating the standard cadmium lamp in a furnace, that source has rarely been used except for measuring the meter and for the determination of some secondary standards of wave length.

Mercury vapor is a pure monatomic gas which with moderate excitation emits a relatively simple spectrum characteristic of neutral atoms without any background of band spectra due to molecules or molecular compounds. The electronic structure and electron-binding forces are such that the spectral lines of mercury are distributed from ultraviolet to infrared. Two ultraviolet lines (1,850 A and 2,537 A) are the most intense; they produce ultraviolet burns (erythema) but are wholly ab-

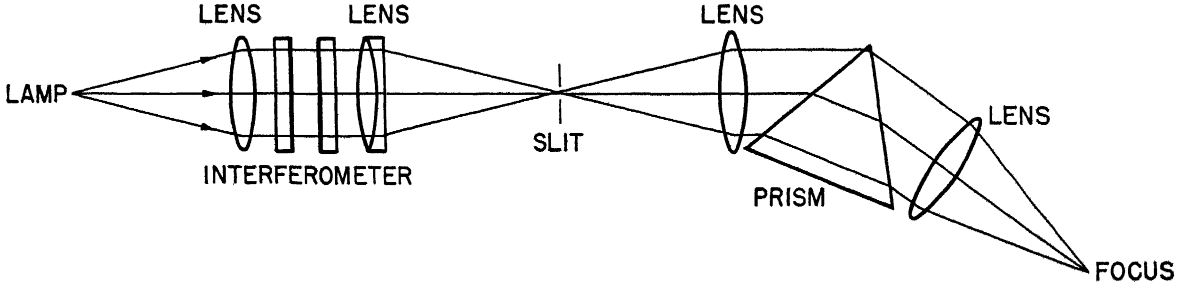

Fig. 6. Schematic diagram of optical train for measuring relative wave lengths, or for measuring lengths in terms of wave lengths. Twice the distance between the interferometer plates is obtained from measurements of the diameter of circular interference fringes. See Figure 7.

sorbed by ordinary glass. The third strongest line is the green one (5,461 A), to be recommended as the ultimate standard of length. With moderate excitation of an electrodeless lamp, interference patterns of this line can be photographed in a few seconds.

A small prism spectrograph suffices for the separation of yellow and green interference patterns when making length measurements, and if the green line alone is desired for counting fringes or for testing optical flats it can be isolated by using appropriate and convenient color filters. Thus the mercury waves of shorter length than green can all be absorbed by a solution of potassium dichromate or by a sheet of shade-yellow glass, and the two yellow lines can be removed by a solution of didymium nitrate or a sheet of didymium glass.

Mercury is the only heavy stable element that has an appreciable vapor pressure at low temperatures, and therefore it is unique among all elements in radiating, at low pressure and temperature, a relatively simple spectrum of intense and exceedingly sharp lines, provided that isotopic structure is eliminated. The green line of mercury, rejected by Michelson fifty-six years ago on account of complex structure, has finally been freed of its seven-isotope curse, and now stands alone as the most nearly ideal standard wave length that can ever be obtained from any atoms, natural or artificial. Coupled with the fact that adequate quantities of pure Hg^{198} can now be made by neutron bombardment of gold in chain-reacting piles, the unique properties of Hg^{198} force the conclusion that a progressive scientific world will soon adopt the wave length of green radiation (5,461 A) from Hg^{198} as the ultimate standard of length.

Since it is the width and character of the ruled lines themselves that limit the accuracy of meter–wavelength intercomparisons, there is hardly any point to measuring the wave length of Hg^{198} green light relative to the meter. This wave length can readily be measured relative to that of cadmium red light ten times more accurately than either can be measured relative to the meter. Adoption of the present provisional relation between the meter and cadmium red wave as exact, and subsequent substitution of Hg^{198} green for cadmium, appear to be the logical and expeditious approach to the ultimate standard of length.

In conclusion, I wish to emphasize that I am not trying to abolish the meter; on the contrary I am anxious to perpetuate it by giving it a scientific definition that will make it more accurately reproducible. The meter is here to stay because it

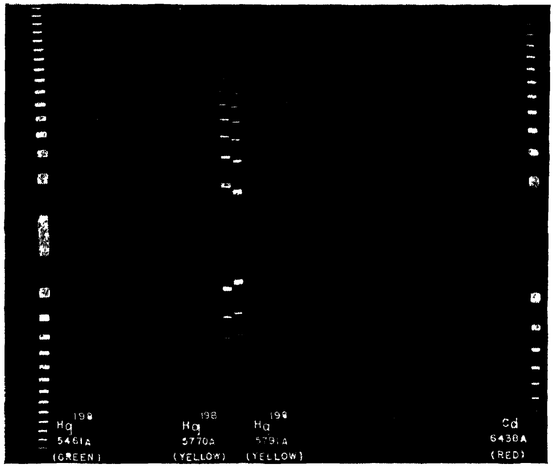

Fig. 7. Fabry-Perot patterns for the green and two yellow radiations of mercury 198 and the red radiation of cadmium, photographed simultaneously by means of a prism spectrograph.

is a very convenient and useful instrument for calibrating ruled scales with which most length measurements are made. It is highly arbitrary and unscientific, however, to define the primary standard of length as a distance between two relatively coarse, irregular lines on a metal-alloy bar, especially since there is no a priori reason for believing that such a material standard will be immutable for all time, or will forever survive all possible accidents and world catastrophes.

Everyone knows there has been a steady demand for increased accuracy of length measurement, both in science and in industry, but the present meter cannot satisfy all the requirements. The spectacular demonstration of interference methods of measuring with light waves by Michelson in 1892 was soon followed by the first set of metric gauge blocks produced by Johansson in 1896. These contributions set new standards of accuracy for the twentieth century, and the most precise measurements of lengths have been made, and will continue to be made, with light waves. It appears that the last possible improvements in this direction can now be made (1) by recognizing the wave length of green light from Hg^{198} as the *Ultimate Standard of Length,* and (2) by perpetuating a constant and more accurately reproducible meter through its definition in terms of the Ultimate Standard of Length *in vacuo.*

BIOLOGICAL PROBLEMS OF THE OCEAN

DANIEL MERRIMAN

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OCEANOGRAPHY in its modern sense—including as it does the physical, chemical, and biological study of ". . . the sea and all that is therein"—is a young science. If we are to date its birth (and this is of little more than academic interest), the three-and-a-half year, 69,000-mile expedition of H. M. S. *Challenger* three quarters of a century ago holds first claim. This cruise, resulting in widespread popular interest, a wealth of oceanographic data, and a magnificent series of publications, gave great impetus to research on the seas.

It is well to remember that although the chief motive of the *Challenger* expedition was biological—the question of life at the depths of the ocean—this was really the first time that scientists interested in the physics, chemistry, and biology of the sea worked together on problems of mutual interest. I spent a good share of last summer in England reading the original longhand, unpublished diaries and notes of some of the men on that expedition: Sir C. Wyville Thomson, Sir John Murray, J. J. Wild, J. Y. Buchanan, Dr. von Willemöes-Suhm and others. If there is any one thing these diaries show (apart from the breadth and capacity of the men themselves), it is the increasing understanding, as the voyage progressed, of the complete interdependence of the observations by chemists, physicists, and biologists in the solution of problems of the ocean. There is now, as then, the absolute necessity that the workers in different branches of oceanography should be in close touch so that they may profit from one another's advances.

A century ago, at the time of the founding of the AAAS, marine biologists were concerned mainly with the distribution of the known forms and the search for, and description of, new species. Some conception of working conditions one hundred years ago may be gleaned from an article in the *Athenaeum* (London) of July 5, 1873, which compares the equipment on the *Challenger* (then on the first year of her expedition) with that of the *Rattlesnake*, which was on surveying work in 1846. The *Rattlesnake* had on board as naturalist Mr. Macgillivray, and as surgeon no less distinguished a naturalist than Mr. Huxley. At Rio de Janeiro they determined to try their luck, and Mr. Macgillivray wrote:

> I had looked forward with eager anticipation to the result of the first dredging of the voyage. *None of the ship's boats could be spared*, so I hired one pulled by four negro slaves, who, although strong active fellows, had great objections to straining their backs at the oar when the dredge was down. *No sieve having been supplied*, we were obliged to sift the contents of the dredge through our hands,—a tedious and superficial mode of examination. Still some fine specimens of a curious flat sea-urchin (*Encope marginata*), and a few shells, encouraged us to persevere. Two days after, Mr. Huxley and myself set to work in Botafago Bay, provided with a *wire-gauze meat-cover*, and a curious machine for cleaning rice; these answered capitally as substitutes for sieves, and enabled us, by a thorough examination of the contents of the dredge, to detect about forty-five species of mollusca and radiata, some of which were new to science.

The writer in the *Athenaeum* for 1873 concludes his comparison of conditions on the *Rattlesnake* and the *Challenger* by the following somewhat acid remark: "The Lords of the Admiralty have, we are glad to know, learned since that time to render it unnecessary for naturalists sent out by them to borrow meat-covers and machines for cleaning rice."

I

The *Challenger* expedition stimulated so much interest and brought back so many new species that marine biologists in the last quarter of the nineteenth century concentrated their efforts chiefly on description. Before long, however, the rate at which new species were discovered began to decrease, and by the turn of the twentieth century attention was focused more on faunistic and life-history studies. About this time the dread of depletion in certain major fisheries loomed large, particularly in the North Sea, which is fished so

intensively by its bordering countries. Accordingly, in Europe there was formed the International Council for the Exploration of the Sea, and here in this country the United States Bureau of Fisheries produced a most distinguished record of research.

Johan Hjort, eminent Norwegian oceanographer, writing in 1907 on the question of the practical use of scientific fisheries investigations, compares agriculture with the fishing industry and sums up the ultimate goal of the fishery biologist of that time:

> . . . in the history of agriculture in every country there has existed a period when all progress consisted in breaking up fresh land, a time when there was far more unutilised soil than there were human beings to till it. This is the period of expansive cultivation. But we also know that, with increasing population, there came in most of the old civilized lands a time when the question was, how to get the fullest advantage out of the fields in use—how to obtain from high farming the most beneficial and economical results. So, too, we find in the fishing industry times when new fishing-grounds are discovered, while again another generation may be occupied with the problem of how to deal most profitably with the old grounds that are known so well [1]

There were many causes for optimism in this aim of rational utilization. As but one example, the tool of age determination by scales and otoliths was now available; it therefore appeared possible not only to trace the variations in the ages of a stock of fish as a particular fishery developed—a task which would be for the fishing industry what population statistics are for insurance—but also to solve the problem of at what age it is most profitable to catch the fish, both from the point of view of the return to the fisherman and the preservation of the stock for the future. Two fundamental questions were uppermost in the minds of the members of the fishing industry, and it was up to the biologist to answer them:

1. The causes of the great fluctuations in annual yield of many prominent fisheries
2. The age-old argument as to whether or not the supply of fish was falling off as a consequence of overfishing.

With regard to the first, fluctuations in the fisheries have from time immemorial been of enormous economic importance; in the Middle Ages, for instance, the appearance of herring schools was the deciding factor in the economy of whole provinces —towns sprang up or vanished with equal rapidity as schools of herring came or disappeared; in more recent times certain Scandinavian cod fisheries have ranged in yield between 3 and 23 million fish within the short space of three years.[2] To understand these fluctuations in abundance, it is necessary to know something of the reproductive habits of fishes[3]—quite apart from the causes of change in migratory pattern. In general, there is an inverse relationship between the number of eggs a species lays and the amount of parental care given to those eggs and the developing young—the greater the number of eggs the less the amount of care and vice versa. With fish that lay few or an intermediate number of eggs and give varying degrees of parental care, the mortality rates of the developing young tend to be fairly constant; hence we know in a general way that a certain number of spawners will produce a predictable number of young, and it is therefore possible to control the size of the population. Here it is obviously necessary to protect the adult mature stock, through closed seasons, length limits, etc., so that an adequate recruitment of young is assured. With fish which produce huge numbers of eggs, however, (and accordingly offer them no protection), frequently there are tremendous fluctuations from one year to another. Such fish produce from 50,000 to several million eggs per individual, and most of them scatter their floating eggs in the water to develop as best they may. Many of the great fisheries are based on species of this reproductive type—cod, haddock, mackerel, pilchard (or sardine), bluefish, striped bass (or rockfish). These fish show the phenomenon of dominant year classes; i.e., the production of so many fish in one year that this age group dominates the whole population for a series of years. The classic example of a dominant year class is the Norwegian herring, where the young born in 1904 were so numerous that they were a major element in the catch from 1907 to 1921 and virtually supported the fishery for fifteen years. It is clear, then, that in fisheries with large numbers of eggs the fluctuations in abundance are likely to be due more to the environment than to the size of the adult stock. In short, the number of spawners is not as important as the precise condition of the water in which the eggs and larvae undergo their development. To get good survival there apparently has to be an optimal combination of such factors as temperature, drift, salinity, and of the nutrients that ultimately are responsible for the production of floating or planktonic organisms, which in turn form the food of the newly hatched fish. As an example of the order of magnitude of survival, study of the mackerel[4] shows that the mortality in 1932 from the fertilized egg to a two-inch stage was 99.9996 percent; i.e., the survival was roughly 1–10 fish per million newly spawned eggs. Hence, a fluctuation in sur-

vival of several ten thousandths of a percent may be the difference between average production and the production of a dominant or weak year class.

More adults do not necessarily mean more young. Indeed, dominant year classes have a peculiar predilection for turning up when the adult stock is at its lowest level. In the case of many commercially important fish there is no conclusive evidence that an increase in stock will produce more young. We understand so little of the population mechanics of the stocks of fish which produce large numbers of eggs that we cannot predict with certainty what will happen if the catch is decreased and the number of spawners is increased. This is true so long as the stocks remain somewhere between an extremely high level (where competition between individuals results in great mortality and perhaps in predation on other desirable species) and an extremely low level (where there are an insufficient number of spawners). The size of the stock of most commercially fished species probably lies between these extremes. There is, therefore, no guarantee that more adults mean more young; this is so because the environment is so important in the development of the eggs. If one grants the foregoing reasoning, then one of the main jobs confronting the fishery biologist is to discover for each species the environmental conditions that produce good and bad survivals so that he is able to make predictions of economic value to the fishing industry.[5] Here the fullest exchange of information between the physical, chemical, and biological oceanographer is essential to the solution of the problem. As Fleming has pointed out, the lack of physical boundaries increases the complexity of such biological problems of the ocean.

II

The two questions that faced marine biologists in the early part of this century—causes of fluctuations and the overfishing problem—are still very much in the foreground. Partial answers have been given, but progress over nearly fifty years has been halting and confined to isolated cases. There is still much room for argument and disagreement. There are many reasons why this is so:

1. The fishery biologist is called into the picture only when a particular stock gives cause for alarm; i.e., when it has reached a low level. Seldom does he have the opportunity to study the virgin stock and then to measure the effect of man's intervention on the population.

2. Most of the studies have lacked the necessary continuity. Perhaps a biologist is asked to study a particular species which is at a low level through some natural fluctuation; several years later the population regains some of its stature, and another species gives cause for alarm, so the biologist is taken off the first (just when he's starting to grasp the problem) and moved to the second. Our man power has been used most inefficiently in this respect.

3. There is an acute shortage of trained personnel. The fact is that we know very little about the populations of living forms in the sea. The major feature of fisheries work—the fact which should above all determine the character of the methods and the theory—is that it is a population problem, aligned with the work of Lotka, Volterra, Verhulst, Pearl, Elton, and others on other populations.[6] For a full understanding of these problems, broad training through the graduate level is an essential. Furthermore, the necessity of field observation, the drudgery of the collection of the basic raw data, and the disadvantages of long-term studies in a time when competition for advancement is strong and when there are less arduous fields of research that offer sure and relatively swift publication—all these matters are not calculated to attract young men who have invested many years and much money in their own education.

I have devoted considerable space to biological problems of the ocean dealing with the fisheries for two reasons: first, as we have seen, because historically they deserve prominence; and, second, because they are of direct practical importance. Before turning to biological problems of the ocean which have less obvious practical application, let me mention one more matter dealing with the resources of the sea. In the last quarter of the nineteenth century the attention of biologists was drawn to the deep and open ocean; then, as we have seen, through the need for study of the coastal and bank fisheries, the attention of marine biologists became focused on the margins of the oceans. It is, in my own opinion, long since time that intensive biological study of the high seas by the United States was undertaken. Granted, we are taking steps in the proper direction, but we have a long way to go before we catch up with the prewar knowledge and ability of the Japanese in this regard.[7] Much has been said and written of late about the alarming rate of increase of the human population of the world and the scarcity of food.[8] In this connection the newspaper accounts of the AAAS Centennial symposia attributed a Cassandra-like quality to the remarks of a number of the

scientists gathered for the occasion. It seems inevitable, even taking into full account the possible advances of science, that the oceans will be called upon to produce more food. Clearly, if there is to be any substantial increase, it will have to come from the high seas; intensive research will be needed to produce it and to enable us to fish the available stocks at the optimal level.

III

What are other biological problems of the ocean? They are so many that to attempt to pick out more than a few main fields of endeavor would be ludicrous. Here let me follow some suggestions that come from discussion among British investigators, particularly H. W. Harvey and F. S. Russell of the Marine Biological Laboratory at Plymouth.[9]

1. Half a century ago and later, much attention was given to the description of new species; *individual* specimens were described in some detail. More recently, work on land animals has tended toward the study of *samples* of animals of a single species and their variations, with considerable advance in our understanding of evolutionary processes. It is high time that the study of trends in variation from one area to another was undertaken more intensively on marine animals. This involves the fields of systematics, genetics, geographical distribution, anatomy, etc. And in passing let me state that there is still crying need for good taxonomic work, descriptive anatomy, and embryology, to say nothing of the problems that still await the experimental embryologist who wishes to work on marine animals.

2. Turning to another branch of biology, the physiologist has for some decades conducted highly profitable researches on shallow-water marine forms. But thus far very little has been done on oceanic animals; their respiration, excretion, osmoregulation, coloration, luminescence, digestion, respiratory pigments, and a host of other matters deserve close scrutiny. The problem of the availability of deep-sea forms is by no means insurmountable at such a station as Bermuda.

3. For anyone interested in the study of behavior, the organisms of the ocean offer tremendous possibilities. There is still much to be done in the analysis of the causes of the vertical migration of planktonic animals in deep water; moreover, new tools are now available to make such analysis open to broader attack. As Russell has pointed out, we know very little about the swarming of animals in deep waters, or about their feeding habits. Some of the most fascinating problems are posed by the relationships between two or more species in the same community, whether they be planktonic, benthonic, or nektonic. Similarly, we have barely scratched the surface in understanding the interrelationships between the different animals in different communities.

4. In the study of the life histories of marine animals there is still an incredible amount to be done both in the shallow waters and more especially with oceanic forms The life histories of relatively few commercial fishes are adequately known, and the invertebrate bottom fauna badly needs study. Surely it is a scandalous state of affairs that with many of the open-sea pelagic game and commercial fishes practically no studies have been made on the age at sexual maturity—that we are literally unable to state at what age or length these fish first spawn, an essential to intelligent thinking on problems of rational utilization.

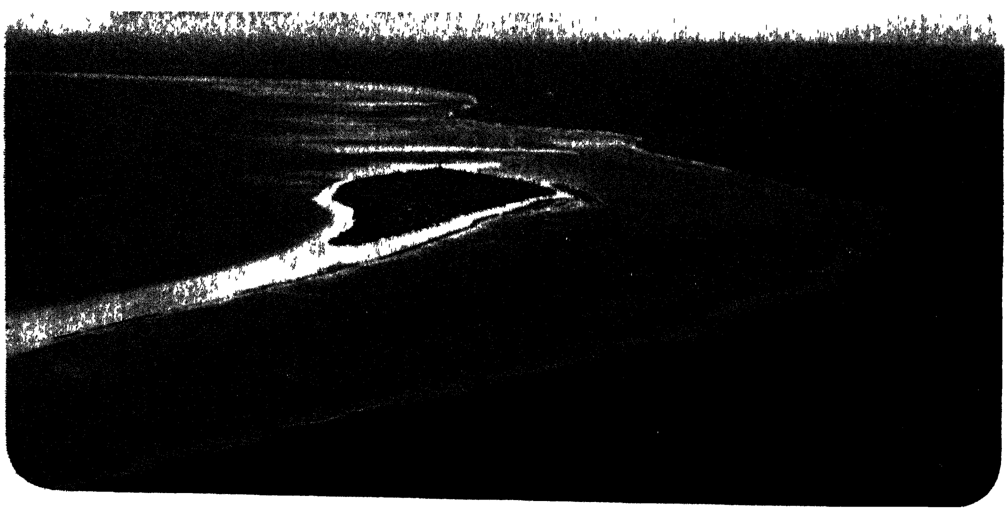
5. Finally, let me mention briefly some of the problems associated with studies of the fertility of the ocean. It was stressed in this symposium how physical conditions in any area of the sea fluctuate from one period to another and over long periods. Such changes affect the populations of the sea; there are fluctuations in the abundance of different species and in the general fertility. Sometimes, however, great changes in fertility occur that do not appear to coincide directly with any notable change in physical characteristics. Work in the English Channel over many years has shown close positive correlation between the phosphate maximum, the plankton population, and the number of young fish spawned and surviving during the summer; more phosphate or nutrient resulted in greater phytoplankton production, which in turn apparently supported a greater animal population. Harvey believes that there is a reasonable chance that water masses may in the future be labeled in terms of their potential fertility. But there is a long way to go in this direction. When we come to assess potential fertility we shall need to know much more than we do now about the balance between the plants and the grazing animals, and much more, too, about all the chemical factors affecting plant production. We need the answers to these and a host of other questions about the fertility of the seas.

To get some of these answers we shall need the closest exchange between workers in all branches of oceanography. We shall need to provide opportunity for the free and inquiring minds—the best in the field—to go where they will. Carefully planned research is essential, but I think we should bear in

mind that it can be dangerous to plan too much. No one can foresee what new discovery will change our outlook, or what practical result will come from investigation in pure science. Let us not fall into the trap of directed and applied research to the extent that our over-all advance toward the solution of the biological problems of the ocean might suffer.

REFERENCES

1. Some Results of the International Ocean Researches. Edinburgh: Mercat Press, 1908. Pp. 40 (Translation from *Særtryk of Aarsberetning Vedk. Norges Fiskerier*, 1907, 2 *Hefte*). Published by Scottish Oceanographical Laboratory.
2. *Ibid.*
3. MERRIMAN, D. The Conservation of Marine Fish with Special Reference to Striped Bass. *The Salt Water Sportsman*, 1948, 9, (4), 6–7, 14–15.
4. SETTE, O. E. Biology of the Atlantic Mackerel (*Scomber scombrus*) of North America. *U. S Fish and Wildlife Serv. Fishery Bull. 38*, 1943, 50, 149–237.
5. WALFORD, L. A. Correlation between Fluctuations in Abundance of the Pacific Sardine (*Sardinops caerulea*) and Salinity of the Sea Water. *J. Marine Res.*, 1946, 6, (1), 48–53.
6 KESTEVEN, G. L. *An Examination of Certain Aspects of the Methodology and Theory of Fisheries Biology.* Council for Scientific and Industrial Research, Division of Fisheries. Cronulla, N. S. W.: 1946. Pp. 159.
7. CHAPMAN, W. McL. The Wealth of the Ocean. *Sci. Mon.*, 1947, 64, (3), 192–7.
8 OSBORN, F. *Our Plundered Planet.* Boston: Little, Brown, 1948. Pp. 217; VOGT, W. *Road to Survival.* New York· Sloane Associates, 1948. Pp. 335.
9. *Discussion on Oceanography.* General Meeting, Linnaean Society of London, Mar. 21, 1946. London: Taylor & Francis. Pp. 16.



Airplane view of Bock Island, Marshall Islands, Ralik Chain, Rongerik Atoll. (Photo by Leonard P. Schultz, Smithsonian Institution.)

POPULATION AND FOOD SUPPLY: THE CURRENT SCARE

M. K. BENNETT

An expert in international trade in foodstuffs, prices, production, and standards of living, Dr. Bennett (Ph D, Stanford, 1927), executive director of the Food Research Institute at Stanford University since 1942, here sounds a rather different note than the one we have lately been hearing from other experts in similar fields.

THE so-called "race between population and food supply" has again come forward as an absorbing topic of discussion, in popular and professional circles alike. Discussants mostly present a deeply pessimistic view of the future. At a press conference on May 18, 1948, Sir John Boyd Orr, retiring director of the Food and Agriculture Organization of the United Nations, is reported to have "pleaded with the correspondents to help awaken the people to the fact that in the race between population and food, population was winning —'and we do not know how to stop it.' "[1] A few months earlier Sir John had said, "Taking account of the expected increase in world population, food production must be increased by 110 percent [more than doubled] in the next 25 years if sufficient food is to be provided for all mankind. Only cooperative international action can prevent the direst calamities."[2]

Reiterating in July 1948 Sir John's estimate of what a tremendous 25-year increase in production would be required to provide the world with "sufficient food," and defining sufficiency as a meager 2,600 calories per person per day (meager for the U. S., that is, but not for Oriental populations), President Milton S. Eisenhower, of Kansas State College, added, "And I say in all earnestness that it is an open question whether food production, for all our science, can be increased that much."[3] Reports in September 1948 from the Inter-American Conference on Renewable Natural Resources in Denver, and from meetings of both the American and the British Associations for the Advancement of Science were replete with similar pessimism. A Canadian publication stated in August,

> it would seem, then, that perhaps never again—unless an unforeseen miracle occurs—will the white people of the world be able to enjoy the extremely high level of food that was available to them in 1938 and 1939. The white races . . . will have to adapt themselves . . . to a gradual change in their diet, consuming less livestock products and more cereals.[4]

There is also William Vogt's recent book, *Road to Survival* (New York: Sloane Associates, 1948), described by its publishers as a "revelation of the fact that the earth, as abused by man, is unable to support the human race in terms of its most basic need—food." Fairfield Osborn's book *Our Plundered Planet* (Boston: Little, Brown, 1948, 75–76) chants much the same lament.

> The problem [he says] is how to conserve the remaining good natural soils that exist on the earth, together with the complementary resources of forests, water sources, and the myriads of beneficial forms of animal life. There is no other problem. If that is not solved the threat to human life will grow in intensity and the present conditions of starvation that are already apparent in various parts of the earth will seem as nothing in the years that lie ahead.

Examples need not be multiplied to indicate that we are in the midst of a resurgence of widespread discussion of the old problem of pressure of population upon the food supply. In the English-speaking world three waves of such discussion, heavily weighted with pessimism, have been recognizable since the first one was touched off by Malthus' famous *An Essay on the Principle of Population . . .*, published anonymously in 1798. The second wave came in the late 1890s in connection with the German controversy about the relative merits of agrarian and industrial national economies. Perhaps an ephemeral shortage and high price of wheat was a contributing factor. It was in 1898 that Sir William Crookes delivered his famous address, "The Wheat Problem," to the British Association for the Advancement of Science; some people took it as a "cosmic scare."[5] But again interest in the global food-supply problem waned, only to be stimulated for the third time, for a few years after World War I. We are now in the midst of the fourth wave.

I

What are the sources of this resurgence of interest, and what is new in current discussion?

It appears that many have recently become aware of an increase in births following the cessation of hostilities—at least here in the United

States, in some countries of Western Europe, and in Japan At the same time it has become clear that war casualties—deaths—were in many countries not so large as to slow down rates of population growth. Also, there has been in the past few years tremendous inventiveness in the field of lifesaving techniques, to mention only the use of the sulfa drugs, penicillin, Atabrine, and insecticides of the DDT type—all tending to reduce mortality rates and all tending to become cheaper and more widely available. Coupling these new developments with widening knowledge of the facts about world population growth during the past half century and more, many come to the conclusion that the population outlook for the next half century is for growth, very rapid and perhaps alarming because the earth's surface is limited in extent.

Certainly the available evidence on world population growth during the past two centuries is impressive. There were perhaps about 875 million people in the world in 1750. Now, as we near 1950, there are perhaps 2,350 million. The words "perhaps" and "about" are used advisedly, for the world's population has never actually been counted. But the estimates, imperfect as they are, seem sound enough to warrant the statements that the world now contains nearly three people to every one person existing two centuries ago, and that the increase, at least if one takes it by centuries since 1650, has been at an accelerating rate in the world as a whole though not in each of its parts.

If we take the present or 1950 world population as about 2,350 million, the probability is that the increase in the past 50 years has amounted to roughly 750 million people, and this in spite of two world wars in the interval. It is an increase of over 45 percent within the adult memories of a good many living men. Much of the world's population growth since 1900 has occurred in areas rather far out on the horizon of our vision, such as India, Burma, Japan, and the Netherlands Indies. Nevertheless, our own national population, helped by immigration, has approximately doubled since 1900, and there seems to be no large geographical area of the world where population increase has not been substantial in the past half century. The rates of natural increase, it is true, were lower in Western Europe, North America, and Australasia than in most other parts of the world. Indeed, not more than a decade ago these low rates of natural increase gave rise to concern about decline of certain national populations in the future. But recently that concern has diminished; the dates of probable maxima of populations have been postponed by the demographers.

One authoritative projection of population growth places the more or less probable world total in the year 2000 as about 3,300 million people.[6] An increase of some 900 million is implied for the half century which we are about to enter—in absolute numbers, a larger increase than occurred in the half century just past. Of course this is not a firm prediction, but it represents the type of projection in the minds of many who currently discuss the problem of population and food supply. They regard population trends as rather intractable and stubborn; and from all we know this seems a proper attitude to assume. Current discussion of the question seems generally to take for granted a great and perhaps unprecedented increase of world population in the next half century, barring only the advent of events highly unpalatable to mankind. Birth rates, it is supposed, cannot reasonably be expected to fall as rapidly as death rates, which are more and more reduced by modern life-preserving techniques—unless indeed the death rates should be elevated by war, famine, or pestilence, all of which men of good will seek to avoid.

Some discussants recognize that rates of natural increase vary from nation to nation, and that the lower rates, presaging little or no augmentation of certain national populations if we neglect immigration, tend to appear in prosperous or advanced countries. Those are countries at once highly urbanized, highly industrialized, highly literate, highly productive economically. Among them are the United States and Canada, Australia and New Zealand, the United Kingdom, Belgium, the Netherlands, Switzerland, the Scandinavian countries, France. Germany was of this group before the war, but may not in all respects be of it now. However, the population of the high-income countries of lowest rates of natural increase of population make up much less than half the world's population; so that an expectation of heavy increase in the world total population, in the absence of unwelcome types of checks, seems well justified.

The war, as everyone knows, tended in many though not all parts of the world to cut down agricultural production, and to curtail the international movement of food from surplus to deficit areas Post-hostility shortages of many sorts, currency inflations, balance-of-payment difficulties, shifts of national boundaries and population groups, occupation policies, political revolutions, civil wars, and other influences have so far combined to hamper recovery. Additionally, in the world's great

food-importing area of Western Europe, the weather was very bad for crops produced in 1945 and 1947. Only as the harvest of Northern Hemisphere crops of 1948 came to an end did the Western world begin to emerge from a food shortage of great severity. Here in the United States the shortage touched us in imagination and pocket, not in the stomach. But it has certainly struck the imagination hard. From all over the world for nearly a decade stories of the most urgent food needs, or of starvation itself, have poured into our consciousness. We have been made aware, to a degree hitherto unfamiliar, of the generally restricted diets of a billion and a half or more of the world's population even in prewar years. And population growth during the past decade of war and incomplete recovery has tended to accentuate the meagerness of food supplies in all but a few exceptionally situated food-exporting nations like our own. No wonder, on this count alone, that pessimism rules.

With growing population coupled with lagging recovery of food output and distribution, it appears difficult for the less fortunately situated nations to recoup the level of food intake prevailing before the war, in the late 1930s. But, in the minds of many, perhaps especially the nutrition-minded, such recovery is not enough, not what the world wants or needs. Rather, the desideratum is substantial improvement in the per capita food intake of most nations, perhaps of all—one aspect of "Freedom from Want." This is an idea for several years strongly espoused by the Food and Agriculture Organization of the United Nations. It has spent no little time and effort in publicizing the facts that even before the war, as measured against standards of adequacy set up by the nutritionists, the average per capita food intake of many nations, comprising in total far more than half the world's population, was sadly deficient in quality and not infrequently in quantity; and that even in the best-fed countries at least a modest fraction of the population was either undernourished or malnourished. Although evidence for these conclusions began to appear nearly two decades ago, it has lately been the FAO more than any other organized group that has broadcast information on the quantity and composition of national food supplies. And it has espoused as its foremost objective, its major *raison d'être,* the improvement of human nutrition throughout the world—not merely the recovery of prewar nutritional status.

If those who limit their hopes to a fairly prompt return of the world to its prewar nutritional status are nowadays pessimistic in outlook, we need not be surprised that those whose hearts are set on general improvement of nutritional status are even more so.

The jeremiads of the nutritional idealists, however, are hardly as frightening as those of the conservationists, notably the soil conservationists. Extremists of this group seem to take for granted the population projections of the demographers and the nutritional goals of FAO; and then they proceed to uncover, it would seem practically everywhere, evidence of permanent soil erosion by water and wind, and of depletion of soil fertility. Both are ascribed in dominant part to blind pursuit of profits by the farmers and graziers of the world, which is alleged to lead to mistreatment of the soil.

There seems to be a degree of historical coincidence in the rise of the soil-conservation school to its present degree of public prominence. Great dust storms afflicted our Great Plains in the middle 1930s. A bit earlier, in 1933, this nation had embarked upon the adventure of curing economic depression in part by supporting farm prices and incomes, and the method first chosen was to make Federal payments directly to farmers in exchange for acreage reduction. When the Supreme Court in January 1936 put a stop to this "gentle rain of checks," as a shrewd observer called it, a lawful way to continue became the object of political expediency. And so the payments earlier made to purchase acreage reduction became "agricultural conservation payments," politics opened the door of the public treasury to conservationists; the great droughts carried conviction to the public of a crying need for conservation; and increasingly since then political leaders, soil conservationists, and the public seem to have been at one in viewing soil erosion as a dreadful threat domestically. As conservationists proliferated, the perception of threat to food supply through soil erosion spread so as to compass whole continents. The books by Vogt and Osborn provide outstanding, perhaps extreme, examples of literature originating with the ardent-conservation school. That group, of course, cries for "action now," and for funds as well. Allusion is made to civilizations which, it is claimed, disappeared because their soil was swept away. It is sought to check or control erosion, thus saving civilization.

The soil-conservation school adds to the old concept that the food-producing land of the world is strictly limited in extent the new concept that the land is actually being destroyed, and at a rapid rate. The argument is picked up in strange places. An

advertisement of a British firm selling agricultural machinery reads in part: "To avert mass starvation and world strife, more food must be produced. Man in his ignorance, however, has raped vast areas of the good earth. Soil erosion, like a cancerous growth, has doomed miles of the land to sterility."[7]

The broad and general tenor of current discussion is summed up in a sentence of Sir William Crookes', spoken in 1898: "As mouths multiply, food resources dwindle."[8] A few points made in this fourth wave of pessimism, however, seem new. Concern has largely shifted, for example from "bread-eaters" of the Western world to the teeming populations of Asiatic countries. Again, interest has largely shifted from mere maintenance of accustomed diets to the improvement of diets. "Improved" diets usually involve enlargement of the proportion of calories derived from animal products; and this would involve extra drafts on productive resources because the animals burn up humanly edible grain when they convert it to meat, milk, or eggs. Sir John Orr may be thinking of such dietary improvement rather than maintenance, or even return to pre-World War II status, when he estimates that world food production must be increased by 110 percent in the next 25 years if sufficient food is to be provided for all mankind. New also in current discussion is the heavy emphasis on destruction of soil through erosion. And, finally, one sees little in earlier discussions the idea, often mentioned now, that there would be little purpose in fostering the international spread of new lifesaving techniques and sanitary measures, because resulting increased pressure of population on food supply would merely plunge into misery those whose lives were saved. The emergence of some of these new notes tends to intensify current pessimism. A vicious circle as perceived nowdays seems, if not altogether different from what it seemed before, to be more vicious than we used to think.

The consequences envisaged in current writings are various, some perhaps rather vague. Sir John Orr speaks of "direst calamities" in the absence of concerted international action, but is not specific about the nature of those calamities or about the international action. Eisenhower speaks of "the specter of permanent, world-wide hunger"[9] as now seeming very real. Our British advertiser speaks of the necessity of greater food production if "mass starvation" and "world strife" are to be averted. Vogt, Osborn, and Eisenhower alike make much of the disappearance of earlier civilizations, as in Mesopotamia, Syria, Rome, northwestern China, and Guatemala, and they suggest that modern civilization may be headed in the same direction. Deserts are said to be on the march, because men disturb and destroy natural vegetation. Less frightening though perhaps still unpalatable is the Searle Grain Company's opinion that the people of the world may never again enjoy as good a food supply as existed just before World War II, and that the white races in particular may have to cut down on ingestion of livestock products and resort more to grain.

II

It seems pertinent next to review sketchily what has happened to food supplies or intake over the world in the past 50 years or so when population increased by perhaps 750 million people. The recent past has some bearing on what may conceivably happen in the next 50 years when, the demographers suggest, a further increase of some 900 million may reasonably be expected in the absence of intolerable "positive" checks to population growth.

The question posed with respect to the past half century has to be stated with more precision. Not much is to be learned by asking what has happened to the average per capita calorie intake of the whole world, or what has happened to the composition of the diet of the world as a whole. We simply do not know, and cannot expect to find out. But there is no point in raising such a question. The world is nothing like a unit with reference to food intake,[10] any more than it is with reference to per capita real income, and a per capita world average is next door to meaningless. Much more useful to raise are the questions: Where throughout the world has food intake become more plentiful and more varied in composition in the past half century? Where less so? Where unchanged? And why?

Simply because the only available information pertains to nations or colonies, the changes must be located mainly in terms of national boundaries —and even this only approximately because national boundaries themselves have changed.

A degree of precision will be added if we mainly consider two aspects of average national diets: (*a*) the average per capita intake of total food calories from whatever crude foodstuff they may be derived; and (*b*) the proportion of national food intake derived from the grains and potatoes (including here such items as cassava and taro). It may be assumed that a long-persisting decline in

the "cereal-potato" fraction of a national diet represents an improvement of diet unless there is evidence of accompanying exceptional decline in total calorie intake per capita. For the cereals and potatoes are almost universally the cheapest of all foods per thousand calories; and all population groups except the very wealthy clearly tend to reduce the proportion of total calories derived from cereals and potatoes whenever they can do so, expanding variously the proportions of their food-calorie intake derived from other principal and more expensive groups of foods: namely, sugars, vegetable oils, legumes or pulses, low-calorie fruits and vegetables, and animal products, including meat, dairy products, eggs and poultry, fish, and their accompanying fats. A decline in the proportion of calories derived from cereals and potatoes in a national diet does not constitute altogether conclusive evidence of nutritional improvement of diet; it may not, especially if only sugar or vegetable oil replaces the cereals and potatoes. But in general a decline of the cereal-potato fraction of a diet means increase of palatability and diversity and represents economic improvement of diet. On all the evidence we have, the mass behavior of people is to reduce the cereal-potato component of diets when that component is high, if they can afford it. Such evidence comes forward in all surveys of food consumption by differing income groups, in whatever places and at whatever dates the surveys are made. But the tendency does not run to complete displacement; perhaps 10–20 percent of cereal-potato foods would be retained by a mass of millionaires.

At the present time it happens to be impossible to ascertain, systematically, nation by nation for the whole world, exactly what changes have occurred in per capita calorie intake or in the proportion of calories derived respectively from cereals and potatoes and from other foods. If estimates of population growth over the past half century are commonly lacking or uncertain, the position is far worse with respect to food supply. We know most about the nations where the record keeping is best; where domestic crops have long been painstakingly estimated, and imports and exports systematically recorded. The measurement of a national food supply is fearfully complex to accomplish, not only because there are so many components in a food supply, but also because so many important foodstuffs are usable and are used both for food and for feed.

More is perhaps known about the United States than any other country. Elaborate official statistics and estimates cover, year by year, the period since 1909. They show, in the 30 years between 1909 and 1939, a small decline in total calories per capita "available . . . at the retail level" from about 3,500 to about 3,300. They show also a much more striking decline in the proportion of total calories derived from grains and potatoes—from 44 to 29 percent. On the other hand, there were measurable increases in the proportions derived from sugar (from 12 to 16 percent), from fruits and the vegetables other than potatoes (from 7 to 10 percent), and from meat, fish, poultry, eggs, dairy products, and fats and oils (from 37 to 45 percent). Thus the proportion of national calories derived from grains and potatoes fell, and the proportion derived from other foodstuffs, more expensive per thousand calories as a group and with respect to nearly all individual items, reciprocally rose—at least from 1909 to 1939.[11]

If one now seeks to push backward in time through the 20 years preceding 1909, the official statisticians give less help. But Holbrook Working[12] was able to establish convincing evidence of decline in the per capita consumption of the chief grain products, wheat flour and corn meal, and also an increase in per capita consumption of sugar, during those two decades. Flour and meal consumption fell from about 560,000 calories per person per year in 1889 to about 450,000 in 1909, a decline of nearly 20 percent. The rise in sugar consumption did not offset this. Working's incomplete findings are consistent with the interpretation that general trends known to to exist from 1909 to 1939 existed also from 1889 to 1909—a decline in total food calories per capita, a decline in the proportion derived from grains and potatoes, and an increase in the proportion derived from other foodstuffs than grains and potatoes, demonstrably from sugar.

These, then, were the tendencies in the United States for half a century preceding World War II. They represented a general sufficiency of total calories even with decline; that decline is reasonably attributable mainly to lowered requirement due to reduction of physical activity—machines increasingly working for men. They also represented wider freedom of choice in foods, greater variety and palatability—in short, an improvement of diet in the economic sense, whether or not in the nutritional sense. This could not have happened without increase in per capita real income, of which there is plenty of evidence that need not be brought forward.

But the United States is only a part of the

world. What happened elsewhere? Of this we know less. It is a proper subject for prolonged and intricate research. Yet a good many pieces of evidence can be tied together to indicate that decline in the cereal-potato fraction of national diets was rather widespread in the half century before World War II. Quite commonly we perceive in statistics declines in per capita human consumption of what are generally regarded as inferior cereals—rye, corn, barley, oats, and the millets and sorghums. In itself this indicates the existence of economic circumstances permitting wider choice of more expensive and desirable foodstuffs, among which the preferred cereals, wheat and rice, commonly appear. Quite commonly we perceive increases in per capita consumption of sugar. Not uncommonly we perceive declines in per capita consumption of a preferred cereal, wheat; but this reflects a reciprocal rise in consumption of more expensive foods. Trends toward economic improvement of national diets can be demonstrated satisfactorily at least for Canada, Australasia, the British Isles, Scandinavia, Germany, the Low Countries, Switzerland, France, *and Japan*—in kind resembling the trends in the United States, in degree and in details quite likely different. One need not much hesitate to add to this list such nations as Argentina, Brazil, Mexico, Cuba, the former Baltic States, Poland, Czechoslovakia, Austria, Hungary, Yugoslavia, Greece, Italy, Spain, and Turkey. In each of these, there appears to be no suggestion of a long-term trend toward insufficiency of total calories per capita, even if there was slight decline as in the United States and for similar reasons; and there is some evidence of increasing diversity of diet.

Again looking at the half century preceding World War II, we see, though most dimly, developments which have the appearance of being somewhat different in several Oriental countries—India, Burma, Korea, Formosa, Java, the Philippines. The meager statistics for recent decades—one cannot press far back—suggest a gradual small reduction of per capita calorie supply, coupled with tendencies for inferior cereals and for sweet potatoes to increase while the favored grain, rice, declines in consumption.[13] In spite of some small increase of wheat consumption, occasionally of sugar, and in spite of lessened incidence of local famines, one finds little direct statistical evidence of increasing per capita consumption of fats, meat, dairy products, and fish. The dependence on cereal-potato foodstuffs remains very high; if it declined, the decline was small. There is a suggestion here, though only a suggestion, of general tightening of the food situation—fewer calories where more were needed—rather than enlarged scope to diversify diet.

The largest population groups not yet mentioned are China, the African continent, and the USSR. On food developments in China and Africa, little useful statistical evidence is to be found. That northern China suffered sporadic localized famines in the course of the half century is certain. Whether there was improvement or deterioration generally in calorie supply or in composition of the national average diet is unknown. There is perhaps evidence of improvement in African food supplies, but no conclusions need be drawn here.

In the USSR, from 1898 to World War I, there seems to have been general sufficiency of calories aside from local shortages, also a shift from rye to wheat, and possibly a degree of increased diversification of dietary composition. The first world war and the Revolution led to famine in 1921–23. Subsequent recovery was interrupted by the drive for collectivization and ensuing slaughter of livestock, and another famine occurred in 1932–33. Again came recovery, but Soviet statistics are not such as to tell us truly about calorie supplies and composition of diet just before World War II.[14] There is ample reason, however, to believe that the rural fraction of the population was then less well fed than it had been before World War I; and the rural fraction remained, in spite of growing industrialization, much the larger fraction of the total. Also, the urban population itself seems unlikely to have been as well fed before World War II as it was before World War I.

So much for the history of the relationship between population and food supply during the half century preceding World War II. The world population growth was enormous. But meanwhile food intake improved in a good many national populations. This, it should be emphasized, is the fact so far as concerns the regions about which we know most and can have most confidence in the statistical evidence. As for the populations where deterioration of the food position is suggested, either the credible direct evidence on food supplies is decidedly meager, or we can be fairly certain that the deterioration sprang in large part from governmental interventions, as in Korea, Formosa, and the USSR.

Even with respect to India, one cannot feel confident that the food position really deteriorated progressively, although the statistics—such as they are—point in this direction. For we know that at the same time India unquestionably became

increasingly urbanized and industrialized. Except as this comes about through a socialized control—investment squeezed from consumption—such as prevailed after 1917 in the USSR but never in India, urbanization and industrialization spell enlargement of national real income per capita; and that tends strongly to carry with it improvement of diet in the economic sense. It is difficult to believe, of India or any other nonsocialized country, that city populations grew as they did in relation to rural populations for any other important reason than that the farm people saw a chance to improve their income status in the cities, and that the process of industrialization opened avenues for them. The people who migrated from farm to city were not jumping from frying pan to fire. On this line of reasoning, one cannot feel fully confident that India, and perhaps, though less probably, even China, were not enjoying a somewhat more diversified average national diet, albeit a poor one by our standards, in 1939 than 50 years before. The facts are not clear, whereas the facts *are* clear concerning the great majority of nations belonging to the commercial Western world.

The half century preceding World War II may then reasonably be regarded as one more accurately characterized as giving evidence of improvement in per capita food supplies than the opposite. Widespread indeed were demonstrable improvements, whereas positive deterioration suggested in some places is subject to doubts not readily dispelled, or can be explained in terms of political interference with economic development. That this is the picture that emerges is the more remarkable because a destructive world war occurred in the interval, to say nothing of minor yet devastating localized wars; and because a rather general economic paralysis beset the world toward the end of the 50-year period. The forces that make for higher levels of living, with accompanying dietary improvement, must have been tremendously powerful.

III

It would be possible to review in a general way what happened in the 50 years preceding 1939 to improve national food situations in so many parts of the world. One would mention, as did Joseph S. Davis in his explanation of history's answer to Sir William Crookes,[15] not only expansion of acreage but increase of yield per acre, through improved breeds and varieties, better rotations, the spread and cheapening of transportation, the invention of laborsaving machinery, and so on. It may be added as factual material that between 1888 and 1938, even in long-settled areas of the world excluding China and Russia, it proved possible for the yield per acre of wheat to increase fully 25 percent while acreage increased some 10 percent; and in the new areas of the world, although expansion of acreage supposedly to poorer and drier land was no less than sixfold—more than 100 million acres—the yield per acre nevertheless did not decline if one makes allowance for the unusual character of the droughts of the middle 1930s in the Great Plains.[16]

But this approach is perhaps no more illuminating than to ask the question, What is it that was impeding before 1939, is now impeding, and may well continue to impede the growth of economic productivity in general and of agricultural production specifically? What, in particular, tends to prevent more food and more varied food from being produced and put into the hands of the multitudes who need or desire it?

Those impediments are overwhelmingly numerous if looked at one by one. Economists long since invented, however, a set of rubrics convenient for the discussion but including only four terms. They would say that agricultural productivity, with reference to an unchanging supply of land, must depend upon the application of the three other factors of production—labor, capital, and management. The rubrics—land, labor, capital, and management—are convenient, if only because bearing them all in mind precludes our focusing attention upon only one or the other of them, as I suspect some soil conservationists do.

About any acre of agricultural land now in use in the world, theory and history tell us (1) that it has a *maximum* theoretical (economic) productivity, given the state of technology of the moment; (2) that its maximum theoretical productivity may or may not actually be achieved at the moment, depending upon whether the optimum combination of labor, capital, and management is being applied; and (3) that in all probability, as judged by history, the point of maximum productivity will be shifted upward as time passes because technological advances will be made, even if we cannot predict either the degree or the pace of advance. But nothing in history or theory tells us what the output may be, in volume or value, at the point of maximum productivity.

The most impressive probability about the acres of the world now devoted to agricultural use is that a truly enormous gap exists between

actual productivity and maximum productivity under optimum application of labor, capital, and management.[17] This refers to maximum economic productivity, not maximum physical productivity, which no doubt is the greater. In all probability a very large fraction of the managers of the world's agricultural land are inhibited from applying optimum doses of labor, capital, and management. Either they are not aware of the best in current technology, or they are helpless to make use of it, or they are unwilling even if able. The mere existence of cover crops and green manures, commercial fertilizers, effective sprays against weeds whether new or old, hybrid corn, rust-resistant wheats, inoculation against hog cholera, tests for animal tuberculosis, is literally unknown to thousands of land managers in the very areas of the world where population is alleged to be most obviously outrunning food supply. In those same areas and others are land managers who know of an improvement, but have no incentive to put it to use. Somewhere a large landholder, wealthy enough and socially important enough to satisfy all his ambitions, may remain content with farming methods reminiscent of the Middle Ages. Perhaps one could find instances in Spain or Chile. Elsewhere a lack of security of life and property—in essence, a lack of orderly government—may inhibit the application of improvements widely known. Bad though orderly government, exercising unwisely its power of taxation, may inhibit improvements in farming, simply because the fruit of effort is not permitted to accrue to the men who make it. The Soviet Union is a case in point. Above all, perhaps, a great many land managers of the world cannot apply what they both know and are willing to try for the reason that they lack capital to cover the initial cost. The purchase of a pound of improved seed, to say nothing of a good spade or plow, may unfortunately be quite beyond the capacity of a great many who cultivate the world's soil.

For these and other reasons, the gap between agricultural potential and agricultural practice is unquestionably very wide, and has been very wide at any given point of time for far more than half a century. One of the other reasons, a most compelling one, is lack of demand or of the development of demand. There has to be an exchange of products between those who manage the land and those with whom they exchange produce; and the last must have something to exchange. Agricultural productivity not only responds to advances in economic productivity on other fronts, but stimulates it as well. The process is complex in the extreme, involving of necessity appropriate fluctuations in the relative prices of the factors of production; but its existence ought not to be overlooked merely because it is difficult to describe and trace. The invention and use of a pound of higher-yielding seed, of a longer-lived and sharper hoe, or of a way to twist rope at lesser cost, work reciprocally to enhance both agricultural and industrial productivity. Multiplied in thousands of instances, such innovations exemplify economic development. More elaborate division of labor, a most excellent way of increasing economic productivity, is an accompanying phenomenon. The race is less accurately between population and food supply, than between population and economic development in every sphere. And even here the analogy is imperfect, for the racers are hobbled together: economic development at some stage becomes associated with a slowing down of the rate of population growth.

Much that can be said about the gap between agricultural practice and agricultural potential may equally be said about a gap between food harvested and food eaten. Losses in storage and transportation are truly impressive, and are unquestionably subject to reduction as the effectiveness of inhibiting influences is removed or lessened.

So far the emphasis has been given to factors that impede the increase of food yield per acre on land under cultivation now. It remains briefly to speak about land itself, the lack and the destruction of which are currently so heavily emphasized as impediments to improvement of per capita food supplies. Almost axiomatic is the fact that there is now less naturally good land for men to bring into use than ever there was before in historical time. Far from true would be a statement that there remains no unused land whatever subject to development for food crops. There are still areas of the world where lack of transportation hampers settlement, and where the presence of the tsetse fly or the malarial mosquito acts similarly. No one could reasonably suppose that the tropics are now fully developed, with reference especially to oil-bearing palms. Also the limit of land cultivation—at best a vague concept—is elastic, not fixed. It changes, for example, whenever a shorter-maturing variety of grain is invented, moving the limit both in latitude and in altitude—and even, perhaps, permitting double cropping of land once single-cropped. If the behavior of demand is such as to call all these reservoirs of food production into activity, their

product will be huge indeed. Economic planners in Brazil estimate that Brazil has 150 million acres fit for growing wheat. Those acres may never or may not soon be settled, but the estimate itself is indicative of ideas held by some people that by no means all the productive land of the world is as yet under the plow. And land is not all. One cannot dismiss as inconceivable the profitable use of the plankton of the oceans as food or feed; and ways and means of utilizing solar radiation to provide the energy for artificial synthesis of the food elements remain to be speculated about.[18]

Some writers, such as Osborn and Vogt, who have espoused the cause of soil conservation, paint a picture of rapid contemporary destruction of soil resources through erosion by water and wind. The question here is not whether there is any erosion at all, for there is, but how important it may be; and this is a difficult question. If a man alleges, as Vogt does, that in this country we have lost a third of our topsoil in 150 years and that the future of our country is still within our control but will not be after only a few more decades of such abuse as we have subjected it to, no easy counterassertion comes to mind. It is comforting then to read the words of an experienced soil scientist, Charles E. Kellogg, chief of the Division of Soil survey in this country (*Sci. Mon.*, 1948, **66**, 478, 479): ". . . a large part of the arable soils of the world are made better by good farming than they were naturally." And to have him say, "For a time, it almost seemed that each popular writer was trying to outdo the others in dramatic statement. Some even went so far as to assert that erosion had swallowed whole civilizations. . . . It is sincerely to be hoped that the period of extreme statement on soil erosion is nearly over. . . ."

One can hardly fail to be impressed not only by the tendency of the extremists to engage in what may be called spurious quantitative thinking, but also by their failure to make clear precisely where all the damage they see has been done, or to differentiate between geological and man-made erosion, or to stress what has happened on the great agricultural flatlands of the earth equally with what they say has occurred on the much less important sloping uplands, which are singled out as manifesting the soil-destroying evils of so-called "fire farming" or "shifting cultivation." On reading the extremists, one would never guess that the Missouri River was carrying silt—was called the "Big Muddy"—when men who knew the plow or herded domestic animals first saw it; or that beavers had ever made dams which silted up and formed mountain meadows long before the mountains above were grazed; or that dust storms had occurred in the Great Plains in the 1890s, before they felt the plow.

One who has no claim to expertness in this field may neverthless find it impossible either to deny the existence of man-made soil erosion and the desirability of a degree of social action against it, or to become greatly exercised about it when he thinks of the impressive array of quite different factors which inhibit agricultural productivity.

IV

What need be said in conclusion? Certainly it seems futile to engage in unqualified prediction in so inexact a field of inquiry, and it would be tiresome to state all the qualifications necessary in prediction. We must concede, perhaps, that the "direst calamities" lie in the realm of possibilities, though in appraising even possibilities it would be helpful to know more precisely what those calamities are thought to be. If one of the "direst calamities" is eventual reduction of the per capita ingestion of animal products among the white races, we must concede its possibility; and so with "mass starvation" if by that is meant what has long occurred locally and among relatively few people at any point in time. To concede possibility in these matters, however, is not to argue probability.

We need not concede the possibility within the foreseeable future of the emergence of "permanent world-wide hunger," if that phrase means everybody hungry at once. That cannot happen unless a world society develops and finds a way to achieve absolute equality of personal income, a possibility surely so remote as hardly to be called a possibility at all. In such a society the tremendous contents of the feedbin of the world could be utilized to assuage human hunger; humanly edible grain now lost to man in the conversion processes of animals could then be captured for human consumption.

We need not concede that possible localized national deterioration of diets of historical type leads at all certainly to world strife. Hungry nations are commonly economically weak and weak in military potential as nowadays measured in other terms than clubs or knives, and hence may be chary of assuming the risk of aggression; and in any event wars have other causes than scarcity of food.

We might not be wise to concede, on the other hand, the possibility of an increase of 110 percent in world food supplies within 25 years. That is a decidedly short time for so tremendous an increase.

And, yet, at the same time that we concede the possibilities both of deterioration of food supply and of the unlikelihood of hoped-for extreme improvement, we need not exclude the possibility of a geographically spreading gradual improvement of national diets, doubtless unequal in pace among the several nations, resulting in generally firmer assurance of food supplies increasingly consistent with human aspirations. It is well to remember that the recent years of food shortage may, if a long stretch of peace lies before us, seem in retrospect merely transitory.

Pessimism about maintenance or improvement of per capita food supply, even where population is densest, is not intellectually necessary, not compelled on the basis of historical fact or logic. We know approximately what the trends have been More important, enough is understood about the powerful forces lying behind economic productivity and dietary improvement to warrant the statement that the ultimate productivity of the earth's surface had best be regarded as incommensurable and elastic, not fixed. It can be measured, and thereafter set in relation to a future growth of population itself not firmly predictable, only in terms of surface—square miles or square feet. The customary but spurious quantitative exercise—only 4 billion cultivable acres now, less than 2 acres per person, with 2.5 acres estimated to be required per person to provide a minimum adequate diet, and with population growing while soil is washed away—ought not to mislead. The possible product of the earth defies measurement even on the assumption of stagnant technology, and the assumption itself gains no support from history.

Time may prove today's pessimists to have been right in their attitude; the actual outcome remains to be seen. But if they prove right, the historical reasons are quite unlikely to be limited to an over-rapid increase of population set against an over-rapid destruction of soil resources. The future historian will need to inquire whether some other contributory causes were important—whether men and governments acted wisely in such directions as engaging in wars, keeping civil order, spending appropriately on education and research, taxing with propriety, intervening intelligently in economic life, nursing economic isolation, or freeing the channels of trade. To follow the wrong paths is to hamper invention, stifle capital accumulation, hinder investment domestically and internationally, and hence to retard the general economic development, one aspect of which is improvement of national diets. If the right paths are followed in these directions, and not only in human reproduction and in land use, time may prove today's pessimists to have been wrong, as with the pessimists of yesterday.

REFERENCES

1. *N. Y. Times,* May 18, 1948, 14.
2. FAO: The Year Past, the Year Ahead. *U. N. Bull.,* Jan. 1, 1948, 19.
3. Eisenhower, M. S. Man, Soil and Civilization. *Northwestern Miller,* July 6, 1948, 24.
4. *Grain Market Features.* Searle Grain Co., Ltd. Winnipeg. Aug. 25, 1948, 2.
5. Crookes, Sir William. *The Wheat Problem.* (3rd ed.) London: Longmans, Green, 1917, xii.
6. Notestein, F. W. Population—The Long View. In T W. Schultz (Ed), *Food for the World.* Chicago: Univ. of Chicago Press, 1945, 57.
7. *Economist* (London), Aug. 21, 1948, 307.
8. Crookes, Sir William. *The Wheat Problem.* (3rd ed.) London: Longmans, Green, 1917, 3.
9 Eisenhower, M. S. Man, Soil and Civilization. *Northwestern Miller,* July 6, 1948, 24.
10. Bennett, M. K. International Contrasts in Food Consumption. *Geog. Rev.,* 1941, 31, 365–76.
11. Clark, F., Friend, B., and Burk, M. C. *Nutritive Value of the Per Capita Food Supply, 1909–45.* USDA Misc Pub 616, Jan. 1947, 1, 18, 32
12 Working, H. The Decline in Per Capita Consumption of Flour in the United States. *Wheat Studies of the Food Research Institute,* 1926, 2, 283.
13. Wickizer, V. D., and Bennett, M. K. *The Rice Economy of Monsoon Asia.* Food Res. Inst. Grain Economics Ser. 3, Stanford Univ., 1941, Chaps. IX, X.
14. Jasny, N. *The Socialized Agriculture of the USSR: Plans and Performance.* Food Res. Inst. Grain Economics Ser. 4, Stanford Univ., in press.
15 Davis, J. S. The Specter of Dearth of Food: History's Answer to Sir William Crookes. In *On Agricultural Policy,* 1926–1938. Food Res. Inst. Misc. Pub. 9, Stanford Univ., 1939, 19–22.
16. Bennett, M. K. Trends of Yield in Major Wheat Regions since 1885. Part II. Irregular, Stable, and Declining Trends. *Wheat Studies,* 1938, 14, 259.
17. Johnson, S. E. *Changes in Farming in War and Peace.* USDA Bur. Agr. Econ. FM 58, June 1946; Black, A. G. Iranian Agriculture—Present and Prospective. *J Farm Econ.,* 1948, 30, 422–42.
18. Alsberg, C. L Progress in Chemistry and the Theory of Population *Ind. & Eng. Chem.,* 1924, 16, 524 26.

SOME ADVANCES IN CHEMOTHERAPY

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THE dramatic advances in the treatment of bacterial infections with the sulfa drugs, penicillin, and streptomycin do not, unfortunately, extend to the filtrable viruses. The viruses causing diseases such as the common cold, influenza, poliomyelitis, encephalitis, and yellow fever are not affected by any of the new drugs or antibiotics. There are, however, certain minute intracellular parasites that resemble both bacteria and filtrable viruses, and infections caused by them are accessible to chemotherapy. In order to define the problem more clearly let us first briefly summarize our present knowledge of the nature of viruses.

The filtrable viruses differ from bacteria in several properties. The size of the smallest bacteria is in the range 125–250 millimicrons. The largest viruses and the related rickettsiae are about the same size or slightly larger than the smallest bacteria and, like them, can be seen under the ordinary microscope. Other viruses range in particle diameter from 150 to 10 millimicrons, or down to the size of the largest protein molecules.

From our present knowledge of filtrable viruses, it is apparent that this group may contain infectious agents of very diverse nature. In fact, the differences among the various filtrable viruses are probably of much greater magnitude than the differences among species of bacteria. Some of the largest viruses are coccoid, or spherical, in shape, and the rickettsiae are rod-shaped; these infectious agents resemble bacteria both in their morphology and staining characteristics. Certain other viruses as photographed under the electron microscope have quite a different morphology from bacteria. The viruses of smallpox and cowpox are also among the largest, but they have less resemblance to bacteria, being cuboid, with several internal round dense areas. Some of the bacteriophages (viruses which infect bacteria) are ovoid in shape, with a flagellum, or tail.

Crystallization of two of the plant viruses, tobacco mosaic and bushy stunt of tomatoes, and their characterization as nucleoproteins have led to interesting hypotheses about the nature of these agents as nonliving, self-propagating substances. It is not within the scope of this article to discuss these studies with plant viruses except to relate their implications to similar studies on animal viruses. The virus of tobacco mosaic separated from crude infective sap by ultracentrifugation and chemical procedures is obtained in the form of long fibers pointed at each end. These are now generally spoken of as paracrystals. The virus particle itself is rod-shaped, with a diameter given as 15–20 millimicrons and a variable length of 37–1,400 millimicrons, depending upon the state of aggregation. The virus of bushy stunt of tomatoes has been obtained as true cuboid crystals; the particle of this virus appears to be spherical, with a diameter given as 30–40 millimicrons. Both these plant viruses have been prepared in a state of purity such that no protein constituents derived from the plant tissues can be detected by delicate immunological methods. Chemical analyses reveal no component in these viruses other than protein and nucleic acid. These and other properties of the plant viruses seem to set them apart from animal viruses and bacteria.

The animal viruses have not been obtained in a comparable state of purity. Several have been purified to the extent that they may be identified as characteristic forms under the electron microscope, give single boundaries by ultracentrifugation and electrophoresis, and show a fair degree of uniformity in chemical composition. Estimates of the amount of impurities derived from the host tissues vary from 20 percent to several hundred percent of the content of virus particles. Chemical analyses must therefore be considered less significant than in the case of the plant viruses, but major quantitative differences in constitution between the virus and infected tissue have been found. The larger viruses such as influenza and vaccinia (cowpox) contain nucleoproteins, lipids, cholesterol, neutral fat, and carbohydrates, and their chemical constitution appears to be not very much different from that of bacteria. Bacteriophages thus far

studied appear to contain much larger amounts of nucleic acids than other viruses or bacteria. Copper, biotin (a vitamin of the B group), and a coenzyme (flavine-adenine nucleotide) have been demonstrated in vaccinia virus. Purified viruses, unlike bacteria, exhibit no enzyme activity or metabolism, yet it seems likely that at least some of these agents are metabolically active while growing inside the host cell.

At the borderline between virus and bacterium are the rickettsiae, which cause typhus, spotted fever, and other diseases in man, and a group of coccoid agents, the so-called psittacosis-lymphogranuloma group (Chlamydozoaceae), which cause pneumonia, venereal disease, and eye disease in man and also produce infections in birds and animals. These agents are apparently obligate intracellular parasites like other filtrable viruses and are therefore not as accessible as bacteria to serum antibodies and chemical inhibitors. In other respects they resemble bacteria. In sections of infected tissue fixed and stained by appropriate methods, rickettsiae are found, in masses resembling colonies, in the cytoplasm or nucleus of cells. Similar microcolonies of the elementary bodies of the chlamydozoaceae may be found in the cytoplasm, and recent evidence indicates a life cycle for these viruslike agents. The colonies apparently develop from a minute initial body by repeated fission until the infected cell dies and bursts. The liberated elementary bodies then enter adjacent cells, form new initial bodies, and the process is repeated. Certain bacteria, such as the bacillus causing tularemia, will, under some conditions, multiply in the cytoplasm of cells; there are other relatively large infectious organisms, such as the leprosy bacillus and the plasmodia of malaria, that are, like the rickettsiae and viruses, obligate intracellular parasites. Most bacteria by contrast multiply in the intercellular fluids of an infected animal and are seldom if ever seen in the cytoplasm of cells, except when taken up by phagocytes. With few exceptions, bacteria can be cultivated in lifeless media of varying degrees of chemical complexity.

Some of the rickettsiae and the chlamydozoaceae are susceptible to the action of the same chemotherapeutic agents that have been found effective for the treatment of bacterial infections. Soon after their discovery sulfonamides were used with moderate success in the treatment of lymphogranuloma venereum and trachoma, diseases caused by members of the chlamydozoaceae. This observation emphasized the close relation of these larger viruses to bacteria. Further investigation revealed that certain members of this group were almost as easily inhibited as bacteria by sulfonamides. The rickettsiae are not affected by the sulfonamides, but a related substance, sulfonamidobenzamidine, has been reported to be active against experimental typhus infections in mice.

Penicillin is inhibitory in varying degree to most of the chlamydozoaceae and to some of the rickettsiae in experimental infections of mice and chick embryos. This antibiotic has been used with apparent benefit in the treatment of virus pneumonia caused by psittacosis and related agents, but the required dose is much larger than for bacterial infections. Although rickettsial infections in chick embryos are retarded by either penicillin or streptomycin, these antibiotics have not found successful application in the treatment of rickettsial diseases in man. Streptomycin is apparently inactive against the chlamydozoaceae. In general, therefore, the sulfonamides and antibiotics have been found more limited in effectiveness against these borderline agents than against bacteria. A recently discovered antibiotic, chloromycetin, shows marked chemotherapeutic activity against rickettsiae, and field studies on human disease are at present being done by members of the research staff of the Army Medical School. Another antibiotic, aureomycin, discovered within the last few months, is reported in preliminary experimental studies to be effective both against the rickettsiae and the chlamydozoaceae.

In addition to the antibiotics and sulfonamides, other compounds that inhibit the rickettsiae and the chlamydozoaceae have been found. Several of these substances are more active against experimental infections with these intermediate agents than against bacterial infections. It has recently been shown that certain of the acridines, a class of substances used for some time in protozoal diseases such as malaria and trypanosomiasis, are moderately effective against experimental infections with the rickettsiae and the chlamydozoaceae. The activity of various acridines against these agents apparently does not correspond to their activity against malaria parasites. Atabrine, an effective antimalarial, has no significant activity against the rickettsiae and chlamydozoaceae, whereas a drug called nitroacridine 3582 inhibits these agents but is relatively ineffective against experimental malaria. Another class of substances which has been found to possess therapeutic activity in experimental infections with the rickettsiae, but is inactive against the chlamydozoaceae, is the thionine dyes, methylene blue, toluidine blue, and certain

related compounds. In their chemical structure these dyes resemble the acridines, and also in a general way the B vitamin riboflavin.

Para-aminobenzoic acid prevents the bactericidal effect of sulfanilamide and related compounds, and is a component of the vitamin folic acid. Presumably, sulfanilamide inhibits the growth of bacteria by interfering with their metabolism of *p*-aminobenzoic acid, as will be discussed in more detail. In the rickettsial diseases *p*-aminobenzoic acid is an active chemotherapeutic agent, but sulfanilamide is without effect. Para-aminobenzoic acid has been used clinically with some benefit in the treatment of typhus fever and scrub typhus. The therapeutic effect in man, however, is not as striking as in experimental animals.

Certain hypotheses about the mode of action of some of the rickettsiostatic drugs merit discussion because they introduce a new concept applicable only to obligate intracellular parasites: namely, that substances like *p*-aminobenzoic acid and toluidine blue modify the metabolic processes of the infected cell rather than act directly on the infectious agent. Recent observations have shown that *p*-aminobenzoic acid stimulates oxygen consumption by embryonated eggs, whereas rickettsial infection depresses the consumption of oxygen. Toluidine blue is also known to serve as a readily reversible carrier of hydrogen in oxidation-reduction systems. It is possible, therefore, that these substances, by increasing oxidation processes of infected cells, produce unfavorable conditions for the growth of rickettsiae, or furnish the infected cells a metabolic pathway alternative to those enzyme systems destroyed or impaired by the infection. Increase in temperature or supply of oxygen also tends to raise the metabolic rate and increase the survival rate of chick embryos and mice infected with rickettsiae. Substances such as potassium cyanide, which in sublethal amounts depress the oxygen consumption of embryonated eggs, seem to favor the growth of the rickettsiae. It is conceivable that chemotherapeutic agents could also beneficially modify or supplement various cellular enzymatic processes other than those concerned with oxidation.

Most of the substances mentioned, and many others, have been tried in experimental infections caused by the smaller viruses, but the results have been negative or inconclusive. They are also ineffective against the virus of vaccinia and related agents, which are apparently as large and as complex as the chlamydozoaceae. Possible exceptions are certain of the acridines and a drug named hexamidine. The latter substance and the previously mentioned nitroacridine 3582 are reported to have some inhibitory effect on the virus of influenza in embryonated eggs, but these observations lack confirmation. Other acridines inhibit the growth of bacteriophage without corresponding diminution of growth of the bacterial host cell.

Some success has been attained by following a concept involving a somewhat different approach to the problem, namely, attempts to block the attachment of the virus to the host cell. The viruses of influenza and mumps are adsorbed on the red blood corpuscles of certain species and agglutinate these red cells. It has been found that the attachment of these viruses to red cells is prevented by certain polysaccharides, or complex sugars, and by extracts of red blood cells and bacteria. Some of these substances inhibit the multiplication of the viruses of influenza and mumps in embryonated eggs.

Since the influenza virus is considerably smaller than the intermediate agents discussed above (although it superficially resembles a minute bacterium in morphology and chemical composition), successful inhibition of this virus would represent a step beyond the borderline. Scientists at the Rockefeller Institute for Medical Research have found that pectin, a complex carbohydrate from fruits, and related substances prevent infection of influenza virus in embryonated eggs when given in large doses half an hour before, or up to two hours after, the virus. Workers in Australia have obtained similar results with what appears to be an unrelated agent. Extracts of cholera vibrios and certain other bacteria were found to contain an "enzyme" that destroyed the receptors by which the virus became attached to red corpuscles. This enzyme also destroyed the virus receptors in the extraembryonic cavities of chick embryos and prevented infection with influenza or mumps viruses. When the receptor-destroying substance was removed, the embryonic tissue recovered susceptibility, apparently because of regeneration of receptors, within twenty-four to forty-eight hours.

Of apparently somewhat different mechanism is the action of certain polysaccharides derived from bacteria on the mumps virus and the pneumonia virus of mice as studied by another group of workers at the Rockefeller Institute. These polysaccharides prevented agglutination of red blood corpuscles by influenza virus or mumps virus. They did not, however, prevent the adsorption of influenza virus to red blood corpuscles or inhibit the multiplication of this virus in chick embryos; they did prevent adsorption and multiplication of mumps virus under comparable conditions. In-

hibition of multiplication of the mumps virus in the allantoic cavity of chick embryos was obtained even when the polysaccharide was given as long as four days after the virus. Conversely, with the pneumonia virus of mice, several polysaccharides prevented multiplication in the lungs of mice but had no effect on the agglutination of red corpuscles by this virus. In these studies there was no evidence that the polysaccharides prevented attachment of the virus to the host cell or worked directly on the virus itself. Thus a series of observations based on the concept of blocking the attachment of virus to cells has led to the discovery of active inhibitory substances, but the mechanism of action of at least some of these appears to be different from that which was originally postulated.

The antibacterial action of certain chemotherapeutic agents is attributed to specific interference with an enzyme system or metabolic process of the bacterial cell. Success in the treatment of disease with such drugs depends on the lack of a correspondingly poisonous effect on the tissues of the infected animal. It is possible that the same process can occur regardless of whether the infecting agent multiplies (like bacteria) outside or (like viruses) inside cells, provided, in the case of viruses, that the chemotherapeutic agent can penetrate the cell membrane and that it is not destroyed inside the cell.

It is likely that the sulfonamides, acridines, and various antibiotics affect the same enzyme systems in the rickettsiae and chlamydozoaceae as they do in the bacteria. The infectious agents intermediate between viruses and bacteria may have lost many of the abilities for synthesis of their own essential nutrients and have thus become dependent for these on the host cell, but they probably have retained some of the bacterial enzymes inhibitable by chemotherapeutic agents. In the chlamydozoaceae, the mechanism of action of the sulfonamides and their reversal by the chemically related substance, *p*-aminobenzoic acid and its derivatives, seems to be identical with that found in bacteria. Other members of this same group of organisms are highly resistant to the action of sulfonamides in a way analogous to resistant bacteria.

In some of these intermediate infectious agents the enzyme systems may have undergone variation or mutation so that there have been evolved new systems which are related to, but not identical with, those found in bacteria. Since rickettsiae are not inhibited by sulfonamides but are inhibited by the closely related substance, sulfonamidobenzamidine, we may have here an example of such enzyme modification with a corresponding change in the structure of the substance required for inhibition. Other examples may be found in the acridine derivatives where modification in chemical structure influences the relative activities for protozoa bacteria, rickettsiae, and viruses.

The smaller viruses may be resistant to the antibacterial drugs, not so much as a result of their intracellular site of multiplication, but because they lack the vulnerable enzyme systems attacked by these substances and are dependent on other systems intimately associated with the synthetic and metabolic activities of the host cell. In viruses such as those of influenza, mumps, and rabies, which are one half to one third the size of the chlamydozoaceae, there may be still other enzyme systems which can be specifically inhibited by as yet undiscovered substances; or it is possible that other more active derivatives of acridine or hexamidine may be found. The smallest viruses may have been reduced by a retrograde evolutionary process to such a condition of metabolic dependence on the host cell that they have no enzymes and are little more than carriers of a hereditary pattern reproduced by some mysterious process in the cell. Is it possible to modify these enzymatic processes of the cell so as to present an unfavorable medium for the entrance and multiplication of the virus? Such a mechanism may be involved in the action of *p*-aminobenzoic acid on rickettsial infections, and in the action of polysaccharides on the pneumona virus of mice. It has also been found that deficiencies in certain vitamins seem to slow down the progress of infections with viruses like poliomyelitis in experimental animals, whereas other types of vitamin deficiency may favor the growth of viruses.

Further extension of the range of chemotherapy for the diseases caused by filtrable virus might be accomplished by additional empirical discoveries. It appears, however, that a satisfactory solution of the problem requires a knowledge more complete than we have at present of the fundamental life processes of cells.

THE NINETY-EIGHTH PRESIDENT OF THE AAAS

J. J. CHRISTENSEN*

Division of Plant Pathology and Botany, University of Minnesota, St. Paul.

A DISTINGUISHED national and international figure in his chosen field, plant pathology, Professor E. C. Stakman, of the University of Minnesota, is one of Minnesota's outstanding scholars and scientists. He holds a commanding position also in the broader fields of biology and agriculture, as attested by an ever-increasing demand for his services in various consultant and advisory capacities.

For many years Dr. Stakman has been an active Fellow of the American Association for the Advancement of Science and since 1947 has served as a member of its Executive Committee. He has contributed effective leadership to other scientific societies as well, particularly to The American Phytopathological Society, as president in 1922 and as committee chairman, including the War Board of American Plant Pathologists during World War I and the War Committee during part of World War II. His work in the National Research Council began in 1931 with membership in the Division of Biology and Agriculture and has included various offices; at present he is vice chairman of the Division and also chairman of the Committee on Aerobiology, which is engaged in a study of aerial dissemination of disease organisms between regions and between continents. In the American College of Allergy he serves on the Research Council. He is a member of the Advisory Committee on Biology and Medicine of the United States Atomic Energy Commission. Last fall he went to Japan on a scientific mission for the National Academy of Sciences. These and similar activities indicate his breadth of interest and experience in the agricultural and biological fields. What he has gained from this experience and from his other varied interests will be available for the furthering of the objectives of the AAAS when he becomes its ninety-eighth president on January 15.

Professor Stakman has made many outstanding contributions to science in the fields of plant pathology and mycology. His investigations are interesting not only because they contain much that is of practical importance to agriculture, but because of their elucidation of certain fundamental biological concepts. His pre-eminent researches have been on the genetic variation and the nature and extent of physiologic specialization in parasitic fungi, and in the epidemiology of stem rust. In his work on physiologic specialization he demonstrated for the first time that within a variety of a species of the stem rust fungus there were races, or strains, that looked alike but which behaved differently on different varieties of cereals and grasses. On the basis of this work he was awarded the Emil Christian Hansen gold medal and prize, in recognition of his accomplishments in the development of new ideas and of methods for investigating the rust problem. These contributions are applicable not only to a study of rust but to investigations of microorganisms in general.

Dr. Stakman's discoveries in physiologic specialization of fungi have been a tremendous stimulus to basic research in microbiology; they have changed our concept of a species if not more than, then as much as, any other single discovery in biology; they have modified our taxonomic concepts in the lower organisms; and they have revolutionized the method of developing resistant varieties of crop plants. They also are of great value in the production of antibiotic substances and in other industrial processes associated with microorganisms. A knowledge of specialization is of fundamental importance to sound procedure in justifying and in establishing plant quarantine regulations.

In spite of Dr. Stakman's zeal for research, he is primarily and outstandingly a teacher. He is an exponent of good teaching and has contributed materially to building up sound curricula for agricultural students at the University of Minnesota. In classrooms as well as in personal contact, he is constantly inspiring and stimulating students and fellow-workers to find out things by their own experimental investigations, to acquire a sound backlog of scientific facts determined by other workers, and to assimilate and organize demonstrated principles of science that are applicable to future problems. Lectures and seminars are usually not given in a formal, standard manner but are frequently interspersed with questions and with class discussion; often the philosophical implications of scientific data are considered. By devious means he

* The writer is greatly indebted to Miss Laura M. Hamilton for her assistance in the preparation of the manuscript.

stimulates discussion and will readily take either side of an argument in order to develop the ability of students to organize and synthesize facts. He is also a firm believer in laboratory work; to him there is no substitute for laboratory and practical experience in the biological sciences.

He also encourages the development of a broad cultural education; as one means of achieving this, he started a library, which includes the arts and the humanities, for the seminar room of the plant pathology building at Minnesota, to which students traditionally contribute upon their departure and to which Stakman honoraria are channeled. Although he is a strong believer in rigorous educational discipline, to most graduate students he is a real friend and counselor as well as an understanding teacher. He is at ease with students of all ages, understands their educational and personal problems, and participates in their sports, cultural activities, political and social arguments, and all intellectual discussions.

The remarkable fact is that Stakman's friendship with a student does not end when the student has completed his formal education and has earned his coveted degree. Many continue to bring their problems to him. They also like to bring their children to see him, and the children and "Stak" are soon fast friends. They quickly sense that he is a warm-hearted, human sort of person.

Students come from all parts of the world to study under him. Under his supervision more than 180 students have obtained higher degrees, and many others have come to Minnesota to take post-doctorate work. His former students are to be found in scientific laboratories of the United States and of the world. Some of his students have attained distinction in biological research; others have assumed executive positions in their government's agricultural organizations; a few have contributed to important international agricultural and educational programs, among them representatives to the Food and Agriculture Organization.

Dr. Stakman's enthusiasm for education and research has not been confined to the United States. He has traveled extensively on scientific missions in this and other countries and has been employed in an advisory capacity, during periods of leave from the University, in far-flung regions of the world. He has made several professional trips to Europe and has traveled around the world twice. In 1921 he was sent to Alaska by the U. S. Department of Agriculture to study rusts, and the next year to Europe to study the barberry, alternate host of black stem rust, in relation to the control of this destructive disease of wheat and other small-grain crops. For many years he has directed and personally made observations on stem rust in Mexico and the United States, particularly in the Great Plains area. In this work, Dr. Stakman and his colleagues were the first to use the airplane in determining the spore content of the air in studying the long-distance dissemination of plant-disease organisms. In the winter of 1930–31 he was guest lecturer at the University of Halle, Germany. He also has participated in international scientific congresses: as delegate to the first Pan-Pacific Science Congress in Australia in 1923; in the International Botanical Congress at Cambridge in 1930; and at Amsterdam in 1935, where he served as vice president of the section on microbiology.

While on leave in 1930, Professor Stakman was employed by the Firestone Plantations Company to investigate disease problems on their rubber plantations in Liberia; their present research laboratory there was set up on the basis of his recommendations. He also has firsthand knowledge of rubber growing in Ceylon and other areas of the Far East. During World War II, as part of the program of national defense in the Western Hemisphere, he was sent by the U. S. Department of Agriculture into northwestern South America to study native rubber in the jungles and the possibilities of developing *Hevea* rubber there.

In recent years Dr. Stakman has been active in the development of educational and research programs in Latin America. In 1941 he served as member of a survey commission appointed by the Rockefeller Foundation to study agricultural conditions and problems in Mexico, and in 1943 helped to implement the Foundation's program of agricultural improvement. As an extension of this work he has traveled recently in several Central and South American countries to study agricultural problems and to determine the status of education and research in the natural sciences. On the basis of his study, more extensive cooperative investigations have been instituted on plant-disease problems of international importance. He has been instrumental in encouraging Latin-American students to come to the United States for graduate work, and the first of them now have returned to important positions at home. Through his interest and encouragement, the Mexican Phytopathological Society has recently been organized.

As a result of this extensive experience, he has a sound knowledge and sympathetic understanding of the problems of this and other countries. And at the same time, as his students will testify,

he has an understanding sympathy for the problems of the individual in both Eastern and Western Hemispheres.

Elvin Charles was born in May 1885, in Algoma, Wisconsin, but has lived in Minnesota since early boyhood He grew up in the village of Brownton, Minnesota, where he obtained his early education and farm experience. Stakman has not forgotten his farm life and enjoys relating boyhood experiences. He still has a genuine interest in rural life and is well versed in methods and types of farming, not only in Minnesota but in many other parts of the world.

In 1902 he graduated from Cleveland High School in St. Paul, and in the fall registered in the College of Science, Literature and the Arts of the University of Minnesota, majoring in biology and political science. He received the Bachelor of Arts degree in 1906 and was honored by election to Phi Beta Kappa. During the next two years he taught in Minnesota high schools at Red Wing and at Mankato, and in 1908–09 was superintendent of the school at Argyle. In 1909 he became a member of the University staff as an instructor in plant pathology and began his graduate work under his former botany professor, E. M. Freeman. Two years previously, Professor Freeman had established at St. Paul one of the first departments of plant pathology in the United States. In 1910, Stakman received the Master of Arts degree and, in 1913, the degree of Doctor of Philosophy. He now holds, in addition, an honorary degree of Doctor of Natural Sciences from the University of Halle-Wittenberg, Germany.

From 1913 to 1941, Dr. Stakman was head of the section of plant pathology; at present he is chief of the Division of Plant Pathology and Agricultural Botany. He also has been associated with the U. S. Department of Agriculture in various capacities since 1914. In 1918–19 he was pathologist in charge of the barberry-eradication campaign, in which he has long been interested. Since 1917 he has been Pathologist and Agent in the USDA in charge of cooperative investigations carried on with the University of Minnesota on stem rust and rust epidemiology. He also represents the University in directing cooperative relationships with Federal pathologists stationed at the Minnesota Experiment Station.

In 1917 he married Louise Jensen, of Minneapolis, who was then mycologist at Minnesota. They live near the St. Paul campus of the University.

Dr. Stakman has been honored by membership in the American Academy of Arts and Sciences, the American Philosophical Society, founded by Benjamin Franklin, and the Washington Academy of Arts and Sciences. He also holds, among others, honorary life membership in the Sydney (Australia) University Agricultural Society; he was made an honorary member of the Canadian Phytopathological Society in 1945, and a foreign member, in 1946, of L'Académie Royale d'Agriculture de Suède, founded in 1811. There is in addition a long list of professional societies of which he is a member.

In both speaking and writing, Stakman expresses his thoughts with ease, clarity, and forcefulness. He is in demand as a speaker and last year traversed the United States in a lecture series sponsored by Sigma Xi, honorary scientific society. His publications include about 200 titles and embrace a wide range of researches. Many of the articles are classics in their field. Some are of a summary type, others have a general scientific and philosophical aspect. Most of his writings have been published in scientific journals or as bulletins, and some as chapters in biological books. He has pressing invitations to write books but has been too much occupied in research and in directing graduate students to undertake the compilation of texts. From 1925 to 1929, he was editor-in-chief of *Phytopathology*; before the war interrupted publication, he was American editor of *Phytopathologische Zeitschrift*; at present he serves on the editorial board of the *Annual Review of Microbiology*.

"The Big Chief," as the students call him, gets a tremendous amount of enjoyment from life, and to everything he does brings a stimulating vigor. He is genuinely interested in any subject whether it be history, politics, religion, football, or tobacco. (One of his hobbies is the collection of pipes.) Being a voracious reader and possessed of an incredibly retentive memory, he is well versed in a great many fields. He likes intellectual discussion; he has the unique ability to weave together apparently insignificant facts into a sound, logical argument, and will fight for what he considers a valid premise. Particularly fond of history, he will discuss at the least provocation diverse political situations with historical perspective and background.

He has always been very much interested in sports. He played baseball, tennis, and handball; and he coached baseball and football in high school. For years he was the driving force behind the plant pathology softball team at University Farm, first as a player, later as the coach. His motto was:

"Whether you work or play, do your very best." His team won the campus championship eighteen out of twenty years. He still has the happy faculty of relaxing completely, and he follows Minnesota football closely.

Dr. Stakman has probably contributed fully as much as an educator and propagandist for the advancement of science, culture, and civilization as he has as a professional scientist. He has a deep and abiding interest in education and in fundamental science, and he understands thoroughly their function and use. For years he has been a tireless fighter for the advancement of science, advocating more basic research, not necessarily for the pressing problems of today but to provide a reservoir of facts for future use. According to him, "Fundamental research over a long period of years . . . gives the most practical results."

ELVIN CHARLES STAKMAN

A BOTANICAL NONCONFORMIST*

RALPH E. CLELAND

Professor Cleland (Ph.D., Pennsylvania, 1919), who is head of the Department of Botany, Indiana University, delivered the address from which this article is taken as retiring president of the Botanical Society of America at the Dinner for All Botanists, Washington, September 11, 1948.

THE evening primrose, Oenothera, began its scientific career in the 1880s, more than 60 years ago. It chose as the stage upon which to make its debut, not its native haunts, but a foreign shore, having been transported from America to Europe, in all probability, in ships carrying ballast. Springing up on the ballast heaps of Europe, the evening primrose spread relatively unnoticed over the Continent. A few taxonomists examined it and attempted to classify it. A few florists picked out one or two of the showier kinds and introduced them into gardens in England and Germany. But the unique features of the evening primrose went unnoticed until 1886, when De Vries happened upon it. Since that time, the plant has received more than its share of attention.

The thing that first attracted De Vries' attention was the peculiar habit which the plant showed of giving rise on rare occasions to individuals very different from the common run of offspring. De Vries noted this in an abandoned field near Amsterdam, which the evening primroses had taken over. Lamarck's evening primrose, as De Vries called it, is a showy form, and the field where he found it must have been a place of beauty. But what attracted De Vries was the fact that, whereas most of the plants in the field were indistinguishable from one another, here and there a very different individual reared its head. Could these be what De Vries had been searching for, new species in the act of originating? Subsequent breeding experiments showed that such forms arose in each generation from *lamarckiana*, and that some, at least, of these new types bred true when selfed. De Vries was encouraged to think that he had a case where new "elementary species," as he called them, were springing full-fledged into existence, like Athena from the head of Zeus. Upon this assumption, and based largely upon Oenothera, De Vries developed his celebrated mutation theory of evolution.

Unfortunately it turned out, however, that there was much more to the behavior of the evening primrose than met the eye. These so-called mutations were not what De Vries thought they were, but were instead just one manifestation of the peculiar way in which Oenothera manipulates its chromosomes. At that time, of course, the part played by chromosomes in heredity was only dimly suspected.

It was not long before De Vries discovered that Oenothera is unusual in another respect. Not only does it throw occasional sports, it is also unique in its *normal* breeding behavior. Although De Vries made this discovery prior to 1900 when, along with Correns and Tschmermak, he unearthed Mendel's published work, he was already familiar from his own experiments with the essentials of what is now called Mendelian inheritance. He knew that homozygous or pure individuals breed true and that hybrids or heterozygous plants tend to produce splitting progenies. He therefore interpreted the fact that his races of Oenothera bred true (except for the rare mutations) to mean that they were pure. On this basis, two pure races crossed to each other should yield but one class of progeny. Instead, they often produced splitting progeny, and the hybrids thus produced, which should have yielded splitting progenies when selfed, often bred true. Thus De Vries was confronted with an anomalous situation. Other plants breed true when pure, and produce varied progeny when they are hybrid. Supposedly pure races of Oenothera, when crossed, often give splitting progenies, and undoubted hybrids often breed true. Thus, Oenothera seemed often to behave in a manner diametrically opposite to that of other plants. De Vries was unable fully to understand this behavior. He could not bring himself at first to admit that his races were heterozygous, as Renner insisted, for that would have called in question the significance of his so-called mutations. He tried to explain the formation of more than one kind of gamete in his supposedly pure races by assuming the presence of pangenes in a labile or unstable condition, which mutated, thus giving more than one kind of germ cell. This, however, failed to explain the behavior of Oenothera with complete satisfaction.

* The help of the Rockefeller Foundation and of Indiana University in all this work is gratefully acknowledged.

It remained for Renner to give the correct genetical interpretation. (Bartlett in this country hit on the same fundamental conception independently.) In so doing, he discovered uniqueness indeed in the genetical behavior of the evening primrose.

Briefly, what he found was this. Although the evening primrose has 14 chromosomes and should, therefore, according to normal Mendelian behavior, have 7 independent groups of genes, it frequently behaves as though it had but one pair of chromosomes and one linked group of genes. In most other organisms, the genes received from father and mother become shuffled when an individual produces reproductive cells, so that many combinations of paternal and maternal genes are possible among the germ cells produced. Thus, it should theoretically be possible for an evening primrose, with 7 pairs of chromosomes, to produce 128 different kinds of sperms and eggs. Instead, most races of Oenothera produce only 2 kinds, and these are identical with the sperm and egg which united to form the plants producing them. This is the behavior one would expect if it had received only 1 chromosome through the sperm and 1 through the egg. It receives, however, 7 from each gamete.

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The other peculiarity analyzed by Renner was the fact that when two different races are crossed, the hybrids formed do not always behave as their parents do. They do not always produce just two kinds of gametes identical with the ones that united to form the hybrid. In other words, the genes which were all linked into a single group in each of the parents have now become partially unlinked in the hybrids. Strangely enough, if race 1 is crossed with races 2, 3, or 4, the genes transmitted to the F_1 hybrids are found to have become unlinked in different ways in the different hybrids, but always in the same way in the same hybrid, no matter how many times this hybrid is produced. Thus, one cross may produce a hybrid with two linkage groups, another may produce one with three groups, another one with four, and so on. Two genes may be linked in one hybrid but independent in another. In short, the single linkage group found in the parents has disintegrated in the hybrids, has disintegrated in different ways in different hybrids, but always disintegrates in the same way in the same hybrid.

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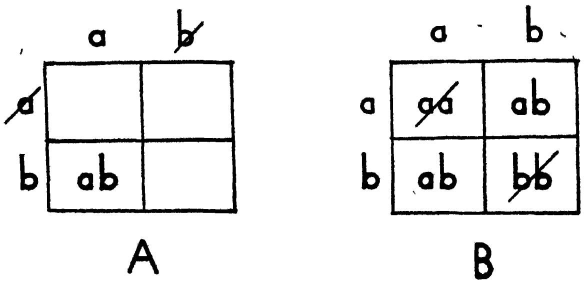

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This behavior, which was unknown until found in Oenothera, furnishes the basis for the breeding peculiarities that Renner analyzed. Obviously, what happens is that a given chromosome, descended, let us say, through the sperm, synapses at one end with a chromosome derived from the egg and at the other end with a different egg-derived chromosome. In other words, it is homologous at one end with one chromosome and at the other end with another chromosome, and so it synapses at its two ends with two different chromosomes. This is true of all the chromosomes in the circle. The result of this setup is that chromosomes of paternal and maternal derivation alternate with each other in the circle, the force holding them together being the ordinary synaptic force.

But if paternal and maternal chromosomes alternate in the circle, and if adjacent chromosomes go to opposite poles of the spindle, obviously all paternally derived chromosomes will go to one pole and all maternally derived choromosomes will go to the other pole (Fig. 3). Then all paternally derived genes will go to one pole and into the same germ cells, and all maternally derived genes will go to the other pole and into other germ cells. The result will be the same as though all paternal genes were in one chromosome and all maternal genes in a single corresponding chromosome. Thus, although there are 14 chromosomes, the plant behaves normally as though it had but one pair of chromosomes, producing only two kinds of germ cells—one kind being identical with the sperm, the other with the egg that united to produce the plant. Since each set of genes has a zygotic lethal and cannot as a result exist in double dose, and since the plant is generally self-pollinated, thus excluding as a rule sets of genes from other plants, all the progeny of a plant will normally be exactly like itself, recombining the same two sets of genes which this plant itself received and which it transmits intact to the next generation. This will continue indefinitely, the same two kinds of sperm and egg being produced in each generation. Each set of genes is transmitted from generation to generation intact and indefinitely. Each is a continuing entity. We call it a Renner complex. The plant breeds true, therefore, but not because it is homozygous—in fact, in all such races the two sets of genes are noticeably to strikingly different genetically.

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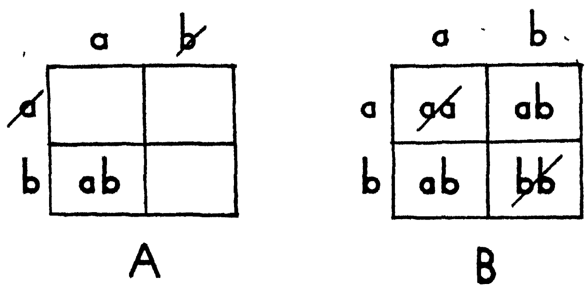

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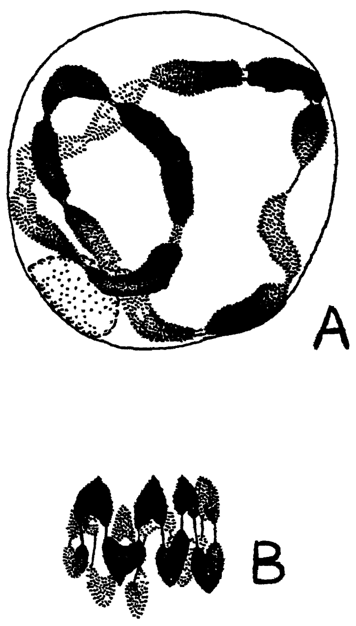

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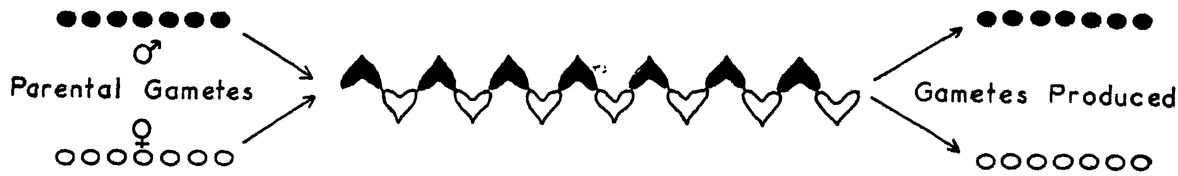

Fig. 3. Diagram to show that the meiotic mechanism produces gametes that are identical with the gametes which united to form the plant. Alternation of paternal and maternal chromosomes in the chain, and the separation of adjacent chromosomes, bring about this result.

this suggestion by making crosses between races and examining the hybrids to see how their chromosomes behaved in relation to the way their genes behaved. I was fortunate to have the opportunity of spending two summers and the intervening winter with Renner and Oehlkers, subjecting this hypothesis to extensive tests. The chromosome behavior of many hybrids was examined, and these hybrids were then subjected to breeding tests to determine as far as possible the number of genetic linkage groups present. The results were striking. Those hybrids which had a single large circle bred essentially true, showing that their genes were linked into a single group. Hybrids with several smaller circles or a mixture of circles and pairs produced splitting progenies, the complexity of this splitting increasing as the number of independent circles or pairs increased. Thus the phenomenon which Renner observed in hybrids was found to follow a very simple rule; namely, the number of linkage groups is equal to the number of chromosome groups, whether these be pairs or circles. Many hybrids between races have been studied since this initial investigation was made. All 15 arrangements into circles or pairs have been found over and over again among these hybrids, and their genetic behavior, wherever tested, has been what might be expected on the basis of this hypothesis.

All of this, however, raises further questions. Why do the Oenothera chromosomes behave in this peculiar way? Why do they not pair normally as they do in other organisms? In some races of Oenothera, particularly in some subgenera, they do behave normally, but the majority of the races throughout most of the range have ⊙ 14 as their chromosome configuration. Why is this peculiar behavior found in the races, and why are different chromosome arrangements found in different hybrids?

Belling was the first to offer a fruitful suggestion in this connection. Finding a circle of 4 chromosomes in a hybrid between two races of Datura, he suggested that one of the parental races involved had suffered an exchange of segments (reciprocal translocation) between two nonhomologous chromosomes (Fig. 4). From one parent the hybrid had received only normal chromosomes, but from the other parent it had received a pair of translocated chromosomes. When germ cell formation took place in this hybrid, and homologous regions synapsed, the translocated chromosomes gave ⊙ 4 with the untranslocated ones. Belling suggested that reciprocal translocations such as this might be the explanation of the Oenothera behavior, large circles being formed as the result of a series of translocations, one following the other.

There is no time to go into the way in which it was proved that Belling's suggestion was indeed valid for Oenothera. Suffice it to say that the work of a number of different investigators has shown beyond doubt that circles have come into existence in Oenothera as a result of reciprocal translocations, the large circles being the result of series of successive translocations. Such translocations are known in other organisms, especially in plants, but Oenothera is unique among organisms in the extent to which it has experienced this sort of exchange. A few other genera are known in which a single species has had sufficient background of interchange to have developed a circle incorporating all its chromosomes. No other group is known, however, in which the great bulk of the races existing in nature have had

TABLE 1

POSSIBLE ARRANGEMENTS OF 14 CHROMOSOMES INTO CIRCLES AND PAIRS

○ 14
⊙ 10, ⊙ 4
○ 8, ⊙ 6
⊙ 6, ⊙ 4, ⊙ 4
⊙ 12, 1 pair
⊙ 8, ⊙ 4, 1 pair
○ 6, ⊙ 6, 1 pair
⊙ 4, ⊙ 4, ⊙ 4, 1 pair
⊙ 10, 2 pairs
⊙ 6, ⊙ 4, 2 pairs
⊙ 8, 3 pairs
⊙ 4, ⊙ 4, 3 pairs
⊙ 6, 4 pairs
⊙ 4, 5 pairs
7 pairs

such a background. Oenothera has gone to an extreme in this regard.

As a result, the Oenothera population possesses a structure which is in many respects unparalleled among plants or animals. Throughout most of the range, the population consists of a multitude of races with ⊙ 14, more or less isolated from one another because of the self-pollinating habit, breeding true because of the large circles and the lethals which allow only one genetic combination to exist in each race, this same genetic combination appearing over and over again in practically every individual of every generation. The circle of 14 chromosomes present in each race is composed of two sets of chromosomes which have had different histories of interchange and are therefore entirely unlike each other in the arrangement of their segments. Not a single chromosome of one set is completely homologous with any chromosome of the other set. To make this clear, let us designate each chromosome by two numbers connected with a dot, the numbers representing end segments of the chromosome. One set of chromosomes might be designated as follows: 1•2 3•4 5•6 7•8 9•10 11•12 13•14. The other set of chromosomes associated with it to form ⊙ 14 will have an entirely different arrangement of segments, let us say, 2•3 4•5 6•7 8•9 10•11 12•13 14•1. Synapsis of corresponding segments will produce ⊙ 14. There are of course many arrangements of segments which will give ⊙ 14 with each other, and many different arrangements exist among the hundreds or thousands of races bearing ⊙ 14.

How has such a situation arisen? It is logical to suppose that once upon a time the ancestors of the Oenotheras were all normally behaving plants, with paired chromosomes. Then translocations began to take place and circles began to arise, at first small, then larger and larger. Did this happen within races, each race with ⊙ 14 thus being the result of a separate and distinct evolutionary process? The evidence indicates that this was not the case but that different interchanges occurred in different strains, and then by occasional hybridization, and by interchanges within or between the complexes of the new hybrid strains, new segmental arrangements were formed, many of which by further hybridization came to be associated with different complexes to form different races, a given arrangement thus being present in more than one race. As a result, we find that many different segmental arrangements exist, and many combinations of complexes are to be found in different races. We also find that, although the two associated complexes in a race are usually dissimilar in segmental arrangement, it is common to find complexes in different races which are similar or even identical in the arrangement of their segments. All of this suggests that when we find complexes in different races which are similar or identical segmentally, they are phylogenetically closely related and have had a recent common ancestor. This gives us a clue to the evolutionary relationships that exist in the group.

Using this clue, we have tried to determine where the relationships lie among the North American races of the subgenus Euoenothera. This enormous group, the largest of the 15 subgenera of Oenothera, is the one that includes most of the races studied by De Vries and Renner. It is also a very difficult group taxonomically. I would like to show how we have tried to ferret out these relationships and very briefly to indicate what sort of situation we have found. Our method has been to take two races and cross them. If the complexes of these races are closely related, their segmental arrangements will be similar or identical, and when they are combined in the hybrid they will give mostly or entirely paired chromosomes with each other. If the complexes of the two races are unrelated, they will have unlike arrange-

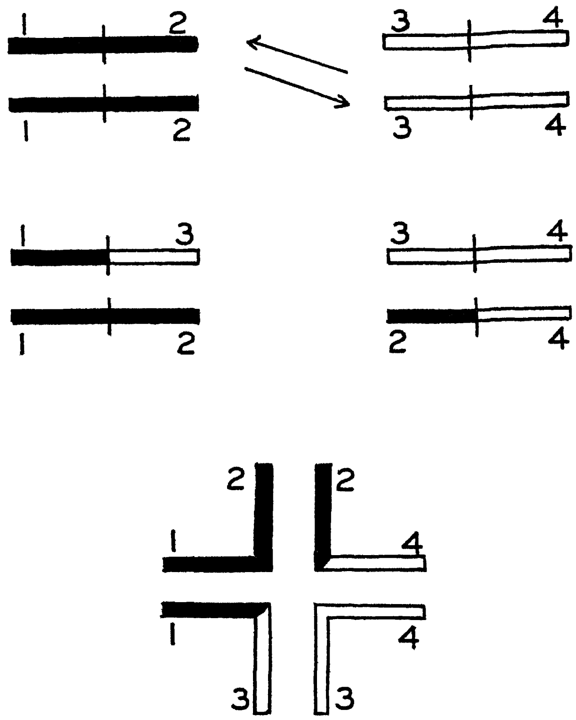

Fig. 4. Diagram illustrating reciprocal translocation. *Above,* two chromosome pairs, their ends numbered 1 and 2, 3, and 4, respectively. *Middle,* arrangement of segments following interchange of ends 2 and 3. *Below,* result of synapsis following interchange. A circle of 4 will result, instead of separate pairs.

ments of segments and will give large circles with each other when combined. By making all sorts of crosses, one can discover in this way many cases of relationship.

We can go farther than this and take one complex as a standard and determine the segmental arrangements of other complexes in terms of this standard. We can then take each of the arrangements thus determined and compare it with the primitive arrangement, and in many cases can see what interchanges have taken place to give rise to these arrangements. We can get a rather clear idea in this way of how they have evolved and how they are related both to the original and to each other.

As a result of our analysis of segmental arrangements, we find that the hundreds of races of the North American Euoenotheras tend to fall rather sharply into a small number of larger groupings, of which we have so far recognized seven. Each of these groupings is composed of races that have much in common. Phenotypically they are much alike, geographically they occupy near-by areas, and their complexes show a close relationship in segmental arrangement to one another. On the other hand, each of the large groupings is quite different from the others in cytogenetic behavior, and in most cases it differs from them also in phenotype and in geographical distribution. These groupings, therefore, have many of the features of species, and where they can be recognized phenotypically (in a few cases they cannot), it is probable that they should be accepted as species.

I shall not try to describe these groupings at this time. Suffice it to emphasize that the North American Euoenotheras are made up of multitudinous true-breeding and relatively isolated races and that these races fall rather neatly for the most part into natural groupings that in some cases may prove sufficiently distinct to deserve acceptance as species.

There is one other aspect of the behavior of the evening primrose that I should like to mention. Oenothera is a shining example of the fact that seemingly deleterious alterations of the hereditary mechanism may sometimes and under certain circumstances be turned to good advantage in the course of evolution. Oenothera has suffered several kinds of alteration or mutation, any one of which by itself might have placed it at a disadvantage in the struggle for existence. By combining all these apparently disadvantageous mutations into one plant, however, they have been turned to advantage in the case of Oenothera and have given it greater survival value than it probably would have had if these changes had not taken place.

The first seemingly deleterious type of alteration to appear was the one we call reciprocal translocation. In most organisms, translocations are disadvantageous, since they lead to frequent failure of the chromosomes to segregate properly in the reduction divisions, resulting in sterile germ cells. Oenothera, however, seems to have overcome this danger quite neatly. It fortunately began with a set of chromosomes all of which were of the same size, with spindle attachment regions placed in the center of the chromosome. It seems to have developed a technique by which, as a rule, the breaks occur close to the spindle attachment point. Thus, all the exchanges whose products have survived, and all which have been observed to occur naturally, have resulted in equal interchange segments, and have therefore produced little alteration in the size or structure of the chromosome. When the chromosomes in a circle are all equal in length and have median spindle attachments, the amount of irregularity in the separation of the chromosomes in meiosis is reduced to a low level and the resultant sterility is negligible. This is the case in Oenothera which, because it has not suffered seriously from the occurrence of interchanges, has had a tendency to retain the interchanges that have occurred, thus developing in the population a large amount of heterogeneity from the standpoint of segmental arrangement.

A second type of mutation developed by Oenothera that would seem to constitute a handicap was the production of lethals. Lethals produce serious sterility. One kind, the gametophytic lethal, kills every sperm it enters, or every egg, as the case may be; another kind, the zygotic lethal, kills every zygote into which it enters through both sperm and egg. A pair of zygotic lethals, one in each complex of genes, will kill 50 percent of the offspring. The presence of such a lethal is therefore a serious matter, and lethals must have been a great handicap in the earlier stages of Oenothera evolution. But the time came when Oenothera was able, through the increasing heterogeneity in segmental arrangement, to bring together sets of chromosomes which were entirely unlike each other and which therefore gave ⊙ 14 with each other. When such situations began to arise, Oenothera was at last able to derive an advantage from this erstwhile handicap, the presence of lethals, as the following will show. If a pair of balanced lethals is present in a single pair of chromosomes, it will ensure the heterozygosity of this pair, since it is impossible for either chromosome of the pair to exist in double dose. Hetero-

zygosity is advantageous in that it contributes toward hybrid vigor. It would, therefore, be of advantage if all the chromosomes could be made permanently heterozygous and thus bring about a maximum of hybrid vigor. But a pair of balanced lethals cuts fertility down to 50 percent. If balanced lethals were to appear in all the pairs of chromosomes, the increased hybrid vigor thus achieved would be offset by a tremendous reduction in fertility. The first pair of lethals would cut the fertility down to 50 percent; a second pair would cut the remaining fertility down another 50 percent; and so on. Obviously, an increase of this sort in the number of lethals would result in extinction. When a $\odot$ 14 shows up, however, a single pair of lethals will ensure the heterozygosity of the whole group of chromosomes. Since all the chromosomes of paternal origin in the circle go into the same germ cell, they all accompany any lethal present in one of these chromosomes and the whole set of chromosomes is prevented from existing in homozygous condition by this one lethal. A single pair of lethals, therefore, will ensure the heterozygosity of all the chromosomes. Thus, at the expense of only 50 percent reduction in fertility, the heterozygosity of all the chromosomes—consequently the maximum in hybrid vigor—is ensured.

But it will be argued that a loss of 50 percent in fertility is a serious loss, and so it is. It is questionable whether the hybrid vigor gained thereby would have been worth what it cost if it had not been for a third, seemingly unfortunate, mutation that occurred. This mutation was a reduction in length of the style, bringing the stigma down to the level of the anther. As a result, hours before the flower opens, the flower is self-pollinated and little opportunity is thus afforded for pollen from other sources to function. Self-pollination in general is a bad thing, for it tends to bring about homozygosity and eliminate hybrid vigor. In this case, however, its bad effect is prevented. The lethals will not allow homozygosity to occur. On the other hand, self-pollination has a positively good effect, since it ensures a much heavier pollination than would be likely if the plant were pollinated by insects, thus helping to overcome the sterilizing effect of the lethals. As a result of this fortunate combination of what would otherwise be unfortunate characters, both the lethals and self-pollination are prevented from having any harmful effect and are allowed to produce only their beneficial effects. The lethals ensure maximum hybrid vigor, and their sterilizing effect is largely balanced by the richness of pollination brought about by the self-pollinating habit. Self-pollination in turn cannot bring about reduction in hybrid vigor because the lethals prevent this action. The lethals furthermore are enabled to accomplish their desirable function only because of the presence of the large circles resulting from successive reciprocal translocations.

We thus have the unusual picture presented to us of three different kinds of alteration, each of which by itself might have proved a handicap in the struggle for existence. By developing a technique of interchange, however, which does not give rise to much sterility, and then combining the results of interchange with the presence of potentially deleterious lethals and self-pollination, the genus has achieved a combination of characters that has given it great survival value. It is interesting to note that the genus has spread from the southern tip of South America to the far reaches of northern Canada, and from the Pacific to the Atlantic, and that in general the sections of the genus that have ranged the farthest and are the most numerous are the ones with large circles, lethals, and self-pollination. Thus the genus presents us with a unique example of the way in which it is possible, in the evolutionary process, for apparently deleterious characters to combine in such a way that together they give to the plant increased survival value.

Space does not permit a deeper excursion into the intricacies of Oenothera cytogenetics. Enough has been said, however, to give some idea of the the degree to which Oenothera differs from ordinary organisms. Taking a number of untoward alterations in its hereditary make-up and combining them in such a way that they have increased rather than decreased the survival value of the plants possessing them, the evening primrose has developed a type of cytogenetic behavior and a kind of population structure that so far as known are quite without parallel.

It has been said that one should cherish his exceptions. If this is true, then Oenothera is especially to be prized. Originally regarded as one of the major puzzles among the higher organisms, it now stands forth as one of the most instructive exceptions to ordinary cytogenetic behavior and as one of the most interesting cases of unusual evolutionary development.

A SURVEY OF JAPANESE SCIENCE

HARRY C. KELLY

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A SCIENTIST'S first impression of Japan is of many technological developments copied from Western countries, and of Japanese scientists and technologists whose chief aim seems to be to get Western scientific and technical books to overcome the effects of their isolation during the war.

After several years of observation, this first impression still holds true. It also becomes evident that the Japanese are unquestionably the technical leaders of the Far East, that they have made world contributions to the advancement of mathematics, theoretical physics, and chemistry; that in fields indigenous to Japan, such as agriculture, fisheries, and sericulture, the Japanese show great skill in practical methods and the application of recent scientific results; and that in some subdivisions of these indigenous fields, Japan may lead the world.

The word "science" as used for organizational purposes in Japan includes all fields of learning, such as law and literature and the natural and social sciences. The modern forms of Western learning were superimposed on earlier Chinese art, literature, and medicine, so that, to the Japanese, scientific research means any kind of advanced intellectual activity.

The primary purpose of this account is to relate the changes made in the scientific structure in Japan during the present occupation—an occupation whose chief functions seem to be to make the occupation unnecessary, to remove war potential, and to allow Japan to achieve a stable economy in a democratic manner so that she can assume her responsibilities in promoting world peace. In a nation of about eighty million people, increasing at the rate of approximately one million per year in an area about that of California—a country that can raise only about 80 per cent of its food requirements and is short of natural resources—the responsibility is frightening indeed.

The occupation has adopted the sound policy that this responsibility is Japan's. Science and technology are considered essential to recovery, and fortunately many Japanese scientists and engineers recognize this responsibility and the necessity for reorganization of some of the national scientific bodies, which, because of a somewhat feudalistic influence, were incapable of attacking the new problems forced upon them. In general, the attitude of the occupation authorities is to ensure that the Japanese scientists have an opportunity of assuming their responsibilities, and that technical policies shall be determined by competent and representative scientists.

The three national bodies of science are the Imperial Academy of Science, the National Research Council, and the Japanese Society for the Promotion of Science. The reorganization of these bodies was one of the important problems facing the scientists. One of the problems was to devise a method whereby a body of the best scientific talent of the country, elected by competent fellow-scientists, could be used to guide and coordinate the development of science and technology in Japan and at the same time provide a sound democratic governmental structure. The Japanese have offered a solution in an elected body of scientists called the Science Council of Japan, and a governmental committee called the Scientific and Technical Administrative Commission.

The present Japanese culture began with the introduction of Chinese learning in the sixth century A.D. This exchange of learning was brought about by scholars, technicians, and Buddhist priests who came from China and Korea, and later by Japanese students who visited China. The introduction to Chinese culture had such a profound influence that it almost completely overshadowed the native culture. This was especially true in science and technology. The Japanese learned Chinese astronomy (calendar making), mathematics, medicine, sericulture, weaving, paper making, metallurgy, pottery, shipbuilding, architecture, surveying, etc.

The custom of sending envoys to China ended in A.D. 894. Most Japanese feel that this had no serious effect on developments in Japan. They believe that significant elements of the culture had already been assimilated and that China did not achieve anything outstanding during this period. In this early contact with Chinese culture, Japan was merely an imitator, but gradually her own characteristic culture began to emerge.

Early Japanese culture flourished in the eighth to the eleventh centuries; then came a 400-year dark age—the years of civil war. The intervals between wars were not long enough for any significant advancement. There were, however, some minor war-related technical developments, such as sword and armor making and castle building.

The arrival of the Portuguese in Tanegashima (a small island south of Kyushu) in 1543, with the introduction of the musket, is popularly regarded as the first encounter with Europeans. The Portuguese traders were followed by the missionary St. Francis Xavier in 1549. At that time Japan was still in a state of turbulence, but national peace was finally established in 1585. From 1592 to 1598, however, the country was at war with Korea. Japan lost the war, but again came into contact with Chinese and Korean culture—a culture that had made some advancement during the 700 years of Japan's isolation. The defeated army brought newly published Chinese books, as well as Chinese technicians, back to Japan. This again gave new impetus to the development of Japanese technology.

In the year 1603 the Tokugawa Shogunate came into power. This regime lasted until 1868, and in the 265 years of the Shogunate rule the Japanese enjoyed national peace. During this period, great advancements in science and technology were made.

At the time of the introduction of European culture into Japan in the sixteenth century some scientific and technical knowledge was passed on by Christian missionaries. The Japanese listened to the missionaries with great interest, and a few people from all classes were converted to the new creed. Later the government began to see danger in the propagation of Christianity, fearing that Spain and Portugal were using religion as a means of invasion, and Hideyoshi prohibited the teaching of Christianity. This policy was also followed and most strictly enforced by the Tokugawa government, finally resulting in Japan's closing its doors to all European books, including Chinese translations of such books. This prohibition was announced in 1630 and marked a new era of isolation from European culture. A limited amount of trading was allowed with the Dutch at the Port of Nagasaki. Even here caution was used, for the only contact with the traders was through interpreters (who were allowed to learn to speak, but not to read, Dutch).

As time went on, special features of European culture came to be more keenly appreciated from the material imported by the Dutch. On the advice of scholars of the period, Shogun Yoshimune in 1720 removed the ban on foreign books other than those dealing with religion. (This so-called period of "Dutch learning" lasted nearly 150 years, until the end of the Shogunate government.) Translations of Dutch books were soon published, books on medicine and astronomy far outnumbering other technical books. Very few if any books on physics and chemistry were translated, and, since facilities for experimentation were lacking, little progress was made along these lines.

Mathematics, however, seems to have been in a quite different category. Starting with the knowledge obtained from some Chinese books on mathematics, the Japanese made unique advancements in the seventeenth and eighteenth centuries. These studies comprised algebra, geometry, and calculus, and there is some evidence that a few of their independent discoveries predate the equivalents in European mathematics. Japanese mathematicians continued their isolated study even after the importation of European books in 1720 and for a short period after the Meiji Restoration in 1868. Mostly owing to the inferiority of Japanese notation, however, the Japanese finally replaced their methods with those from Europe. It is quite probable that the mental training the Japanese received in their development of mathematics was extremely valuable in understanding European science in succeeding years.

Just as in mathematics, there was some independent development in Japanese medicine. The introduction of Western medicine by the Dutch paved the way for the importation of other Western sciences. Japanese medicine was not investigated in the light of modern theory; the investigation would probably be most difficult, not only because of terminology but also because of the mode of analysis.

With the surrender of the Shogunate in 1868 to the Mikado, Japan fully opened her doors to foreigners, and Japanese people were allowed to go abroad. The Japanese put great effort into the study of European and American science and technology to overcome the limitations imposed by long periods of isolation. In the engineering

fields Japan is still in the imitating phase, but her accomplishments in the theoretical sciences of mathematics, theoretical physics, and chemistry, her work in sericulture and agriculture, indicate the contributions that the Orient can make to Western culture.

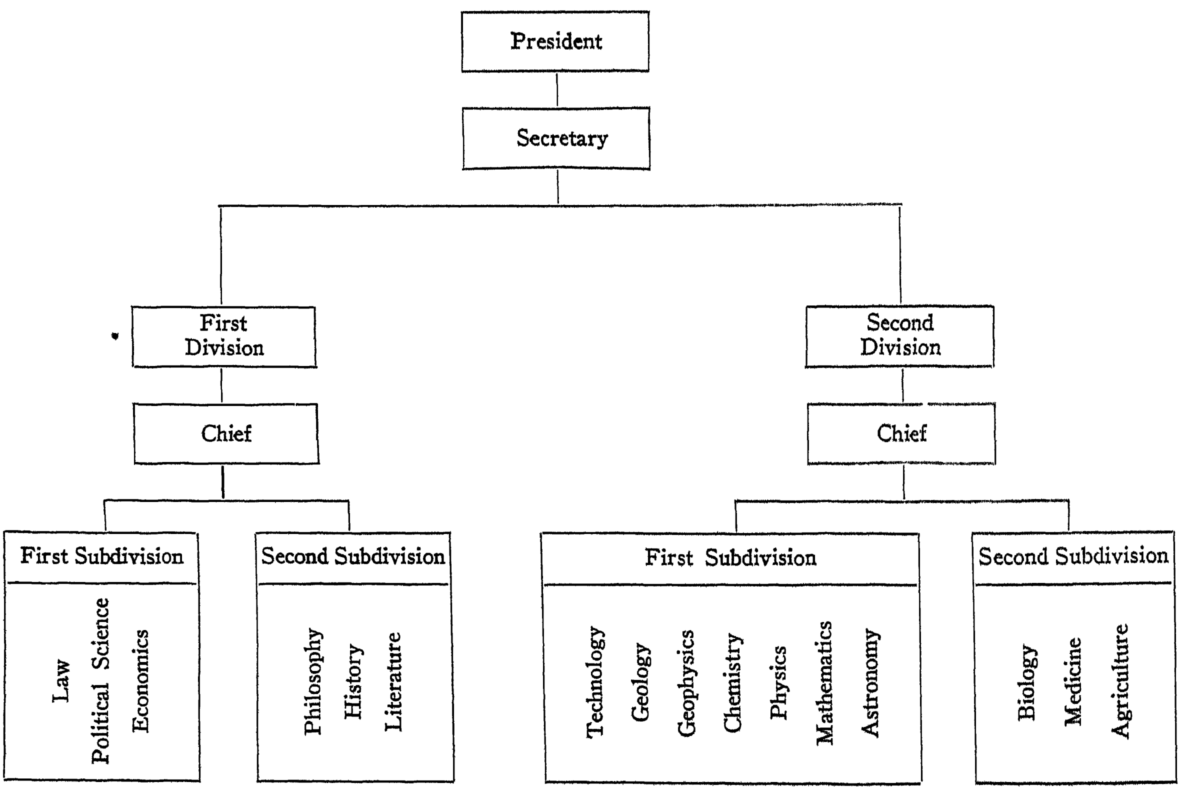
NATIONAL SCIENTIFIC SOCIETIES

The Tokyo Academy, the predecessor of the present Imperial Academy, was founded as a governmental organization in 1879 by the Minister of Education upon the suggestion of Dr. David Murray, an American adviser. The purpose of the new organization was to advise on educational matters and to promote arts and science. The maximum number of members was 40; the Minister of Education appointed the first 7, and the Academy was to select other members with the approval of the Minister. The Academy initiated *The Tokyo Academy Magazine* on its establishment.

In 1906 the Academy was reorganized, and the Imperial Academy of Science came into existence on promulgation of Imperial Ordinance No. 149. The statutes provided that the Academy was to be under the supervision of the Minister of Education and was to have for its aims the development of learning and the promotion of culture. It was a self-perpetuating body. The members were appointed by the Emperor from among men of learning, on the recommendation of the Academy. It was divided into two sections: the Cultural and Social Science Group and the Pure and Applied Natural Science Group (Table 1). The Academy could undertake research projects in cooperation with a foreign scientific organization upon approval of the Minister of Education, and it could become a member of such a foreign organization. The Academy was also allowed to elect distinguished foreign scientists as members. To date, however, only 5 foreign members have been elected, and none of these is living at present. The members of the Academy originally had the right of selection of four members to seats in the House of Peers. This right has now been abrogated under the new Constitution.

In June 1919, the Imperial Academy submitted a memorial to the Minister of Education advising the establishment of a National Research Council. This was created in 1920 by Imperial Ordi-

TABLE 1
JAPANESE IMPERIAL ACADEMY



nance No. 297 and was placed under the jurisdiction of the Minister of Education. It had for its aims the unification, promotion, and encouragement of scientific research and its applications. "Science," as shown in Table 2, included jurisprudence and literature. A section on aeronautical engineering was eliminated at the end of World War II, since a Far Eastern Commission directive prohibits civil and commercial aviation.

An ineffectiveness in the Academy and the Council in the accomplishment of their aims is evident, for in January 1931 one hundred influential people representing higher learned circles met at the Imperial Academy Building and resolved to start an earnest campaign to establish a more effective organization for the promotion of scientific research.

After many committee meetings, in May 1933, the Japanese Society for the Promotion of Science was inaugurated. The Society is a juridical person with the objectives of promoting scientific research and forwarding its practical application, thus contributing to the advancement of culture, the development of industry and national defense, and the enhancement of national prosperity and human welfare. Although a juridical person is not a governmental body, the Society to all intents and purposes behaved as though it were.

In addition to these three important national bodies, there are many professional societies, such as the Japanese Physical, Chemical, Biological Economic, Electrical Engineering, and Mechanical Engineering Societies. These, however, are nongovernmental in character and perform their functions of encouragement of particular fields by discussion and publications. No attempt has been made during the occupation to interfere with them; rather, encouragement has been given to form societies in other specialties, in the hope that the professional societies, whose membership was open to any qualified candidate, would form the nucleus of a more representative and effective national council of science.

SCIENTIFIC ORGANIZATIONS DURING THE WAR

During the war, Japanese science made no noteworthy successful or original contributions, but concentrated almost entirely on discovering substitutes or making minor improvements. This failure was probably due mainly to three factors: distrust by the military of scientists who had been trained or had traveled abroad (these scientists are among Japan's best); poor coordination among the scientists themselves, and the nearly complete lack of coordination between the Japanese Army and Navy, which amounted almost to antagonism; and, probably the most important, the overwhelming might of the scientific and technical developments of the Allies, which had a most demoralizing effect on Japanese research.

The Japanese had very little contact with German science during the war. A few instruments, such as radar, ultrasonic, and infrared apparatus were introduced, but these were not the latest models and some were of prewar origin. There was some preliminary research on such items as proximity fuses, homing bombs, and jet propulsion, but because of poor technique and organizational difficulties not much progress was made. The amount of pure research, however, especially in mathematics and theoretical physics and chemistry, which went on during the war, is surprising. As an example, the theoretical work in nuclear physics kept very nearly abreast of developments in the rest of the world.

The predominant active organ during the war was the Board of Technology, which was under the direct control of the Premier. The Board was a kind of centralized administrative planning organization and placed particular emphasis on aeronautical research. Under the Board, the Council for the Mobilization of Science functioned as an operating agency and assigned research projects to compulsorily nominated personnel. Assignments for volunteers were made through the Japan Society of Technologists. Encouragement for inventive effort was provided by the Imperial Association for the Promotion of Invention. Organizations such as the Science Mobilization Association, the All-Japan Union of Learned Societies, the Japan Society for Aeronautical Techniques, and the National Research Council entered into activities to further the policies directed by the Board of Technology.

Many smaller organizations and committees were formed, such as the Science Neighborhood Groups. The Japanese natural tendency toward theoretical interest and discussions led them to attempt the solution of many problems in committee rather than in the laboratory.

In the encouragement of reorganization of the national scientific bodies by the occupation authorities, this weakness in war potential of the old organizations was not taken lightly. On one hand was the policy that science and technology were to be encouraged, since they were considered essential to economic recovery; on the other, a powerful governmental scientific body might well

serve as the genesis of a new technical General Staff with warlike aims. It was finally decided that if, under the new Constitution, an elected nongovernmental council of scientists were formed, it would be most likely to accept its responsibilities in promoting world peace.

RESEARCH DURING THE OCCUPATION

Research was very nearly at a standstill at the beginning of the occupation. The most important factors were the pressing problem of food and the welfare of the scientists' families. In addition, there was the uncertainty among the Japanese as to what research was to be allowed and what laboratories were to be affected by reparations and deconcentration of the Zaibatsu companies.

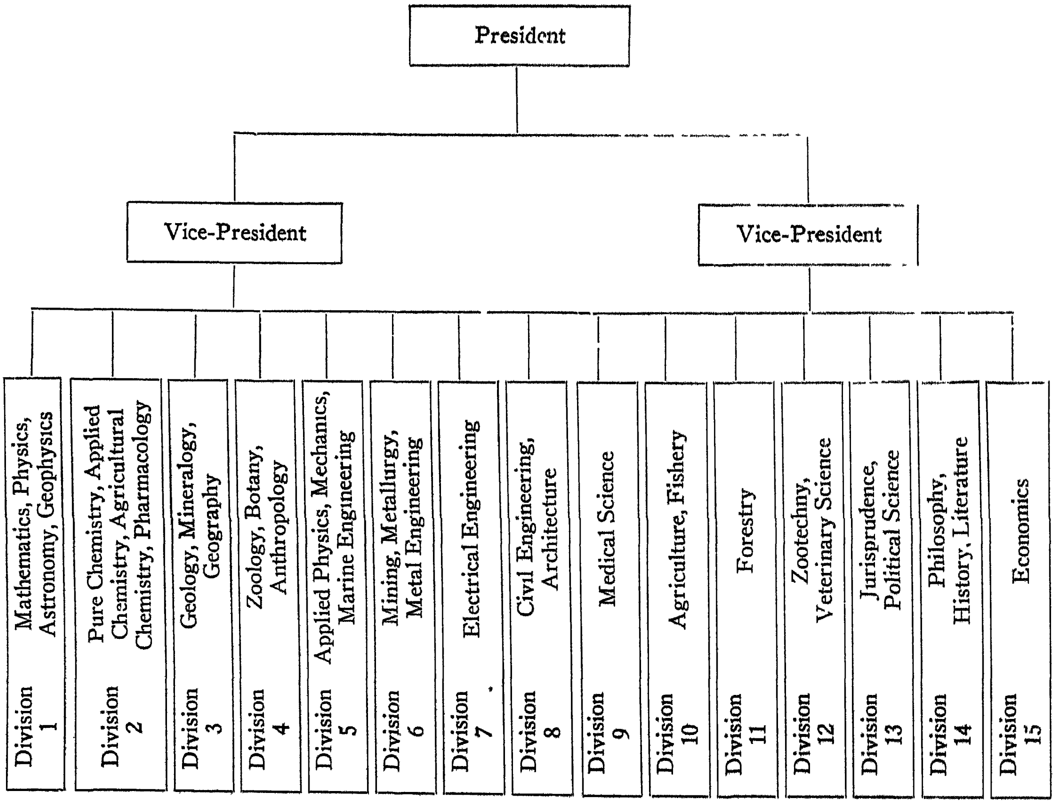
Compared with the rest of Japan, the laboratories survived the bombings fairly well. Most damage was inflicted on industrial laboratories, although a few universities also suffered severely. The laboratories at the University of Literature and Science in Hiroshima and Nagoya University were probably the most severely damaged university laboratories. The universities at Osaka, Sendai, and Waseda and Keio in Tokyo sustained some damage to their laboratories, but many others, such as the Imperial Universities of Tokyo, Kyoto, Kyushu, and Hokkaido, suffered no destruction.

The invading troops did some damage to scientific apparatus, but, considering the circumstances, the damage was not great. Much publicity has been given to the destruction of the four cyclotrons in Japan, but this destruction probably worked in the best interests of Japanese scientists, for it drew attention to the problem of the place of science in society and the necessity for adopting a just attitude toward it.

The first several months of the occupation were taken up with surveillance of Japanese laboratories and a kind of mutual taking of measure with the scientists. After this initial period it became evident that rather than discourage research it was necessary to encourage the scientists to resume their activities, especially where these involved the technical problems associated with food, clothing, shelter, health, and export. It became a policy of the occupation that science and technology were necessary to the economic recovery of Japan, and

TABLE 2

THE NATIONAL RESEARCH COUNCIL



Japanese scientists were encouraged to assume these responsibilities.

The limitations on research were based on the Potsdam Declaration and Far Eastern Commission directives. The Japanese were informed that research and teaching for the extension of scientific and technical knowledge would be permitted except where directed toward warlike activities. In addition to such obvious warlike developments as guided missiles, prohibitions included various phases of nuclear research and aeronautics.

The chief prohibition in nuclear research is the mass separation of radioactive substances. The economic condition of Japan is a very effective limitation on research in this direction. In aeronautics, research and organized instruction directed toward the manufacture, design, or operation of any aircraft or devices specifically designed for aircraft are prohibited. This does not prevent the usual studies in meteorology or the normal teaching and research in aerodynamics not specifically applied to aircraft.

The cooperation of the Japanese scientists with the occupation authorities has always been most remarkable. A few scientists have been purged for political or military activities, but none because of a purely scientific activity.

The Japanese have been encouraged in their desire to enter into their normal activities in publication. A group of them have already abstracted about 5,000 of their papers in physics, chemistry, biology, and engineering published during the war, and the abstracts have been forwarded to the appropriate journals abroad. In addition, current research papers are now appearing in foreign journals. The importation of scientific journals is encouraged, but because of monetary exchange difficulties this is somewhat handicapped. Some journals have arrived as gifts, however, and organizations such as the American Institute of Physics and the American Association for the Advancement of Science have given free permission for the translation and reproduction of their publications until other arangements can be made.

There are about 25,000 natural scientists and engineers in Japan today, distributed approximately according to Figure 1. Using the Japanese definition of science, there are approximately 90,000 scientists. An indication of the interest of younger students is given in Figure 2, which shows the approximate distribution among the different fields of science.

SCIENTIFIC LIAISON GROUPS

A very limited technical staff among the occupation authorities soon made it apparent that a better system of liaison was necessary for a broader contact with scientists and their laboratories. In June 1946 a group called the Japanese Association for Scientific Liaison was formed to give at least geographic representation to the authorities in Tokyo. It was the function of this group to gather specific information as required, to help interpret occupation directives to the Japanese, and, by more intimate contact, to bring Japanese problems to the attention of the occupation. Thus, it was designed to be of mutual benefit to the occupation authorities and the Japanese scientists.

This Scientific Liaison Group worked so successfully that the scientists in other fields asked permission to form similar groups. As a result, liaison groups were formed in agriculture, medicine, and engineering. After several months' successful operation of these groups, they asked that they be made more truly representative of Japanese scientists. The groups had attracted the interest of the more active young scientists, who were restricted in expressing their opinions by the older established organizations.

In the meantime, the ineffectivness of the older organizations was freely discussed. The amount of cooperation among the three national bodies was surprisingly small. Considerable duplication, as well as neglect in attending to urgent activities essential to the rehabilitation of Japan, had been noted. The fact that an individual belonged to two, and sometimes three, of these organizations seemed to make little effective contribution toward improving coordination. The active operations of the Liaison Groups and the reticence of the old established bodies soon made it obvious that the roles of the existing bodies would have to be clarified before real progress was possible. The Liaison Groups were asked for advice on the subject.

After the introduction of the question of the desirability of reorganization of the existing national bodies of science to the Liaison Groups, there was a great deal of criticism of the existing bodies expressed by individuals of the Groups. They especially criticized the feudalistic attitude of the Imperial Academy and its influence on the National Research Council. It seemed to be the general consensus that, although most of the members of the Academy had made real contributions in the past, they were now all too old to be capable of real activity and prevented the younger scientists from expressing their opinions. These were only opinions from individuals, however, and the Liaison Groups would give no formal

opinion, since they did not consider themselves a representative enough group to make a formal plan. At that time there was talk of a possible peace treaty, and the Groups probably were afraid of losing the support of the occupation.

During this same period two other developments took place. A bill was successfully engineered through the Japanese Diet for the reorganization of science and the concentration of its direction and control. Because the bill would emphasize the faults of the existing bodies and because this surprise move met so much opposition from the Liaison Groups and the occupation, it was disapproved Somewhat later, the Imperial Academy approved an occupation plan for abolishing the National Research Council and placing most of its functions under the Japanese Association for the Advancement of Science. This also came as a surprise to everyone, for the Research Council appeared to be the most effective of the three organizations, and the plan had been approved in complete ignorance of most of the members of the Council and the Liaison Groups. The Academy in turn appeared surprised, for it understood from occupation authorities that reorganization plans were to be initiated and formulated by the Japanese themselves. It is not known whether greater understanding was obtained from the reply that the original statement was correctly quoted, but that it was not certain that the Academy truly represented the Japanese scientists.

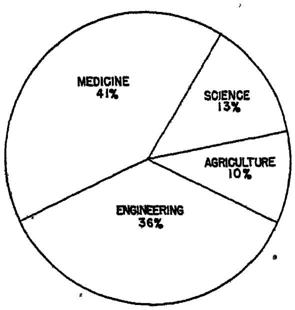
As a result of this activity, the Liaison Groups, the Imperial Academy, the National Research Council, and the Ministry of Education were requested to form a representative body of scientists to formulate plans for the reorganization of the existing scientific bodies. The first step was the formation of Sewanin Kai, a kind of qualifying committee to pass on the qualifications of electors to a planning committee.

In the meantime, the U. S. National Academy of Sciences was invited to send a small group to Japan to advise the occupation authorities on the attitude they should take toward any plans proposed by the Japanese. This advisory group was present in Japan on the inauguration of the "Renewal Committee," a kind of charter or planning group that was to make proposals for new national scientific organizations in Japan. Because of strong criticism from districts outside Tokyo, the new organization was to have geographical representation as well as representation from different scientific fields. Further, the new organization was to reflect a more representative opinion of Japanese scientists, to be adaptable enough to meet the technical problems facing Japan, and to have its charter operate within the new Constitution of Japan.

THE RENEWAL COMMITTEE

The qualifying committee chose the general plan of using an electorate composed of all scientific societies having 500 or more members. Some fifty of these societies existed. There were to be 108 members elected to the Planning Committee, or, as the Japanese termed it, the "Renewal Committee." Each of the seven traditional "faculties" of a full-fledged university—law, economics, literature, engineering, agriculture, science, and medicine—were to be represented by 15 members each. In addition, 3 members were to represent the Society of the Science of History of Japan, the Democratic Scientists' Association, and the Association of Democratic Scientists.

The scientists expressed deep interest and concern in the method and outcome of the election of the Renewal Committee. There were many difficulties in defining eligibility to vote, and shortages of time, paper, and funds for stamps and envelopes to mail ballots. However, it was generally agreed that the selection of the Renewal Committee was



NUMBER OF SCIENTISTS

SCIENCE	ENGINEERING	AGRICULTURE	MEDICINE	TOTAL
3266	9043	2542	10229	25080

Fig. 1. Statistics showing number of scientists and engineers in Japan. (From Ministry of Education Scientific Education Bureau, March 20, 1947.)

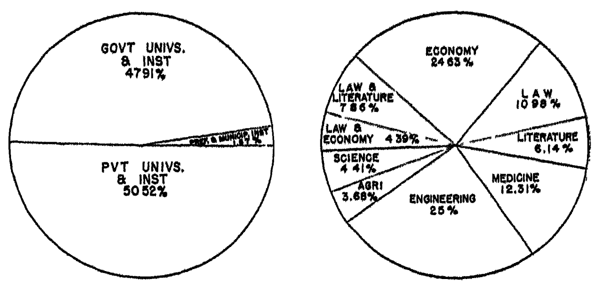


Fig. 2. Number of graduates from universities and institutes of Japan in 1946. *Left*, number of graduates classified by type of institute; *right*, number of graduates classified by faculty.

made in the best way possible under the circumstances. There were some criticisms of the outcome of the election. These were substantially as follows. (*a*) that the preponderance of representation was from Tokyo Imperial University (just over half); (*b*) that the broader Tokyo area had too many members (just over three fourths); and (*c*) that private universities had insufficient representation (less than 10 members).

Despite this outcome, however, the Renewal Committee represented a genuine break with tradition. Of 108 members, only 6 came from the Imperial Academy, only about one half from the large National Research Council (12 of these being recently elected members), and the average age is less than fifty years. It must also be said that preponderance of representation of the Tokyo group rested in some measure on relative quality and not solely upon prestige and influence.

The Renewal Committee keenly felt the criticisms and at one point considered dissolving itself because of them. It was finally concluded that sensitivity to this criticism was a good omen, and the Committee was encouraged to tackle the problem.

The Renewal Committee was inaugurated on August 25, 1947, at the official residence of the Prime Minister in the presence of the Prime Minister, the Japanese Cabinet, the Advisory Group from the United States National Academy, and the heads of the technical sections of General Headquarters of the Supreme Commander for the Allied Powers. The Prime Minister gave assurance that the recommendations of the Renewal Committee would receive a sympathetic hearing by the Japanese government. The Advisory Group expressed confidence in the Renewal Committee, indicated the importance of its task, and signified that it considered the Renewal Committee the most representative that could be had under the circumstances. The members of General MacArthur's Headquarters gave assurance that the problem of reorganization of the national bodies of science was the problem of the Renewal Committee, and that the main interest of SCAP would be to see that recommended organizations were composed of a representative group of scientists, capable of attacking the pressing technical problems facing Japan, and in so doing to preserve sound governmental procedure under the new Constitution in Japan.

Immediately after its inauguration, the Renewal Committee began accepting plans for consideration from the Science, Engineering, Agriculture, and Medical Liaison Groups, the Imperial Academy, the National Research Council, private universities, and union groups. The Committee held eight formal meetings in addition to numerous meetings of subcommittees. During these meetings the proposals made by all interested groups were considered. The meetings were not attended by occupation personnel except on specific invitation to discuss a specific point.

At the conclusion of the eighth general meeting, a draft of a bill for a new Science Council was prepared and suggestions for a technical advisory group to the cabinet, along with a report of the activities of the Renewal Committee, were presented to the Prime Minister.

The recommendation of the Renewal Committee for a new Science Council was passed by the Diet on July 5, 1948. The proposal for a governmental body called the Scientific and Technical Administration Commission was withheld for further consideration pending passage of the Science Council Bill.

THE SCIENCE COUNCIL OF JAPAN

The present National Research Council is to be dissolved, the Japanese Society for the Promotion of Science is to become solely a private and independent body that can receive money from the government only for possible specific contracts. The Imperial Academy of Science is to become solely an honorary body whose membership shall be determined by the new Council.

The Science Council will have 210 members, composed of 30 each from the seven traditional "sciences" of Japan. The members are to be chosen by free election by vote of the scientists of Japan.

The qualifications for the voters appear quite liberal. Graduation from a university or college with three years' postgraduate experience, or a letter of recommendation from one of the professional societies or from a reputable research institution are examples of qualifications required. The Renewal Committee estimates that with the criteria they have chosen, there will be approximately 90,000 eligible voters. There is still some objection to the method of election, from groups with special interests. Their chief complaint appears to be that their organizations are not properly represented. The Renewal Committee, however, insists that scientists who are members of these special groups will receive appropriate representation through the vote of the individual scientists.

For the election, to be held every three years,

Japan is divided into seven districts, each district to have representatives in the seven fields of science; this will give 49 members of the Council who will have been elected on a regional basis in order that geographic representation be obtained. The remaining 161 members are to be members at large elected by a nation-wide vote. The purpose of the latter is that the outstanding men of science can be elected and that they will not be handicapped because they live in a dense population of scientists.

The functions of the Science Council of Japan are given in the *Preamble* and Chapters I and II of law No. 121 of 1948, which follow:

The Science Council of Japan shall hereby be established, on the conviction that science provides the basis of a cultural country and with a view to fulfilling its mission of contributing by the joint will of the scientists throughout Japan to the peaceful rehabilitation of this country and promotion of the welfare of human society as well as to the advancement of science of the world, in cooperative relations with academic societies of foreign countries.

Chapter I

Establishment and Aim

Article 1. The Science Council of Japan shall be established by this Law which shall be called the Science Council of Japan Law.

All transactions of the Science Council of Japan with the Government shall be through the Prime Minister.

Expenditures of the Science Council of Japan shall be defrayed from the National Treasury.

Article 2. The Science Council of Japan shall aim at promoting the development of science and permeate it into administration, industry and the nation's life as the representative organ, internal and international, of the scientists of this country.

Chapter II

Functions and Powers

Article 3. The Science Council of Japan shall perform the following independently:

a. To discuss important matters concerning science, and to make exertion for the realization thereof.

b. To coordinate scientific researches for the enhancement of their efficiency.

Article 4. The Government may seek the opinions of the Science Council of Japan on the following:

a. Compilation of budget for and distribution of the government grants and subsidies in order to help scientific researches and experiments or to promote science in general.

b. The policies on the compilation of budgets concerning the expenditures of the institutes, laboratories under the jurisdiction of Government, and of commissioned researches.

c. Important measures particularly requiring deliberations by expert scientists.

d. Other matters recognized as proper to be referred to the Science Council of Japan for deliberation.

Article 5. The Science Council of Japan shall place itself available to the Government for recommendations on the following:

a. The schemes for promotion of science and advancement of technology.

b Measures for the utilization of the results of scientific researches.

c. The schemes concerning the training of scientific researchers.

d. Reflection of science on administration.

e. Permeation of science into national life and industry.

f. Other matters necessary for the fulfillment of the aims of the Science Council of Japan.

Article 6. The Government on request from the Science Council of Japan may submit data or explanations or set forth their views.

There are seven chapters in the law, but the first two are sufficient for present purposes. The Renewal Committee would prefer that the Science Council be financially independent of the government, but under the present conditions in Japan this is impossible.

THE SCIENTIFIC AND TECHNICAL ADMINISTRATION COMMISSION

Although the Science Council is to be democratically elected and is to represent all fields of scientific activity and all regions of Japan, it is nonetheless elected by a restricted electorate and therefore cannot impose its will on the government. Its control must come from its prestige and intelligent advice.

In order that the government can appropriately consider and implement the proposals of the Science Council, the Japanese Renewal Committee has recommended to the government that a Scientific and Technical Commission be set up in the Prime Minister's office.

The Commission is to be appointed by, and be under the jurisdiction of, the Prime Minister. The maximum number of members is to be twenty-four, at least half of whom shall be persons of scholarly attainment and experience. In the appointment of the latter class the recommendations of the Science Council shall be respected.

The functions of the Commission are to deliberate and make recommendations on the following matters:

a. Measures necessary for the administration of the reports or recommendations made by the Science Council of Japan.

b. Selection of matters which are to be referred to the Science Council of Japan for consultation by the government.

c. Method of execution of international enterprise concerning science and technology which should be carried out by the government.

d. Liaison and coordination of matters relative to science and technology that are under the jurisdiction of the government administrative agencies.

A proposed bill for the establishment of the Commission is now (November 1948) under consideration by the Japanese Diet.

The future holds great responsibility for Japanese scientists and engineers. With a large and increasing population density, limited resources, and higher labor costs than those of the old feudalistic system, the Japanese technologists hope they have a system, at least, which will allow them to assume their new responsibilities in a flexible and effective manner.

This survey has been an attempt to show at least the outward changes in the scientific structure during the occupation of Japan. It probably would be of greater interest—and difficulty—to describe the changes made in United States scientists who, because of the war, have a particular interest in Japan and Japanese scientists. Actions such as that of the United States National Academy in sending two groups of its most eminent scientists to advise the Supreme Commander for the Allied Powers on scientific policies; permission for translation and reproduction of articles from the journals of such organizations as the American Association for the Advancement of Science and the American Institute of Physics; and the invitation to Japanese scholars from such institutions as the Institute of Advanced Study, demonstrate that the scientists of the United States recognize their new international responsibilities.

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AMERICA'S MYTHICAL SNAKES

CLIFFORD B. MOORE

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FOR choice items of folklore in great number and variety, the subject of snakes has everything to offer. Ever since the curse was officially placed on serpents in the Garden of Eden, their reputation has suffered, and doubtful stories about them have increased to such an extent that a very large book indeed would be required to record all the strange habits and remarkable abilities they are purported to possess, together with the inevitable eyewitness testimony.

Probably foremost in the gallery of America's mythical snakes is the serpent that is supposed to take its tail in its mouth and roll about like a hoop. This unique creature is said to have a poisonous sting in its tail, which is launched at its enemy from a rolling position.

There appears to be no classical or European analogue of the American hoop-snake story. Our subject is first encountered in colonial days, and one naturalist of the period described a "water viper" he encountered as having a tail ending in a blunt, horny point about half an inch long.

> This harmless little Thing [he says] hath given a dreadful Character to its Owner, attributing to him another Instrument of Destruction besides that he had before, imposing a belief on the Credulous, that he is the terrible Horn-Snake armed with Death at both Ends, tho' in reality of equal Truth with that of the two-headed Amphisboena; yet we are told, that this fatal Horn by a Jerk of the Tail, not only mortally wounds Men and other Animals, but if by Chance struck into a young Tree whose Bark is more easily penetrable than in an old one, the Tree instantly withers, turns black and dies.

In North Carolina the horn snake that could conveniently roll like a hoop was said to hiss like a goose and to kill its victims with its horny tail. Certain professed witnesses reported "that a small Locust-Tree, about the Thickness of a Man's Arm, being struck by one of these Snakes, at Ten a clock in the Morning, then verdant and flourishing, at four in the Afternoon was dead, and the Leaves red and wither'd." In Pennsylvania the death of a tree from horn-snake venom required twenty-four hours, in New Jersey two days. It was observed in Virginia that the horn snake upon striking its tail into a musket butt could not disengage itself.

Not all the good hoop-snake stories come from colonial sources. The following somewhat modern tall story, not meant to be taken literally, as the colonial stories are, is based upon the fundamental premise that the hoop snake really exists. I have heard it repeated with minor variations or with added color, depending on the section of the country and the imagination of the individual narrator.

> A youth who lived in Oklahoma had a sweetheart on a ranch several hundred miles to the south, on the Rio Grande. Young women are few and far between in the great open spaces of the Southwest—at least pretty girls like Nell. Our hero, who rode a bicycle and whose name was Tom, had a rich rival who sped over the wastelands on a motorcycle.
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> One day Tom was pedaling in the scrub country when his rival sped by on his motorcycle. Before the latter disappeared in a cloud of dust, he tauntingly sang out that he was headed for Nell's to propose to her that very evening. Our hero turned his bike about forthwith and headed for the Rio Grande too—with a wild hope that he might beat his rival (who could, of course, run out of gas many miles from nowhere) and so be the first to propose to Nell. An hour of hard pumping on the bicycle at top speed of fifteen miles per hour convinced Tom he would never reach the Rio Grande before dark and that Nell, in all probability, would be lost. As these thoughts gloomily crowded through his mind, there was a sharp report and a hiss of air. A cactus spine had penetrated the front tire of the bike!
>
> Now thoroughly disconsolate, Tom abandoned his two-wheeled conveyance and sought the shade of a near-by yucca. As he conjured up dismal images of Nell and his rival, he observed a pair of hoop snakes rolling about and playing tag with each other in the brush. Inspired, Tom ripped the rubber tires from his bicycle, captured the hoop snakes, mounted them on the wheels, and again pedaled south—to Nell and paradise!
>
> Being a bit thicker than the original tires, the hoop snakes rubbed against the frame of the bike as the wheels spun around. Quite naturally this irritated and excited the energetic serpents, so they increased the speed of Tom's bicycle to 40 miles per hour. Suddenly running over the sharp spines of a horned toad (*Phrynosoma cornutum,* of course), half buried in the sand, the hoop snakes were really shocked into action and in a wild and glorious burst of speed sent Tom and his bike forward at the un-

heard-of speed of 250 miles per hour. Soon our hero observed his rival's motorcycle some distance ahead and, in a fraction of a minute, passed it in a cloud of dust. In another half an hour the Rio Grande and Nell's ranch came into view.

It was by now quite impossible to control the speed of the infuriated hoopsters, and Tom saw he would have to resort to the spectacular if he didn't want to visit the interior of Mexico. With great presence of mind he steered directly under a grape arbor beside Nell's front door and managed to reach up and catch the main trunk of the vine in his hands. The snake-propelled vehicle continued southward and in no time at all was in Mexico City. It took Tom a good five minutes to stop whirling round and round the grapevine stem, so great was the speed at which he had caught it. On regaining his equilibrium he promptly proposed to Nell, and, as in all true romances, the two lovers lived happily ever after.

Another hoop-snake story (which might well prove the end of all hoop-snake stories) comes from Karl P. Schmidt:

A legendary boyhood friend of the distinguished Chicago zoologist liked nothing better than rambling through the woods, and on one of these frequent strolls his attention was drawn to a stately pine whose needles were becoming brown and withered in death. Having observed this same tree in the full vigor of life only a few days before, he was naturally curious to learn the reason for its sudden demise. Upon reaching the tree he was astounded to find a large hoop snake with its poisonous caudal spine so firmly embedded in the trunk that the serpent could not extricate itself.

His youthful heart filled with pity at the unfortunate hoopster's predicament, and to prevent it from slowly starving, our hero liberated the snake and went his way. The hoop snake was so grateful for this kindness that it followed its benefactor wherever he went from that time on. Naturally, a busy farm lad could not devote much time to capturing rodents and other animal food for his pet and constant companion: he therefore trained it to become a vegetarian, with mashed potatoes as its principal food.

This happy association of snake and boy persisted for some months, but, alas, an unhappy event occurred! One day as the snake was rolling rapidly downhill behind its young master, tail in mouth, as is customary with all hoop snakes, the serpent struck a stone, and the shock of the collision was such that its caudal spine scratched the roof of its mouth, causing it to bleed. Going completely mad at this sudden taste of its own blood, the hoop snake reverted to its carnivorous diet and devoured itself tail-first, before the horrified boy's very eyes.

When we speak of the hoop snake, horn snake, or stinging snake, a single species, *Farancia abacura,* is implied. The circular position of the snake lying prone and engulfing its prey suggests a "hoop;" the tail spine suggests a "horn;" the sharp end of the spine prodding the human hand suggests a deadly "stinger." But here are the essential facts. No snake known to science is capable of rolling like a hoop or of hurling itself bodily against a tree. Besides, no snake is possessed of a caudal stinger. Certain insects and all scorpions have tail stingers, but snakes do not. The business and offensive end of a snake is always its head and never its tail.

Farancia is a large, brilliantly colored, harmless serpent of the South. Because this snake prefers swamps, muddy areas, and the edges of ponds, it has been given the common name of "mud snake." The hard, hornlike tail spine provided by nature is believed by the layman to be a "stinger," and in certain parts of the South the stinging snake is more feared than the rattlesnake or the cottonmouth moccasin.

Various suggestions have been offered to help explain the function of the tail spine, but none of these has been based upon extended observation. Since many specimens prod the hand with the tail spine (young specimens with sharp spines sometimes draw blood), the idea has developed that the snake is simply protecting itself. Another suggestion is that the tail spine is driven into the ground when the snake is struggling with *Amphiuma* or dragging this amphibian prey out of a hole. One writer states that "it probably functions during burrowing." George P. Meade, a leading authority on *Farancia,* has witnessed no such actions as the foregoing, although he has kept and studied the snake in question over long periods of time.

It is always a source of surprise to those who examine a mature horn snake to discover that the famed horn, or sting, is actually no sharper than a blunt pencil point although, of course, that of smaller individuals is generally sharp. This difference appears to be significant, since Meade and others have repeatedly observed that the younger individuals utilize the sharp spine as a

goad when the amphibian prey bites and holds onto the snake. Under these circumstances, particularly when seized near the head, the snake stabs the victim so sharply with the spine as to cause it to release its hold. Blood is frequently drawn, and long, deep scratches are inflicted on the soft body of the amphibian.

It is generally agreed among herpetologists that the hoop-snake myth is almost invariably identified with the mud snake, and it is likewise felt that the possession of the tail spine is in some way related to the hoop snake. A possible explanation in the mind of Ditmars is the "habit of *Farancia* of occasionally lying in a loose coil . . . almost forming a circle" and having the appearance of "a discarded bicycle tire." But, as Meade points out, a much more definite basis for the hoop-snake story may be seen while *Farancia* feeds upon *Amphiuma.* The larger and mature snakes have

a tendency to rotate on their longitudinal axis as they grasp either head or tail of their victim and start to swallow it.

Wherever members of the widely distributed whip snake (*Coluber*) group are to be found, there is likewise to be found the story that these serpents are addicted to the infamous practice of wrapping themselves about people and thrashing them. In certain versions of the story the human victim is said to have been whipped to death!

Whipping or flagellation by snakes is a physical impossibility, and, furthermore, whip snakes are not constrictors, and they cannot hold people prisoners. It is true that the form and coloration of the scales on the tail of the snake, particularly the Eastern coachwhip, do suggest the appearance of a braided whip, from which it derives its common name. But the real basis for the whip-snake belief lies without doubt in the serpent's defensive behavior when confronted by a member of the human race. Thus, when cornered and molested, when prodded with a long stick, this snake with a characteristic display of bravado elevates its tail to an upright position and nervously vibrates it like its close relative, the blacksnake racer. Captive specimens, when held by the neck, almost invariably shake the body violently. The illusion is then complete.

Lawson, in his work *A New Voyage to Carolina* (London: 1709), describes a whip snake thrashing a rattlesnake. The only element of truth in the account is the fact that whip snakes do occasionally attack and devour rattlesnakes: but there is no thrashing or whipping in the process.

The whip snakes, in common with the racers, are members of what is probably the fastest-moving genus of snakes in existence. As told in the legend that has developed around the whip-snake group, a person must run extraordinarily fast and according to Olympic standards if he is to escape being caught and whipped. Why the whip snake desires to torment human beings, and what happens to them upon death, has never been made clear in the mythology of the subject. Certain it is that adult-sized whip snakes measuring 3.5–4 feet in length and no larger in circumference than a woman's wrist, could not devour a human being, and no authentic instances have been recorded of alleged lethal attacks.

Some of the stories about the whip-snake draw largely for their color upon the details of the chase, of assuming that the serpent has a disposition to relentlessly pursue the object of his quarry. A. C. Stimson claims, in an article that appeared in the *Antivenin Bulletin,* to have been followed by a whip snake.

> It is true [he writes] that a Coach Whip will, on rare occasions, follow an unaccustomed sight—for instance, I was followed for probably a quarter of a mile by an unusually large snake of this species. This happened in a small prairie that was surrounded for a radius of about a mile with a semi-tropical thicket. I noticed the snake just as I left the foliage. With his head, which angled about ninety degrees from his neck, reared about two feet from the ground he was calmly watching my every motion. When our respective curiosity was satisfied, I continued my tramp. After a few hundred feet I paused for some trivial reason and was surprised to see what I then thought was another snake in the same identical posture as the other one which I had just left. While it is nothing very uncommon to run across a Coach Whip on the Texas prairie, I had never before seen two such large ones in as short a distance. This time I took a few steps in its direction, and with the speed of a rabbit and the smoothness of running water it poured itself into a scrubby bunch of myrtle, but only when I had approached to within a few feet.
>
> I then walked toward my destination, but watched back for the snake to again erect itself. Imagine my surprise to see, instead, the grasses (about a foot high) being disturbed with that waving motion that only a snake in rapid transit can make, and that disturbance headed directly toward me. I then knew that I was being chased, by the terrible lash which, according to tradition, would soon overtake me, wrap itself about my body and thrash me with its tail until I died in terrible agony; and that, still upholding tradition, I should try my utmost to reach the nearest tree for salvation. . . .

The popular notion that snakes can outdistance human beings on open ground (which would include prairies) has been disproved by herpetologists. In controlled trials, Dr. Walter Mosauer found the fastest speed by any of his subjects to be 3.6 miles per hour. The speediest subject was, of course, a member of the *Coluber* group, which includes the so-called racers.

Certain snakes of the Old World have long been credited with guiding other snakes away from danger, and it is reasonable to assume that early settlers in America were quick to transfer this imaginary ability to our native snakes. At any rate, the pilot blacksnake (*Elaphe obsoleta obsoleta*) is our most famous snake pilot, and, since it inhabits the same rocky hillsides and ledges as the banded rattlesnake and copperhead, it has been given the distinction of piloting these venomous serpents away from danger—hence its common name. The pilot backsnake, also called the mountain blacksnake, is frequently confused with the common blacksnake (*Coluber constrictor constrictor*), from which it differs in possessing keeled scales and a highly polished appearance; in the true blacksnake the scales are smooth and have a satiny luster. The

pilot blacksnake, moreover, is a powerful constrictor, whereas the common blacksnake, despite its scientific name, has no constricting ability. Among the venomous snakes, the copperhead is popularly supposed to have guiding or piloting abilities and is said to serve the rattlesnake in such a capacity. Thus, the belief goes, whenever one sees a copperhead, a rattlesnake may appear on its trail.

Some of the most bizarre and curious snake stories center around those creatures of retiring and secretive habits. This is only natural. In the case of the "glass," or "joint," snake we have, in the popular mind, a serpent which, like no other animal, has the ability to break up its body into small pieces, reassemble itself at its convenience, and resume a normal existence.

From a colonial traveler and writer we learn that the brimstone snakes of North Carolina, being brittle as glass, were easily broken; but, according to a North Carolina doctor, "several in these parts confidently affirm, that if they remain in the same place untouch'd, they will joyn together again."

The traditional American glass snake is not, however, a true snake but rather the legless lizard *Ophisaurus ventralis*. This insect-eating lizard of the family Anguidae, no longer possesses true functioning legs since, owing to its burrowing activities in loose soil and under decaying tree bark, it has no further use for them. It is a fact that the legs of numerous burrowing lizards have degenerated to the point where they are merely useless flaps of skin lying along the body, completely incapable of aiding the creature in locomotion—or they have disappeared altogether. The glass snake, or lizard, does bear certain superficial resemblances to a true snake, but upon close examination it can be readily distinguished from a snake by the presence of well-developed eyelids and ear openings. In lieu of the broad crawling scutes of a snake, the abdomen is provided with many rows of smooth and overlapping scales, which are of no practical value in locomotion. The upper surface of the body is of a glassy smoothness, as one of the lizard's common names suggests.

When pursued by an enemy such as a king snake or some mammal, the lizard attempts to slip away. But, since its enemy can travel much faster, the lizard would be in great danger of being captured and killed save for an apparently wise provision of nature. Thus, when the pursuing king snake overtakes its prey, the lizard can, with a sudden twist, cause its tail to snap completely off at a special breakage plane. Sometimes the fragile appendage will, upon a light blow or two, shatter into several pieces. Fortunately for the lizard, the muscle bundles near the base of its tail are so arranged that they expand and close the arteries, preventing loss of blood. The tail or sections of the tail have a very active reflex motion, which causes them to twist and wriggle with great energy for a short time after breakage, and these are much more conspicuous than the lizard's body. The king snake is thus preoccupied with the wriggling tail in its mouth—so much so that the lizard makes an inconspicuous and successful escape. The king snake, upon releasing the tail from its mouth momentarily, in order to secure a more convenient head-first position of its victim for swallowing, suddenly discovers that this "victim" has no head and vital organs. Searching about for the head and remainder of the body, it can find neither.

The lizard will very soon grow a new, though perhaps shorter, tail, but it is not true that pieces of the cast-off caudal appendage will join together again. The new tail never has the same shape or color as the original one, nor are the scales as even as in the old one. Of all its body structures or organs, the glass lizard can regenerate only its tail, which comprises about two thirds of its body. Consequently, if the shorter but vital part of the body is severed, all life will cease. The loss of a tail is rarely fatal to any reptile, and many other kinds of lizards besides the glass species have the same ability of regenerating the lost part. Other kinds of animals, including the crayfish and starfish, can part with appendages or less vital organs and grow replacements.

It seems to be common knowledge that the thief among snakes is the milk snake. Two concomitant elements enter into the main fabric of the story about this much-slandered creature, and both of these arise from erroneous inferences that have been drawn to explain the simple behavior of a most useful and misunderstood serpent. In the first place, there is the fact that cows on occasion give a decreased milk output, or they may go entirely dry. Second, certain snakes of the species *Lampropeltis triangulum* are sometimes observed frequenting barns or pastures in close proximity to cows. Moreover, the snakes have been seen on certain rare occasions drinking milk from a saucer which had been set out for the cat. The farmer now enters the picture: not being able to assign a good reason for his cows going dry, he blames the snake.

People generally react in one of two ways when

they meet a snake. If it is reasonably small and seeks escape, they may try to kill it with a convenient club or stone. If it is large, they flee its presence. The milk snake, being a reasonably small serpent, is often killed when it unfortunately gets in the way of mankind, and, like so many snakes that fall the victim of club and stone, its body is somewhat mutilated in the process. Were such a mutilated snake a gravid individual, the crushed eggs, as one herpetologist suggests, would give forth a milklike fluid, and this might be construed as confirmatory evidence for the belief in the snake's peculiar ability to milk cows.

There are, however, some very serious objections to the idea that *Lampropeltis* milks cows. The "eyewitness" accounts of snakes in the act of milking almost invariably have an evidential defect (common in a number of snake stories)—they relate an event long past. One distinguished herpetologist, Clifford H. Pope, has said in his work *Snakes Alive* that

> when a farmer insists that his sixteen-quart cow has fallen off a quart a day because of the theft of her milk by a 30-inch snake, he is accusing the snake of consuming about eight times its own volume of food every twenty-four hours! This is easily proved by measuring the amount of liquid displaced by a 30-inch milk snake, a very simple experiment. . . . The volume of a large 37-inch snake of this species it only half a pint. Snakes are well known to eat big meals but not that big! Here are a few other reasons why a milk snake could not perform such a feat:
>
> Unless it carried its own milking stool, it could not reach the cow's udder. If wrapped tightly around the cow's leg near her udder the snake would obviously be unable to force much milk into its body.
>
> There are no sucking muscles in its throat that would enable it to get the milk out, for milking a cow is no simple task, as every milker knows. Nor are there any valves or sphincters in its throat to keep the milk in under pressure of the distended stomach, body-wall, and skin.
>
> The six rows of sharp teeth in its mouth would not tend to soothe the cow; on the contrary the pain would drive her frantic.

The presence of the milk snake in the vicinity of barns, farmyards, and pasture lands is due for the most part to its being attracted to such places by mice and young rats, which are its natural food. One biologist, H. A. Surface, analyzed the stomach contents of 42 specimens of *Lampropeltis* from Pennsylvania and found that field mice comprised 72 percent of the food. As every farmer realizes, it is field mice that raise havoc with the grain supply.

The milk-snake story, variously modified in many parts of the world, is nowhere so grotesque as in Brazil. There it is believed that the snake secures its milk supply, not from cattle, but from human beings. According to this legend, the snake, which frequents the huts of the Indians in the night, interrupts the feeding of the baby while the mother sleeps, inserting its tail into the child's mouth in order to soothe it.

Certain writers in the field of natural science, in denying the truth of the milk-sucking habit of milk snakes, have stated categorically that snakes do not drink milk, and they have cited instances of the snakes refusing to take such liquid nourishment. This is not entirely true. Many snakes *do* drink milk if it is provided for them, and, moreover, it is a common practice in India for milk to be placed in saucers in some zoological parks in place of water, this to ensure the snakes' obtaining nourishment when they refuse other food.

Another mythical serpent, confined to the rural scene, is the horsehair snake. In its adult stage, the worm *Paragordius varius* is long, extremely slender, and unsegmented (in distinction to the earthworm). Its occasional appearance in animal drinking troughs, especially those of horses, and its vague resemblance in form to a horse hair that may float on the same drinking water, have given it a misleading appellation, "horsehair snake." However, in the animal kingdom, this worm is many phyla below, and it is far removed from the phylum Chordata, to which snakes belong.

Paragordius deposits its eggs in long strings in any convenient shallow pond, pool, or receptacle of water. The larvae penetrate the bodies of young mayflies and other kinds of aquatic insects; they later escape from their hosts and seek out second hosts, generally beetles, crickets, or grasshoppers. In the body cavity of the second host the larvae continue their development, eventually emerging and seeking water, in which they become sexually mature worms.

It would appear to be an easy and logical step in the minds of many people unfamiliar with the animal kingdom and natural biological processes, to assume that when a hair is swallowed, a live worm or snake develops from it inside the stomach or intestines. Tapeworms and other visible and smaller human parasites are responsible for this old and venerable item of zoological folklore. Thus, by association, a hair which was once a part of a live animal becomes alive again, only in a different form.

Some people believe that one may swallow a young snake while drinking spring, well, or even tap water. Accordingly, the ingested serpent is said to live and to develop into an adult specimen of its kind inside the human stomach or intestines.

This notion undoubtedly gained new adherents a few years ago in the Borough of the Bronx; New York, when numerous varieties of plankton and other fresh-water organisms were observed in the unfiltererd drinking water following the opening of a new source of water supply from upstate New York.

The late Clifton Johnson, in his authentic collection, *What They Say in New England,* reported the story

> that once there was a certain child that took large quantities of food, in particular a great deal of milk, yet became more and more emaciated. One night when the child was sitting at the table with a bowl of milk before it, of which it had not eaten, a great snake put its head out of the child's mouth. Apparently it was hungry, had scented milk, and came up out of the child's mouth to get it The child's father was by, and he grabbed the snake by the neck, and pulled it out It was four feet long.

Dr. Morris Fishbein, editor of the *Journal of the American Medical Association,* has pointed out that because someone once saw an X-ray picture of a stomach tube in the human stomach, the idea grew that it would be possible for a person to swallow a snake egg and to have it develop in the stomach. This illuminating detail brought out by Dr. Fishbein might well be termed a recrudescence of our hair-to-snake myth but with machine-age applications.

A very curious snake, whose existence is commonly brought to the attention of zoo and natural-history museum curators, is the Northern water moccasin. If the uninformed person happens to be swimming in the waters of Lake Champlain, New York, Sebago Lake, Maine, or any other Northern body of water for that matter, and if he inadvertently steps upon a crayfish or bullhead and receives a response therefrom, or if he sees an eel or a banded water snake swimming in its natural habitat nearby—then such a creature must be the poisonous and ill-reputed water moccasin. It is entirely possible too that leeches, which elongate their bodies while swimming, as well as numerous kinds of harmless snakes that occasionally swim across bodies of water, are confused with water moccasins.

The actual range of the venomous water moccasin includes the Dismal Swamp, Virginia, to Florida and the Gulf States, and Arkansas to Illinois. The dreaded water moccasin of our Northern states all too often turns out to be the banded water snake (*Natrix sipedon*), erroneously called the "water rattle" and "water pilot" in certain localities. This nonvenomous serpent frequents the shores of ponds and streams, is a good swimmer, and feeds upon frogs and sluggish fish. It does not molest human beings unless molested by them.

All green snakes are held to be poisonous. They are supposed to be filled with a green venom, which the green color of their bodies suggests. But the fact is this: venomous green snakes do make their home in the tropics but there are none in North America. It can be said that such a superficial distinction as color is never an indication of a snake's venomous character any more than is the shape of the snake's body or head. The real reason a green snake is green is probably that nature intended this animal to blend in with its grassy surroundings and so escape detection by certain of its natural enemies or its potential prey.

Our smooth green, or grass, snake (*Opheodrys vernalis*) is remarkably gentle and inoffensive, and it is almost impossible to induce it to bite when handled. One professional snake-swallower of my acquaintance prefers snakes of the green and ring-neck species in performing his act since, as he maintains, these are not "nasty" like the garter snake and other kinds.

Inasmuch as our green snakes are probably the most characteristic grass-inhabiting serpents, they are eligible candidates for the term, in its literal sense, of "snake in the grass." But, unfortunately for these inoffensive little serpents, and very unjustly too, "snake in the grass," as applied to certain members of the human race, has come to mean that these people have a reputation for underhanded and surreptitious acts.

No gallery of American mythical snakes would be complete without mention of the celebrated blow snake, sometimes known as the "hissing snake," "blowing viper," "spreading adder,"or "puff adder." This is none other than the common hog-nosed snake (*Heterodon contortrix*), whose antics, in times of danger, have given it a notoriously bad reputation. Numerous stories are to be heard of the way in which this harmless serpent blows its poisonous breath or spits venom from a distance into the innocent observer's face, thereby producing burns, blindness, infection, or convulsions. Even though the majority of these stories are related secondhand, there seems to be no dearth of actual eyewitness "evidence" of such personal encounters, with consequent poisonings.

Since the hog-nosed snake is fairly common in sandy locations, where it preys upon toads, it is not a difficult matter to check on its bad reputation

and to obtain actual observations on its behavior. When first approached, the hog-nosed snake flattens its head and neck, inflates its lung, and then exhales, hissing loudly. To make its display of ferocity even more effective, the snake elevates the forward third of its body from the ground. All this tends to give it a formidable and threatening appearance, even more so than that of any Indian cobra of corresponding size. If the intruder does not quickly retreat from the presence of the bluffing serpent, it may pretend to strike, with continued hissing, but with closed mouth. In fact, one cannot induce this snake to bite at any time. Provided the intruder is intimidated and departs the scene, the snake quickly deflates and either seeks a place of security or resumes its normal activities.

The stories about the poisonous breath of the hog-nosed snake are pure fiction, for its breath has no effect on eyes, nose, or skin. If the snake is picked up with a stick or held in the hand, a pungent and disagreeable secretion may be emitted from the anal scent glands, and, to an already frightened or doubtful person, this odor may be strongly suggestive of a poisonous breath.

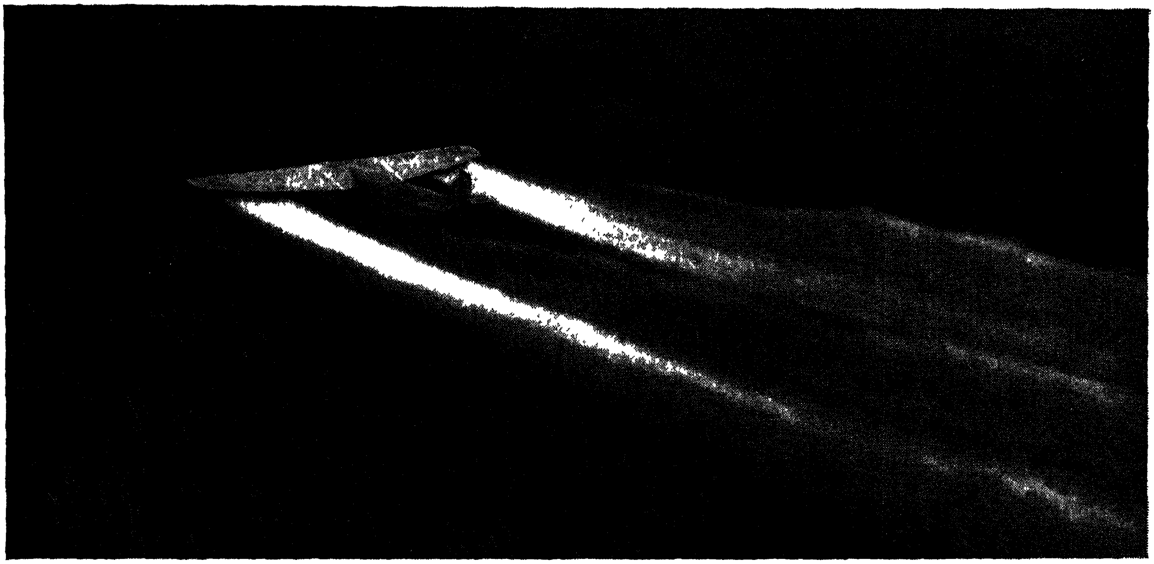
If a snake of this species is further molested, and especially if it is abused or injured, or if its first hissing and puffing antics fail to intimidate, it resorts to a second bluff, that of the death feint, or playing possum. Here the snake writhes and squirms convulsively with mouth wide open and as if in mortal agony. Then it rolls over on its back, the head is rubbed in the dust, there are a few convulsive movements, and finally all movement ceases. To all outward appearances the snake is dead, and it may be moved about or carried on a stick while remaining inert and playing dead. If placed on the ground rightside-up, it will promptly roll over again on its back, thus giving itself away. According to this creature's instinctive behavior pattern, it appears that in order to seem thoroughly dead a snake should be lying on its back. When danger appears past, the snake twists and raises its head slightly; if the coast seems clear, our bluffer turns over and crawls away.

Included in the fanciful category of snakes that hiss, blow, or spit poison, are the ones mentioned by Father Charlevoix, the eighteenth-century Jesuit missionary and traveler. Near Detroit, he related, there existed two rattlesnake islands in whose vicinity the very air was infected. Another colonial traveler learned that certain islands at the west end of Lake Erie were overrun with a species of small, speckled snake about 18 inches long, which hissed poison into the atmosphere:

> When any thing approaches, it flattens itself in a moment; and its spots, which are of various dyes, become visibly brighter through rage: at the same time it blows from its mouth with great force a subtile wind, that is reported to be a nauseous smell; and if drawn in with the breath of the unwary traveller, will infallibly bring on a decline, that in a few months must prove mortal, there being no remedy yet discovered that can counteract its baneful influence.

Real blow snakes do exist, but not in America. These include several common cobras and cobra allies. The specialization in question is undoubtedly for the purpose of repelling antelopes and equally dangerous, though unwitting, foes. The veldt-ranging cobras of Africa learned to expel their venom in a fine spray for considerable distances and with a fairly good aim at the eye. The poison is not caustic and the skin remains unaffected; prey cannot be secured by this means, but the moist eye allows so rapid an absorption that sharp pain and subsequent brief photophobia immediately result from the venom contact.

SCIENCE ON THE MARCH



NEW ORGANIC INSECTICIDES

DURING the last five years a bewildering array of new insecticides has appeared on the market. The prospective user of these products is confused by conflicting statements made by their promoters. Each product is claimed to be more potent than DDT as an insect killer, and at the same time it is heralded as harmless to man. What are the merits of these new materials?

The new insecticides that have attained commercial recognition are the chlorinated compounds DDT and its methoxy analog, TDE [1,1-dichloro-2,2-bis(*p*-chlorophenyl)ethane]; benzene hexachloride; chlordane; chlorinated camphene; 1,1-bis(*p*-chlorophenyl)ethanol; bis(*p*-chlorophenoxy) methane; and the phosphorus compounds hexaethyl tetraphosphate, tetraethyl pyrophosphate, and parathion.

Of the chlorinated organic insecticides DDT is the best known; everyone has heard of it, in fact. During the war the armed services used millions of pounds of DDT in controlling the body louse, flies, mosquitoes, bedbugs, and cockroaches. At the close of the war DDT was made available to civilians, and large quantities of it have been used for the control of the codling moth, Japanese beetle, gypsy moth, pink bollworm, white-fringed beetle, potato flea beetle, and numerous other insects.

Trimotored Ford airplane sprays DDT in Idaho mountain forests for the control of the Douglas fir tussock moth. Airplanes now carry huge loads of liquid insecticides. Sprayed over great acreages in minute amounts, these control insects in extensive or otherwise inaccessible areas. (USDA photograph.)

DDT, $C_{14}H_9Cl_5$, was first synthesized by a German student in 1874, but no use was found for it until 1939, when a Swiss chemist, seeking a substitute for lead arsenate, found it to be poisonous to the Colorado potato beetle. It is made from chloral (the active ingredient of knockout drops) and chlorobenzene (a coal-tar product) and contains 50 percent of chlorine. In 1947 the production of DDT in the United States amounted to 50,000,000 pounds. Originally it sold for $1.60 a pound, but the wholesale price recently tumbled to 29 cents a pound.

DDT is a white powder with a slight fruity odor. It is insoluble in water but soluble in kerosene, oils, and most organic solvents. It can be used as a dust mixed with talc or other diluent or as a spray suspended or emulsified in water or in solution in kerosene, fuel oil, xylene, or other solvent. It can also be used in aerosol bombs, dissolved in a mixture of solvents, one of which, dichlorodifluoromethane (Freon-12), has a sufficiently high vapor pressure at ordinary temperatures to expel the contents of the bomb when the valve is opened.

For combating the body louse, the dreaded carrier of typhus, the soldiers dusted themselves with a powder containing 10 percent of DDT.

During the war bedbugs were controlled by spraying mattresses and beds with a 5 percent solution of DDT in kerosene. Mosquito larvae were killed by a fuel-oil solution of DDT distributed by airplane over marshes. Underwear was proofed against body lice by washing in a xylene emulsion of DDT. For agricultural use DDT is generally applied as a suitably diluted dust or in water suspension as a spray.

The great value of DDT as an insecticide lies in its residual action. DDT is not volatile and not soluble in water. This means that, when sprayed on apples, for example, it does not evaporate and is not washed off by rain but remains as a toxic coating giving protection against insect attack.

The toxicity of DDT to man and domestic animals is low. DDT dust is not irritating to the skin, and millions of pounds have been applied to fruits and vegetables without harm to the consumer. Pharmacologists warn, however, that, despite the fact that DDT has been studied for at least five years, there is still need for more information before a complete appraisal can be made of the hazards involved in its use.

Chlorinated insecticides closely related to DDT are its methoxy analog, $C_{16}H_{15}Cl_3O_2$ (31 percent chlorine), and TDE, $C_{14}H_{10}Cl_4$ (44 percent chlorine). These materials are less toxic than DDT to warm-blooded animals, but they are also less toxic to most insects. The acute oral toxicity to rats of the methoxy analog of DDT is 1/24 that of DDT; that of TDE, 1/10. It should be remembered that acute toxicity bears no relation to chronic toxicity. The methoxy analog of DDT is a kidney poison, and TDE, in addition to being a liver toxicant, appears to have a special predilection for the adrenal glands.

Benzene hexachloride is another old compound only recently found to have value as an insecticide. It was first made in 1828 by Michael Faraday, but was not tested as an insect poison until 1941 in France. At about the same time the English began experimenting with it and discovered that it was highly toxic to many injurious insects. In England it has been called 666 and Gammexane.

Benzene hexachloride, $C_6H_6Cl_6$, prepared by adding chlorine to benzene, contains 73 percent of chlorine. It is easily made, and the raw materials are abundant and cheap. In tests in this country it has proved effective against lice and ticks on livestock, the boll weevil, grasshoppers, and wireworms in soil. Although superior to DDT for these uses, it lacks the residual action of DDT. This chemical has a strong, persistent odor, which is picked up and retained by many fruits and vegetables, and this fact may greatly limit its field of usefulness. In New Jersey last year many potatoes were made unfit for human consumption because too much benzene hexachloride had been added to the soil to kill wireworms. The gamma isomer of benzene hexachloride, which accounts for the insecticidal value of the technical material, has twice the acute oral toxicity of DDT when fed to rats. It is a liver poison.

Chlordane, $C_{10}H_6Cl_8$, containing 69 percent of chlorine, is an American development and is now manufactured by two companies in this country. It is several times as toxic as DDT to house flies and cockroaches, but lacks the long-lasting, or residual, property of DDT. It has come into extensive use by pest-control operators and is employed on a large scale for grasshopper control. Other insects susceptible to chlordane include ants, chiggers, ticks, fleas, mosquitoes, the boll weevil, the plum curculio, and the squash bug. The acute oral toxicity of chlordane to rats is one half that of DDT.

Chlorinated camphene, $C_{10}H_{10}Cl_8$, is made by chlorinating camphene, which in turn is made from pinene, a constituent of turpentine. Its chlorine content is 68.5 percent. This insecticide appears especially promising for the control of insect pests of cotton. Its acute oral toxicity to rats is four times that of DDT.

1,1-Bis(*p*-chlorophenyl)ethanol, or $C_{14}H_{11}OCl_2$, contains 26.5 percent of chlorine. It may be regarded as a combination of chlorobenzene (used in making DDT) and ethyl alcohol. It has given promising results against mites, especially in apple orchards in the Pacific Northwest.

Bis(*p*-chlorophenoxy)methane, or $C_{13}H_{10}OCl_2$, contains 26.3 percent of chlorine. It is related to DDT and also to 2,4-D, the famous weed killer. It has given good results in the control of the citrus red mite, a serious pest of lemons and oranges in California.

Little is known of the pharmacology of 1,1-bis(*p*-chlorophenyl)ethanol and bis(*p*-chlorophenoxy)methane. They should be handled with caution, and spray residues should not be left on fruits or vegetables intended as food for either man or beast.

The other class of new insecticides, the phosphorus compounds, has shown amazing toxicity. No one would have suspected that these organic phosphates would prove so poisonous. Unfortunately, they are highly poisonous to man as well as to injurious insects.

The story of these organic phosphorus compounds is a romantic one. About twenty years ago the Germans started research to develop new and more powerful war gases. Hundreds of compounds

were synthesized and tested on laboratory animals. They were also tested on cockroaches, flies, and other insects to determine their possible value as insecticides. In this way there was developed hexaethyl tetraphosphate, which proved very toxic to plant lice and was used by the Germans, in a formulation called Bladan, as a substitute for nicotine. In the summer of 1945 the Army sent American chemists to Germany to interview German chemists and to uncover German chemical secrets. In this way information concerning hexaethyl tetraphosphate was brought to this country.

Hexaethyl tetraphosphate is easily made by reacting triethyl phosphate, a liquid used in the plastic industry, with phosphorus pentoxide or phosphorus oxychloride. The product is a heavy liquid, which dissolves readily in water and most organic solvents, excepting kerosene. Hexaethyl tetraphosphate is twice as toxic as nicotine to aphids. This fact is of commercial significance, because the demand for nicotine is always greater than the possible supply from tobacco. Hexaethyl tetraphosphate is also highly effective in killing many other insects and is toxic to warm-blooded animals as well, its acute oral toxicity being thirty-five times that of DDT.

Studies by chemists at the Agricultural Research Center at Beltsville, Maryland, have shown that hexaethyl tetraphosphate is a mixture and that its toxicity is due to its content of about 20 percent of tetraethyl pyrophosphate. This compound can be readily made by the same process used in making hexaethyl tetraphosphate, and preparations containing about 40 percent of tetraethyl pyrophosphate are now on the market.

The newest insecticide is parathion. This also was developed in Germany, at Elberfeld, where it was known under the code number E-605. Its secret was brought to this country late in 1945. Parathion has aroused great enthusiasm among the applegrowers in Oregon and Washington because of its ability to kill mites, which are resistant to DDT and most other insecticides. Parathion is also toxic to a wide range of insects that attack other fruits and vegetables. It is at least five times as toxic as DDT to mosquito larvae. It is more stable than hexaethyl tetraphosphate and tetraethyl pyrophosphate, and a spray deposit retains its insect-killing property for several weeks. The disadvantages of parathion are its garliclike odor and its high toxicity to warm-blooded animals. Its acute oral toxicity to rats is seventy times that of DDT; moreover, it damages the colon, and necrosis of the gall bladder has been noted in rats. There are indications that parathion has a cumulative effect.

The prospective user of all these insecticides should keep in mind that they are so new that many questions concerning them cannot be answered. Most of them are highly specific in their action and are ineffective against certain of our worst insect pests; some have offensive odors and may taint foodstuffs; all are poisonous to man and must be applied at such times and in such dosages as to avoid spray residues on fruits and vegetables. Some may cause toxic symptoms if applied to the skin, and the operator who applies them should wear protective clothing and a gas mask. If handled with an understanding of their physical, chemical, and pharmacological properties, however, these new products will constitute valuable additions to the chemical weapons man must employ in his unceasing war against injurious insects.

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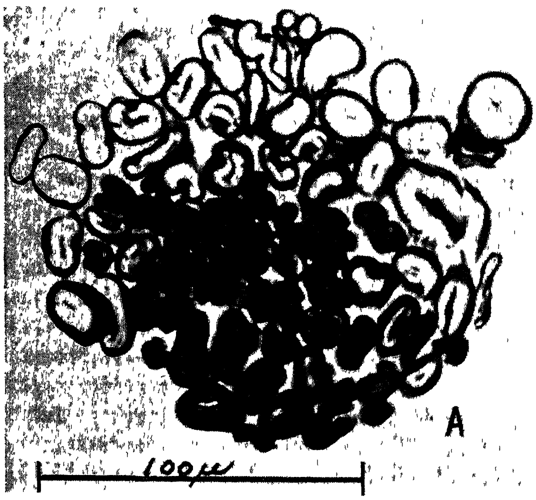
TEXTILES THROUGH THE MICROSCOPE

The microscope is thought of by the average person as an instrument a scientist uses for some such study as the anatomy of bugs. It would seldom occur to him that this same instrument is used, although indirectly, in the improvement of chemically treated textiles, which the customer will eventually purchase in a department store. For example, the housewife examines a piece of cloth for desirable qualities with never a thought of what chemicals or processing went into its manufacture or how the small fibers would look if she could see them.

The microscopist in a textile laboratory is able to observe such things as the fiber shape, the structure of fibers, yarns, and cords, and the penetration of chemicals into yarns and fiber walls so that the chemist may know why his treatment did or did not give good results. Full discussions of the usual methods for such investigations have been published in both textbooks[1,2] and journals.[3,4]

The chemist measures quantitatively the percentage "takeup" of chemicals in a series of samples treated by different methods, but only

through microscopy can he know just how far these chemicals have penetrated into the fiber or yarn. Crease resistance in fabrics may be obtained by impregnating the cloth with any one of several resins[5]. If resin-impregnated cotton yarns are soaked in a solution of a dye that stains the resin and not the cotton fibers and then a thin section is cut, the penetration or lack of penetration can be seen. Figure 1 (*A*) represents a cross section of a pretreated cotton yarn photographed under the microscope to show that melamine resin has partially penetrated the fiber walls; and (*B*) shows that urea formaldehyde has penetrated the yarn only. To obtain these pictures, treated samples were dyed overnight in a 1 percent solution of Kiton Pure Blue V, rinsed thoroughly, and cross-sectioned in the Hardy hand microtome,[6] a special device designed for cutting fibers. Thus, with this technique, samples treated according to various procedures can be examined to see exactly what has



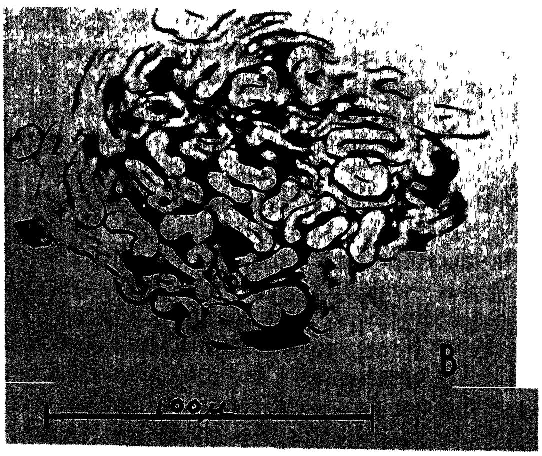


Fig. 1. *A*: Cross section of pretreated cotton yarn in which black areas represent penetration of melamine resin into fiber walls. Some fibers show complete penetration and others are affected around outer edge only. *B*: Cross section of cotton yarn in which black areas represent deposition of urea formaldehyde resin around fibers.

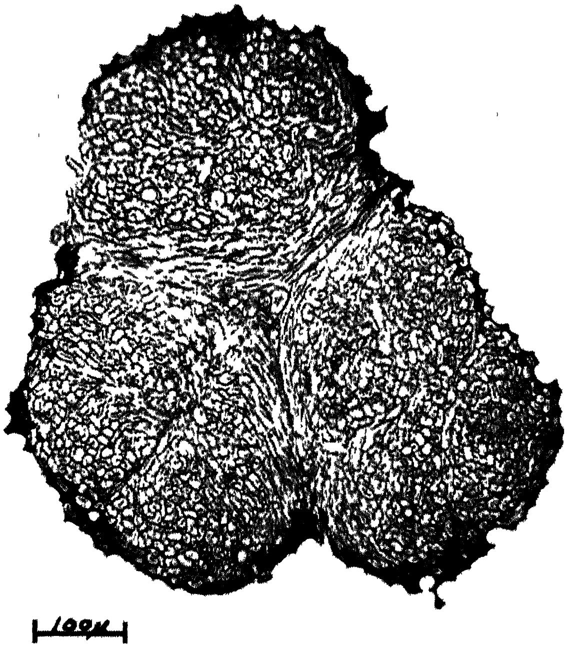


Fig. 2. Cross section of 3-ply cotton tire cord showing adhesion of black rubber stock to edge of cord. Each ply contains 4 single yarns.

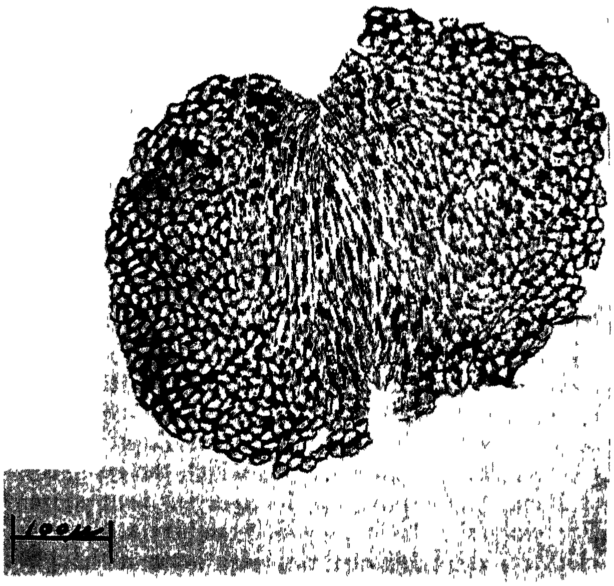


Fig. 3. Cross section of 2-ply rayon tire cord showing penetration of latex into cord. Black areas represent location of latex.

Fig 4. *Left*: Cross section of well-mercerized Empire cotton. *Right*: Cross section of untreated Empire cotton.

Fig. 5. Longitudinal view of cotton fiber swelled with cuprammonium hydroxide to show ballooning. Inside the balloons may be seen the layers of cellulose composing the secondary wall.

happened during each treatment. The method of application that gives the best results can then be selected.

Textile microscopy can also be applied in the study of tire cord construction, adhesion, and impregnation. Rubber adhesion may be determined visually by the microscopical examination of a cross section in which the black rubber stock around the edge of the cord is easily discernible (Figure 2). Latex penetration presents a problem of its own because of the lack of color of this substance; however, by using a staining technique developed at the Southern Regional Research Laboratory,[7] it is possible to show the exact location of the latex *in situ* in the cord. Figure 3 is a cross section of a 2-ply rayon cord showing penetration of latex into the center of each ply.

As a further illustration, the microscope has been useful in studies of mercerized cotton, known principally because of its commercial use in sewing threads, socks, and fine fabrics. This chemical process, which involves treatment with 18 percent sodium hydroxide under tension, swells the fibers and leaves them lustrous. To determine whether a sample of cotton has been well mercerized, it is cross-sectioned and examined under the microscope. If the treatment has been effective the fibers will appear to be virtually round (Fig. 4, *left*) as compared with irregularly shaped fibers in an untreated sample (Fig. 4, *right*).

When the chemist is familiar with the make-up of cotton he can often predict the results of his

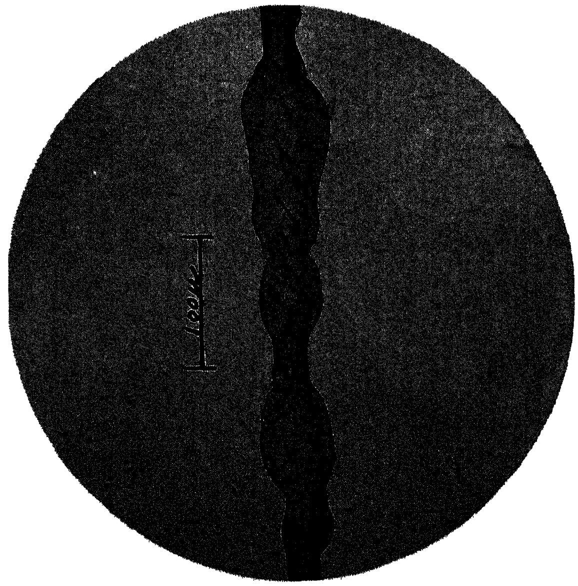


Fig. 6. Longitudinal view of partially carboxymethylated cotton fiber swelled with water to show ballooning similar to that seen in Fig. 5. Layers of secondary wall are not visible since fiber appears to be fused.

treatment before applying it. The microscope aids in studying the internal structure of cotton.[8,9] For this purpose, fibers are swelled with cuprammonium hydroxide, which causes the primary wall of raw cotton to peel back and the secondary wall, consisting of layers of cellulose, to swell out and form balloons. Figure 5 is a photomicrograph of a cotton fiber in which the primary wall, under pressure of the swollen cellulose, has burst and formed constricting collars, or bands. This microscopical technique reveals the cellulosic layers composing the secondary wall, spiraled bands around the outside of the fiber, and, sometimes, reversals in the direction of this spiral.

With the microscope it is possible to study such chemical treatments as the partial carboxymethylation of cellulose,[10] which produces quickly swellable cotton fibers to be used in cloth where rapid absorbency is desired. Figure 6 shows a partially carboxymethylated fiber swelled with water to test the effectiveness of the treatment used. Although the secondary wall of the fiber is distended as in Figure 5, the lamellae are not visible because the chemical and physical properties have been altered by the treatment.

Microscopical investigations have been useful as a check in differential dyeing studies of thick- and thin-walled fibers.[11] According to this dyeing technique, when a mixture of mature (thick-walled) fibers and immature (thin-walled) fibers is dyed in a special solution containing both red and green dyes, the mature fibers absorb the red dye and the immature fibers, the green. The microscopist separates the sample into the two colors and examines the fibers in cross section. For all the varieties of cotton examined, the microscopical test has checked with the dyeing test. Figure 7 (*left*) is a photomicrograph of a cross section of mature fibers (dyed red) and (*right*) of a cross section of immature fibers (dyed green).

Problems in fiber identification are frequently solved with little difficulty when the sample is examined in cross section under the microscope. Each type of fiber has its own characteristic shape, and, therefore, an unknown sample may be identified by comparison with others of known origin. Figure 8 is a cross section of a yarn purported to contain all wool fibers. Upon examination, however, it was found that the cloth contained a mixture of wool, rayon, and cotton. Of interest also is the fact that it was possible to detect the presence of reworked wool because of the wide variety of colors represented in the center of these fibers, indicating their previous use in lighter-colored textiles.

The role that the microscope plays in the im-

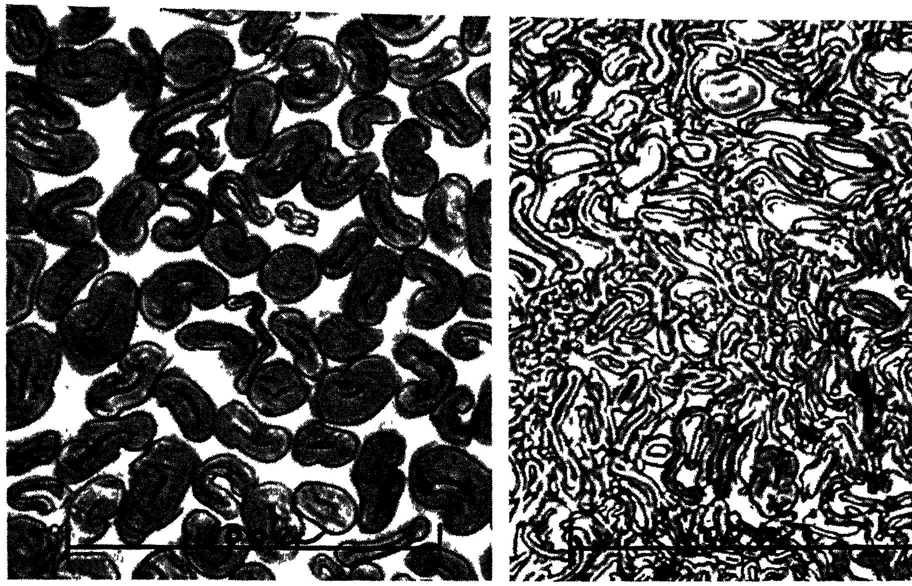


Fig. 7. *Left*: Cross section of mature (thick-walled) cotton fibers dyed red in differential studies. *Right*: Cross section of immature (thin-walled) cotton fibers dyed green in differential dyeing studies.

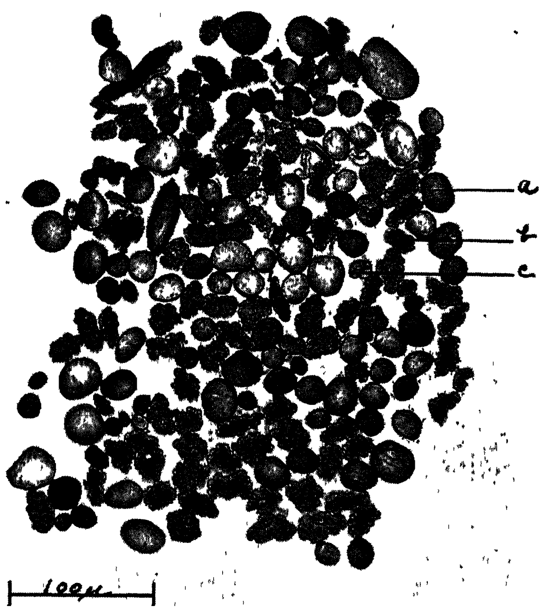


Fig. 8. Cross section of mixed yarn containing (*a*) wool, (*b*) rayon, and (*c*) cotton fibers.

provement of commercial textiles is varied and only briefly outlined in this limited space. There are many other applications of microscopy that play an important part in the commercial manufacture of chemically treated textile materials. Fiber microscopy is not used to study chemically treated materials only, however, but may be applied to such related branches of research as genetics, physical testing, fiber and yarn processing, and the inspection of damages caused by weathering, aging, and mechanical wear. This field of research offers unlimited opportunities for the fiber microscopist to assist first the chemist and ultimately the manufacturer in the development of improved materials for consumer use.

REFERENCES

1. Matthews, J. M. *Textile Fibers.* Mauersberger, Ed. New York: Wiley, 1947.
2. Schwarz, E. R. *Textiles and the Microscope.* New York: McGraw-Hill, 1934.
3. Royer, G. L., and Maresh, C. Applications of Microscopy to the Textile Industry *Amer. Cyanamid Co Tech. Bull No. 796.* Bound Brook, N. J.: 1947.
4. Bailey, T. L. W., Jr., and Rollins, M. L. Some Applications of Microscopy to Cotton Research. *Text. Res*, 1945, 15, 1–6.
5. Powers, D. H. The Effect of Synthetic Resins on Cellulose and Protein Fibers. *Ind. Eng. Chem.*, 1945, 37, 188–93.
6. Hardy, J. I. A Practical Method for Making Thin Cross Sections of Fibers. *USDA Circ. 378,* 1935.
7. DeGruy, I. V., and Rollins, M. L. A Microscopical Technique for Determining Latex Distribution in Tire Cords. *Text. Res. J.*, 1948, **18**, 371–3.
8. Anderson, D. B, and Kerr, T. Growth and Structure of Cotton Fiber. *Ind. Eng. Chem.*, 1938, **30**, 48–54.
9. Rollins, M. L. Photomicrographic Comparison of Milkweed Bast and Cotton Lint in Relation to Cell-Wall Structure of Other Natural Cellulose Fibers. *Bot. Gaz.*, 1947, **108**, 495–510.
10. Reid, J. D., and Daul, G. C. The Partial Carboxymethylation of Cotton to Obtain Swellable Fibers. *Text. Res. J.*, 1947, **17**, 554–61.
11. Goldthwait, C. F., Smith, H. O., and Barnett, M. P. New Dye Technique Shows Maturity of Cotton. *Text. World.*, 1947, **97**, (7), 105–108, 201–202, 204, 206.

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BOOK REVIEWS

THE STUMBLING CLIMB

Science and its Background. H. D. Anthony. 304 pp. $3.00. Macmillan and Co., Ltd. London.

IN RECENT years many histories of science have appeared, dealing with one branch of science or one period or a single country. Similarly, historians have recognized the importance of science in the development of human knowledge as a whole. The author's claim to a new approach to the subject is based on his effort to bring these two lines of investigation into intimate conjunction, carrying them along together so that scientific discoveries become an integral part of the general stream of man's development through the ages.

In the earlier part of the volume, the research, teachings, and discoveries of about fifty outstanding men are set forth in the social and political background in which they arose. Thales, the philosopher, Hippocrates, the great physician, Leonardo da Vinci, Aristotle, and Galileo may serve as examples of the author's clever selections to show how from the beginning of the human struggle to understand the world, one after another great thinker has expanded the field of science with contributions vital to further progress.

Progress was by no means at a uniform rate. There were times when science languished in the doldrums of confusion. Then some brilliant genius invented an instrument or devised a new method that threw a clear light on enigmas and mysteries, and made the pathway easier. There is a real excitement in traveling again over the road on which man stumbled forward for centuries. Such a simple matter as the Arabic system of numbers made elaborate computations, theretofore impossible, easy and rapid. What if today we tried to multiply DCC by XIX in the old Roman notation? Then in the nick of time Copernicus upset the Ptolemaic, or geocentric, theory of the universe, thus clearing up one more mystery; or Harvey demonstrated the circulation of the blood; or Newton discovered the law of gravitation; or Lavoisier laid the foundations of chemistry. Even the alchemists were on the right track on the possibility of transmutation of the original elements. Today the atomic investigations have set the transmutation of uranium among the items of information in the daily press.

The name of Pasteur is immortalized in the process of rendering milk safe to drink. Lord Lister slew the dragon of septicemia. Simpson and Morton brought the blessing of anesthesia to suffering man.

The author takes the reader step by step along the road man has traveled, showing how the thousands of inquiring minds have paved the way to the myriad contrivances of the modern world. And the end is not yet. Each discovery leads to another, the rope by which man has climbed consisting of three strands—knowledge, action, and vision. Each discovery is quickly translated into action and practical use, and also sets men to dreaming of further advances.

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TAGGED ELEMENTS

The Use of Isotopes in Biology and Medicine. A symposium. xiv + 445 pp. Illus. $5.00. University of Wisconsin Press. Madison.

THE use of isotopes in biology is about twenty-five years old, in biochemistry only fifteen, and in medical diagnosis and therapy about ten. Hence the field is very new, standard practice is in the process of development, and the number of workers acquainted with the techniques few.

The present book, really a collection of related papers by notable pioneers in the production and utilization of isotopes, is designed to educate not only the novice about to begin his investigation, but also the expert who wishes to keep himself informed as to the progress being made by his colleagues in related fields of endeavor. It is recommended to the nuclear physicist or isotope chemist who wishes to know his products are being utilized in order that he may plan the direction of his future efforts, and to the biologist, biochemist, and medical man who wishes to make the most efficient use of this new tool.

The Use of Isotopes in Biology and Medicine begins with a short Preface by Perry W. Wilson and an address of welcome by William A. Middleton, University of Wisconsin, where the symposium was held. The first paper of the book proper is an excellent résumé by Hans T. Clarke, of Columbia University, of the Historical Background of Isotopes in Bio-Chemistry. It will be recalled that Dr. Clarke was associated with Dr. Urey in the early days of the separation of heavy water and that he also played an important role in the penicillin research program during World War II.

In the next paper—Separation of Stable Isotopes—the reader is given a brief but thorough course in the cascade diffusion method of Hertz, the chemical separation methods, and the thermal diffusion methods of Clausius and Dickel. This is followed by The Preparation of Radioactive Isotopes, by Glenn T. Seaborg, of the University of California.

Next, Paul C. Aebersold, formerly of the Naval Research Laboratory, Anacostia, D. C., now Chief of the Isotopes Branch, U. S. Atomic Energy Commission, discusses Recent Developments in the Avail-

ability of Isotopes. This is an important chapter for those who contemplate work with a new isotope and who need to know what quantities of this isotope can be obtained.

The fifth, sixth, and seventh papers deal with the detection and measurement of isotopes. These are The Detection of Stable Isotopes, by Alfred O. Nier; Fundamental Principles of the Detection and Measurement of Radioactivity, by Charles D. Coryell; and Assay of Radioactive Isotopes in Biological Research, by Martin D. Kamen. The eighth paper—Preparation of Compounds Containing Isotopes, by Donald B. Melville—ends what might be considered the preliminary part of the book, on history and general techniques.

The middle section, consisting of papers nine to sixteen, inclusive, is of special interest to physiologists and medical investigators. The first five of this group deal with metabolic processes. These are: Studies on the Metabolism of Proteins, by David B. Sprinson; The Use of Isotopes in the Study of Intermediate Carbohydrate Metabolism, by Harlan G. Wood; The Intermediate Metabolism of Lipids, by Konrad Bloch; Tracer Studies on the Metabolism of Mineral Elements with Radioactive Isotopes, by David M. Greenberg; and Application of Radioactive Iodine to Studies in Iodine Metabolism and Thyroid Function, by I. L. Chaikoff and A. Taurog. Papers on Medical Applications of Radioactive Tracers, by Joseph G. Hamilton; Therapeutic Use of Radiophosphorus in Polycythemia Vera, Leukemia and Allied Diseases, by Byron E. Hall; and Treatment of Thyroid Disease by Means of Radioactive Iodine, by Saul Hertz, are of especial interest to the specialist or general practitioner in clinical practice. By way of an aside, Hall's paper should be of some interest to hematologists investigating the cause and treatment of radiation illness as seen in patients and animals exposed to atomic bomb ionizing radiations.

Health Hazards in the Use of Radioactive Isotopes, by William F. Bale, can be classified under the heading of Health Physics. It is an able condensation of many important parts of the late D. E. Lea's book *Actions of Radiations on Living Cells,* plus a few additions. The next paper, by James J. Nickson, Measures for the Protection of Personnel and Property, also belongs to Health Physics, with a sanitary engineering angle.

International Aspects of Atomic Energy, by Harold C. Urey, takes us out of the realms of science into those of politics. This paper should be read by everyone. The last paper, Comments on the Development of Atomic Energy, by Farrington Daniels, is a general survey of some of the problems to be expected in the course of the development of atomic energy.

A listing of other papers on special topics ends the book.

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ABC

The Alphabet: A Key to the History of Mankind. David Diringer. 607 pp. Illus. $12.00. Philosophical Library. New York.

THE first third of this book deals with the history of the nonalphabetic scripts in roughly chronological order: cuneiform writing; hieroglyphic writing; the Cretan scripts; the undeciphered script of the Indus Valley civilization; the scripts of the Hittites; Chinese writing; the ancient scripts of the Americas; the script of Easter Island; other ideographic scripts of certain Asiatic, African, and American peoples; syllabic systems of writing; and certain systems the author calls "quasi-alphabetic"—for example, the early Persian cuneiform, or the inscriptions from Meroë in southern Egypt. The rest of the book is devoted to the main theme: the development of the alphabet itself. Another book promises to deal with the various problems of paleography, or "writing" as a whole, including handwriting. H. J. Uldall (Speech and Writing, *Acta Linguistica,* 1944, 4.11–6) has recently shown the possibilities a systematic study of writing can offer.

There are few comprehensive studies on this subject in the English language since Isaac Taylor's fundamental contribution of 1883. But this book does much more than merely fill a gap: it is bound to stand as the most authoritative treatment of the history of alphabetic writing for a long time to come. This is because the book is extraordinarily scholarly and exhaustive. It is, incidentally, also quite exciting reading. Diringer very seldom engages in speculation, but includes only material he can verify from different sources. His bibliography, though not complete, is useful and well arranged. It is too bad that the author found it necessary to cater to that fictitious individual, "the general reader," here and there, particularly in omitting the diacritic marks that give us the precise pronunciation of the written symbols.

The history of the alphabet illustrates at least three cultural processes: diffusion, independent invention, and "idea diffusion" (Kroeber's stimulus-diffusion). For this reason, the book will have special interest to scholars dealing with the dynamics of culture change. The author adduces much evidence to prove that most alphabetic writings are derived from a single source. The locus of invention was most likely Syria-Palestine, in the second millennium B.C. Of course the scripts of Egypt, Babylonia, and Crete, in all probability, exerted some influence in the process.

The jungle of material connected with the story of the alphabet and its descendants must have been very difficult to organize into logical divisions. This was accomplished with considerable skill, however. From the problem of origin, the author passes to the South Semitic alphabets of Arabia, the beginnings of which still constitute an open question. He next examines the Canaanite (early Hebrew and Phoenician) scripts, the Aramaic ones, and offshoots of the latter used by

non-Semitic groups. One of the most complicated and difficult groups of problems relates to the alphabets of India and southeast Asia, to which the author devotes more than a hundred pages. We approach home base when we reach the Greek alphabet, the main offshoot of which was the Etruscan alphabet; its descendant, the Latin script, in turn, led to the development of all the modern alphabets of western Europe.

The many illustrations add much to the fascination of the book.

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SOCIAL INTEGRATION

Society as the Patient: Essays in Culture and Personality. L. K. Frank. xiv + 395 pp. $5.00. Rutgers Univ. Press. New Brunswick, N. J.

INTEGRATION is as much an imperative in academic thought and science as it is in personality if social progress is to be sustained. For a quarter of a century, Lawrence Frank has pioneered this point of view in his insistence that a psycho-cultural approach to our problems—utilizing the confluent skills and findings of psychology and psychiatry, anthropology and sociology—is essential if adequate solutions are to be found. The present volume of thirty essays, reprinted in essentially their original form from the many professional journals to which he has contributed, represents the wide range of interest to which he has applied his principles of confluence and synthesis. The fields range from economic to social psychology, from education to the speculative orientation of the arts and sciences.

Too many people for too long have operated in the belief that we are all a product of the culture in which we live, as if that living matrix were immutable. Frank's most consistent argument—which strikes at the heart of all temporal power and vested interest intent upon preserving the *status quo* for their own sake—is that culture is plastic and elastic. It holds the hope "of a culture to serve human needs and values. . . ."

Frank is revolutionary only in so far as being thought-provoking is revolutionary. As a social scientist, he is a democratic evolutionist firm in the belief that the individual, too often sacrificed to social organization through his own doing, need not be so sacrificed. Freedom from the social "plaster cast" does not demand shattering that cast, but rather understanding how it came into being, the individual's relationship to it, the possibility of substituting a lighter, more durable, more useful cast. In the beginning man, out of his need for him, made God; and he has been changing, swapping, and substituting for him ever since. This is something of our relationship to culture.

In none of these essays has Frank attempted to package any of the problems about which he writes or to establish any formulas for their solution. Rather, in the language of experimental science, he points the way to, and provides us with, leads. For those who have missed the provocativeness of Dr. Frank's individual contributions, this volume will be most welcome.

JOSEPH HIRSH

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New York

PATTERN OF ALIENATION

A Mask for Privilege. Carey McWilliams. xiii + 280 pp. $2.75. Little, Brown. Boston.

THE thesis of this book is that anti-Semitism is inextricably related to our capitalist society, that it was instigated during the last century by the "industrial tycoons" as a diversionary, scapegoat technique to defend their own privileged positions and to conceal their egregious exploitation, and that it developed into an antidemocratic force, following the consecutive pattern of social, economic, and political exclusion. Driven inevitably to marginal occupations and status, the Jew thus becomes uniquely vulnerable to group hostility. Anti-Semitism, therefore, will ultimately cease only in a noncompetitive, planned society. Although the societal emphasis of the book is a valuable antidote to much of the loose psychologism permeating many of the works today purporting to deal with "prejudice," it rides its economic determinism too hard in the face of—and to the neglect of—other historical factors.

1) Mr. McWilliams never poses these crucial questions: Why in 1877 did the conspiring "industrial bourgeoisie" (rather simplistic historiography) choose the Jew as scapegoat? Why did otherwise sober men of goodwill among *all* classes, trades, and professions find it easy then, as now, to believe the most fantastic theories and canards about him? Why is it that, although other racial, religious, and national minorities have also suffered discriminations, the Jew remains unique among Toynbee's "penalized minorities" of vestigial antiquity? The answer antedates industrial capitalism; it is to be found in organized Christianity, which during its beginnings developed a theological corpus concerning the Jew that not only provided the rationale for official oppression but for all the subsequent iniquities associated with Jewish stereotypes. Before the advent of the Christian Church the Jews had, like others, incurred dislike whenever they participated in economic, cultural, and religious rivalries with different peoples; but after that they became a subject group, deprived of the citizenship they had enjoyed under the Roman Empire. More than that, they were viewed—according to patristic falsifications of history—as the crucifiers of Christ, cursed by God for having betrayed his laws and condemned to wander the earth until the required remnant had been converted to Christ's vision. It was in this period that the concept of the Jew as an "alien body"

was created, a concept which provided a *continuing* base for the economic, social, political, and psychological accretions of anti-Semitism later grafted upon it and which also explains why those multiple and contrary accusations leveled at the Jew by his enemies to this day are so plausible to them. Being what he is, a Jew is simply capable of anything! Incidentally, in view of the fact that McWilliams' pet thesis has long been subscribed to by "bourgeois," let alone Marxist, investigators, it is difficult to account for his statement that "the inadequacy of social theory in relation to this crucial problem is a scandal for which every social scientist in the U. S. should feel ashamed."

2) Although the author outlines the customary libertarian program of civil, legal, political, and economic weapons to combat prejudice and discrimination, the underlying assumption conveyed is that reformism alone will not suffice. What is needed is a democratic, noncompetitive society, one ostensibly approximating the success of the USSR where "the physical and economic security of the two million or more Jews may be taken for granted." No one need argue at this late date that scapegoatism is correlated with economic insecurity, social conflict, and political instability. Furthermore, whether any society, no matter how enlightened, can ever completely cope with the problem of emotional deprivation so that no individual experiences aggressive impulses remains for the future. What we cannot permit to pass at this point, however, because of the important relation between societal structure and intolerance is McWilliams' perpetuation of another Soviet myth.

Contrary to popular opinion, the USSR is not free of anti-Semitism. That the voluble defenders of Russian policies throughout the world point to a democratic constitution instead of to daily realities as incontrovertible evidence is another example of what Silone in another connection has called legalistic "cretinism." Space does not permit any elaboration of this much-misunderstood problem; but the interested reader may consult such authoritative sources as Schwarz, Nomad, and Lestchinsky, as well as the reports of refugees and displaced persons and of the Worldover Press concerning the growth of anti-Semitism within the USSR and its satellite nations. What exists at best in the Soviet Union is ethnic equality; this means that the Jews, like other minorities, are subject to similar privileges and indignities. Ethnic democracy, or the political freedom of a group in its relation to the state, the Jew certainly does not enjoy.

Finally, since McWilliams' cure for anti-Semitism lies in a radical transformation of our society, to be effected in large measure, no doubt, by a political party, one wishes that he were not so nebulous on this subject and that he would for sake of programmatic clarity indicate what existing organization is most capable of furthering his socialist objective.

George Kimmelman

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PHYSIOLOGY FOR BEGINNERS

The Machinery of the Body. (3rd ed.) Anton J. Carlson and Victor Johnson. xxi + 639 pp. Illus. $4.50. Univ. of Chicago Press.

THIS book, the first edition of which appeared in 1937, is designed to serve the beginner in the science of physiology. The authors are eminently well qualified for the task. Dr. Carlson, who is renowned for his work in experimental medicine, is professor emeritus of physiology at the University of Chicago. Dr. Johnson, who was at the time of the first edition a member of the same department, is now director of the Mayo Foundation for Medical Education and Research.

With the background available to the authors it is not surprising that this textbook should be accurate and remarkably inclusive. Constantly emphasized are such important aspects of science as the close relationship of physiology to chemistry and physics. The importance of the experimental method is stressed throughout. As befits the orientation of scientific thought today, deficiencies of knowledge, and problems requiring further research, are frequently pointed out.

Long familiarity with scientific terminology has led the authors to assume an unusually large vocabulary even for the college level. It is undoubtedly true that the vocabulary is expanded and enriched by stretching for words; nevertheless, some simplification in this respect might not be amiss, and the incluson of a glossary of terms for the scientifically untrained reader would greatly facilitate reading the book.

From a mechanical point of view, with the possible exception of illustrations, this book is well done. The type is clear and large. There are few misprints—"mecury" on page 193 is one spotted by this reviewer.

There is no doubt that this is one of the best books available for the intelligent reader who desires to learn something of the workings of the human body. Any educated person cannot help but enjoy reading this book. Indeed, it contains much of value for the scientist who already has some familiarity with most of the topics presented.

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THE SOCIAL INSECTS

Our Enemy the Termite. (Rev. ed.) Thomas Elliott Snyder. xiii + 257 pp. Illus. $3.50. Comstock.

SOME advances have been made in the fields of termite biology and control since the first edition of this book appeared in 1935. The revised edition presents much of this new material, especially that in the field of control. Many new and excellent illustrations have been added.

The first section of the book, dealing with the biology, taxonomy, and paleontology of the group is

generally good. Dr. Snyder has changed his position on the determination of castes to the inhibition theory, which is in line with the most recent experimental findings. I feel, however, that there are several portions of the discussion that do not present the most recent aspects of caste development and where the presentation of the facts is confusing. I refer mainly to the sections dealing with Reproductive Forms and Intermediate Reproductive Forms. It seems unfortunate that the more recent view regarding the reproductive forms, which is strongly supported by experimental evidence, is not also presented. This view holds that there is only one type of true reproductive caste, the alate (the "first form" reproductives of Snyder). All other individuals functioning as reproductives are supplementary forms, arising from immature nymphs, either wing-padded or apterous (the "second form," "third form," and "intermediate" reproductives of Snyder).

The second section of the book presents an honest picture of termite damage and critical suggestions for the prevention and control of termite attacks. It includes a discussion of the new insecticides, as well as those which have been utilized for many years. The keys for the identification of the genera and subgenera of the Isoptera occurring in the United States may prove difficult for the uninitiated. The usefulness of the book is greatly increased by the extensive glossary, which should be a requisite for all such volumes.

The value of the book will be self-evident. It has seemed most important however, to point out a few weak points upon which those wishing to inquire further may be well advised to refer to the work of the investigators mentioned in the text.

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THE ANATOMY OF PHILOSOPHY

The Basis and Structure of Knowledge. W. H. Werkmeister, xi + 451 pp. $5.00. Harper. New York.

THE subject indicated by the title of this book is one of the most obscure in the vast literature of philosophy. Let it be said at the outset that the author has labored earnestly, and always with scholarly ability, to clarify the topics treated with due reference to the developments of the last century in the physical sciences and mathematics. The point of view of this volume is indicated in the introduction, in which the author finds that the position of the "logical positivists" is on the wane. The present book consists chiefly of an elaboration of certain ideas previously expressed in the author's *Philosophy of Science* and other writings. The contents are organized into four principal sections as follows: Language and Meaning; Truth and the World About Us; Formal Knowledge; Empirical Knowledge. The entire second half of the book is devoted to a discussion of the nature of mathematics and scientific method, laws, and principles.

As may be surmised from the Table of Contents, all the knottiest problems of philosophy, and especially of epistemology, are encountered in this volume. An adequate discussion of the author's handling of a single one of them would go far beyond the limits of this review. Suffice it to say that, in my opinion, the basis for the interminable difficulties with which this book struggles is the reluctance to abandon the ancient dualism of "mind" and "matter," to use the traditional terms. If one wishes to adopt this dichotomy (however disguised), certain consequences doubtless follow and there is little reason to quarrel about them. It becomes rather a practical question of what it shall profit us in the solution of certain problems of living on the earth to adopt positions of the type reflected in such passages as, for example, the following:

> The moral and social obligations to which scientists must and do submit transcend the realm of empirical research to find their justification and sanction, not in laboratories and scientific methods, but in a realm of values—human and humanitarian—which can neither be scientifically analyzed nor reduced to functional equations (pp. 331–2).

> Any science, therefore, which is concerned with the characteristic features of "mind" as they are revealed in the "modes" of experience deals with a subject matter which is not in the same universe of discourse with the content of experience which is the subject matter of the natural sciences. Hence, to the extent to which psychology is concerned with "mind" in the sense here defined, it indicates a new dimension of investigation and can never be included in the integrated and "closed" system of the natural sciences. The categories of the "external" world, and the principles and "laws" which integrate the *content* of experience, are not of the type required for the understanding of the *"modes" of experiencing* that content (p. 418).

It is true that the author definitely disclaims (p. x) any intention of dealing in this volume with the social sciences (which he promises to do in a future work), and these few passages in a book devoted mainly to other subjects should, therefore, not be too greatly emphasized. I quote them chiefly to characterize the viewpoint of the book rather than to imply that the contents are mainly on this subject. Also, I believe that the viewpoint reflected in these passages underlies in a fundamental way all the obscurities and inadequacies which characterize the whole volume. In the final analysis, science and philosophy are both a kind of social behavior, and the epistemological problems that have always bedeviled the subject of philosophy can, in my opinion, be resolved only when a more adequate science of human social (including verbal) behavior is developed. But if we assume to begin with the impossibility of an empirical science of this subject matter, then I fear philosophers will

continue to pursue epistemological problems in a perpetual circle.

The fact remains that the author has written a scholarly treatise from the viewpoint he has elected to espouse. Those who find this viewpoint a profitable and an illuminating way of looking at, and verbalizing about, certain problems of philosophy will doubtless find this an excellent text. Nor should the critical remarks made above be allowed to distract attention from the wide range of interesting material and reasoning the author has assembled and, of course, the many excellent discussions with which, I suppose, nearly all would agree. There is an excellent bibliography and an index.

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EVERYMAN'S COMSTOCK

The Insect Guide. Ralph B. Swain. xlvi + 261 pp. Illus. (by Susan N. Swain). $3.00. Doubleday. Garden City, N. Y.

SELDOM does one have the pleasure and satisfaction of reading a book so well suited to the purpose for which it was written as *The Insect Guide*. Dr. Swain begins by giving in tabular form the differences between the Insecta and the other arthropod groups—Palaeostraca, Crustacea, Arachnida, Diplopoda, and Chilopoda—all of which are figured. The Onychophora are omitted, probably because they are not regarded as arthropods or because they are not found north of Mexico. Following this are a brief but well-written and adequate account of insects in general, and another of their structure, the latter accompanied by good figures of their essential features.

The main body of the work consists of notices of the more important families of insects in North America north of Mexico. The selection of families and of the examples chosen to represent them is excellent. An enormous amount of information is given, written in a condensed yet clear and attractive style. This information has evidently been very carefully checked, for there are unusually few statements to which anyone could take exception, and these are chiefly in the account of the butterflies. The nomenclature has been brought thoroughly up to date.

The illustrations are exceptionally good, from the technical as well as from the artistic viewpoint. A few of the colored figures were prepared from faded or discolored museum specimens, which is inevitable, and in some the color reproduction is not too good, which is not the fault of the artist. In the figures of butterflies and moths there are a few minor inaccuracies—for instance, in Figures 83a and 84 the forewings are too pointed—but these are negligible. Figure 83b shows both *Colias philodice eurytheme* and one of the yellow subspecies.

The section on collecting, preserving, and studying insects is comprehensive, well written, and well illustrated. An interesting innovation is the tabulation of the insects, with figures, on the end papers.

Dr. and Mrs. Swain evidently devoted a vast amount of time and thought to the preparation of this book. It is no easy task to gather such an immense amount of material and then to sift it and present it in such excellent form. They are to be congratulated on having produced a book that represents a notable contribution to the literature on popular entomology.

Austin H. Clark

Smithsonian Institution

LITERARY FLIGHTS

Voyages to the Moon. Marjorie Nicolson. xii + 297 pp. Illus. $4.00. Macmillan. New York.

JOHN WILKINS said in 1638, "there are four several ways whereby this flying in the air hath been, or may be attempted. . . . By spirits or angels. By the help of fowls. By wings fastened immediately to the body. By a flying chariot." The first part of Dr. Nicolson's scholarly and amusing book discusses the literature of man's attempts to fly from the earliest recorded Chinese and Greek legends; Daedalus and Icarus are familiar to everyone.

The "new astronomy" of the early seventeenth century, the turn from the old superstitious way of thinking toward a more nearly scientific way, engendered a whole new literary trend. Science does stimulate "the minds of writers of fiction." Then the tales of flights—cosmic and terrestrial—drew away from the supernatural voyages such as Kepler's *Somnium* and Francis Goodwin's *Man in the Moone*. Bacon, Wilkins, and others wrote of attempted flights by artificial wings, and such satirists as Samuel Johnson in *The Dissertation on the Art of Flying* made merry. The idea led gradually to the "flying chariot" enthusiasm of the eighteenth century. In 1705 Daniel Defoe in his *Consolidator* played with the "theme of a world in the moon." Swift's Flying Island in the third book of *Gulliver's Travels* marks the "literary climax of cosmic voyages by means of flying chariots."

Later in the book, Dr. Nicolson shows what "certain fanciful, whimsical, sometimes poetic minds made of the planetary voyage." There is "Endymion's way, by rapture in sleep, or a dream" and Cyrano's "translation of Adam" and "the idea of the separation of soul from body" in Gabriel Daniel's *A Voyage to the World of Cartesius*. In mid-eighteenth century Voltaire published his *Micromégas* which may be the parent of today's pseudo-scientific stories of interplanetary activities. For finale, there is Carroll's *Alice in Wonderland*, which, we never before realized, is a cosmic voyage. Jules Verne, H. G. Wells, and other "moderns" appear in the epilogue.

The book is the light avocational work of a well-known educator of distinction. The material was collected over a period of years while the author was

actively engaged in more serious, though related, matters. It has an extensive bibliography and complete index but is not intended to be an exhaustive treatment of the subject. It is meant to be entertaining, and it succeeds in being informative as well.

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BRIEFLY REVIEWED

Mathematik, Logik und Erfahrung. Victor Kraft. 129 pp. $2.40. Springer-Verlag. Vienna, Austria.

Most of what Professor Kraft has to say will be familiar to English-speaking students of the philosophy of mathematics. The author is particularly concerned with the empirical validity of arithmetic and geometry, and likewise for logic. He also attacks "conventionalism," this in the sense, for example, that it is a pure convention whether empirical space is Euclidean or non-Euclidean (Russell). Possibly the matter in dispute is one of those things in which no meaningful conclusion can be reached. This, however, does not detract from the interest of the author's presentation.

E. T. BELL

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Making Friends with Birds. A. F. Park. xi + 216 pp. Illus. $6.00. Chatto & Windus. London. Macmillan. New York.

This book is not a contribution to technical knowledge of birds, but is concerned with the author's adventures with various common English birds, and his close-range observations of them. Unlike most photographers, he has preferred to work with birds for which no "blinds" or "hides" were needed. This has resulted in the absence from his book of the shyer, more timid, and hence less well-known, species that many photographers seem to consider more interesting or, from the standpoint of photography, more challenging; but it has given him the opportunity of making more extended and more intimate recordings of his subjects. The book is written from the viewpoint of the photographer approaching birds as material, rather than that of the naturalist using the camera as a field tool. The photographs are superb, and the observations in the text are remarkably good, especially when one considers that pictures and not facts were the author's prime goal. Like most popular nature books, there is an element of sentimentality and anthropomorphism in the presentation of the data, but less so than in many books written by men who were primarily naturalists and only secondarily manipulators of cameras.

Some 25 species of birds are treated and pictured in as many chapters. Following these are two chapters dealing with what the author calls the "photographic aspect," in which he gives advice, based on his experience, to prospective photographers of birds' nests, of fledgling birds, and of adult birds, and the differences in the problems each of these categories presents. He ends by giving in tabular form the camera, lens, exposure, f no., emulsion, filter, and lighting used in each of the 180 photographs in the book.

HERBERT FRIEDMANN

U. S. National Museum
Washington, D. C.

The Farmer's Handbook. John M. White. xvi + 440 pp. Illus. $4.95. Univ. of Oklahoma Press. Norman.

I haven't read this book from cover to cover, for it is not the kind of book that you read that way. I have, however, examined it rather carefully and find that it contains a great wealth of information. Naturally, in view of the size of the book and the number of subjects on which it furnishes information, it cannot go into great detail. On a number of subjects on which I have had occasion to check the statements made from the standpoint of accuracy, I have found the information reliable. The book is well indexed, and I think it is likely to be well received by those who may wish a reference book of this type.

V. R. GARDNER

Experiment Station
Michigan State College

The Rescue of Science and Learning. Stephen Duggan and Betty Drury. 214 pp. $3.00. Macmillan. New York.

In 1933 the Emergency Committee in Aid of Displaced Foreign Scholars was organized to assist those scholars who were being driven from German and other European laboratories, libraries, and lecture halls by the mounting fury of Nazi intolerance and persecution. The work of this Committee, directed by the two authors, who are therefore competent to write its history, ended in 1945, and this book is the record of its accomplishments. It is a record in which the United States, its universities, its foundations, and a host of individual contributors may well take pride.

Several thousand scholars were forced to flee from Germany and other Nazi-dominated lands, but not all of these came to the United States. Of those who did seek refuge here, 335 were directly aided by the Committee. This final report therefore tells not only the administrative history of the Committee but also attempts to describe the adjustment of the displaced scholars to their new environments, their contributions to the war effort, and their fields of specialization. It lists the institutions where they were invited to work, together with much other interesting data.

The dry crusts of statistics have been made palatable by being dipped in the milk of human kindness, and the authors and the members of the Committee are to be commended for an inspiring account of a great humanitarian enterprise.

MORRIS C. LEIKIND

Library of Congress

Half Hours with the Great Scientists. Charles G. Fraser. xx + 527 pp. Illus. $6.00. Reinhold. New York.

In recent years there have been published a number of histories of science, some of which have called themselves "stories." For the book under review this title is quite appropriate, for in it the humanistic element is predominant; emphasis is laid upon the attitude of the scientific discoverers—their aims, their points of view, and their methods. The book contains many illustrations, of which thirty-eight are portraits. Moreover, the text contains frequent flashes of humor.

This does not lead to a neglect of the scientific side of the subject. The fundamental facts and principles of physics are classified under five headings, and in each class special attention has been given to such features as might appeal to beginners in physics. As a result, the book as a whole can be recommended as excellent collateral reading for students in physics throughout their whole course of study.

As an instance of some of the interesting things to be found in a study of the scientific methods available to the ancients, the author cites the Greek system of numerals, and gives an example of what we would call simple problems in subtraction and division. The author says: "As we struggle with the intricacies of this computation, our admiration of the mental power of Archimedes, 'The Reckoner,' increases by leaps and bounds."

The lay reader of this book will doubtless find it necessary to omit much of the mathematics in its pages and confine his reading to its narrative and descriptive portions; but, even so, he will certainly gain from it a better understanding of the events that he observes in nature, and will be drawn into closer rapport with the scientific age in which he lives.

PAUL R. HEYL
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Island Life in Lake Michigan. Robert T. Hatt, Josselyn Van Tyne, Laurence C. Stuart, Clifford H. Pope, and Arnold B. Grobman. xi + 179 pp. Illus. $4.00. Cranbrook Inst. of Science. Bloomfield Hills, Mich.

This is a concise and informative study of the mammals, birds, reptiles, and amphibians of the Lake Michigan islands lying off the northwest shores of the Lower Peninsula. Following brief accounts of the recent geological and cultural history of the region is an extensive annotated list of the vertebrates (other than fishes). Short but provocative chapters on modifications of habits and on factors of distribution as shown by the island fauna will be of particular interest to ecologists. An appendix tabulates the species found on each island. There are a good bibliography and index.

LORUS J. and MARGERY J. MILNE
Department of Zoology
University of New Hampshire

The Story of John Hope. Ridgely Torrence. 398 pp. Portrait. $5.00. Macmillan. New York.

All Our Years. The Autobiography of Robert Morss Lovett. x + 373 pp. $3.75. Viking. New York.

The science of education in America found two stout protagonists in John Hope (1868–1936), the fair, blue-eyed, blond Negro president of Morehouse College and Atlanta University, and Robert Morss Lovett (1870—), beloved adjunct of the University of Chicago. Their biographies, listed above, are among the bright spots of the 1948 book crop. Though wide apart in many ways, the careers of these near contemporaries have their parallels. Both were lifelong university men and inspiring teachers, yet both crowned their lives with extra-scholastic service—Hope in the field of Negro advancement and race relations, Lovett in the cause of liberalism and in the public service. Lovett had the fortune to be able to write his own life story, and he has done it superbly. John Hope did not live to write his own biography, but he has had remarkably good fortune too in the fact that so sympathetic and intelligent a person as the distinguished poet Ridgely Torrence undertook to do it for him. Both of these books attain a literary excellence noteworthy in this day of slapdash. Both deserve a wide reading by social and political scientists and educators.

PAUL H. OEHSER
Smithsonian Institution

International Rules of Botanical Nomenclature. Compiled from various sources by W. H. Camp, H. W. Rickett, and C. A. Weatherby. 120 pp. $3.50. Chronica Botanica. Waltham, Mass. Stechert-Hafner. New York.

This is a second printing, by the offset process, of this important compilation. The unofficial special limited edition (originally published in Brittonia **1**: 1–120. 1947) was prepared under the auspices of the American Society of Plant Taxonomists as a service to its members. The need was very great because, owing to war conditions, the original edition was no longer available. Now through its reproduction it becomes available to all investigators who may have need to consult it. Its issue in its present form is most timely in view of the active preparation of data to be considered at the Seventh International Botanical Congress to be held in Stockholm in 1950. This critical compilation is, in many respects, superior to the original Leipzig edition, notably in the complete and critically indexed list of conserved generic names. The addition of critical data in the form of footnotes is another innovation that is highly commendable, for these data tend to clarify certain entries, and call attention to various items needing further official consideration. This is a volume that should be on the desk of every working taxonomist.

E. D. MERRILL
Arnold Arboretum

CORRESPONDENCE

RUPPIA BALLS

The circumstances which led to the publication in THE SCIENTIFIC MONTHLY of Dr. Essig's paper[1] on "The *Ruppia* balls of Little Borax Lake" had an unusual concomitant. The *Ruppia* balls were brought to the office of the University of California herbarium for identification and called to the attention of Miss Annetta Carter, senior herbarium botanist. Miss Carter, because of her wide botanical knowledge and experience, is often able to give valuable suggestions that can put the bewildered botanist or paleobotanist on the right track. In the search for seeds or other objects that might lead to their identification, the balls were shaken on a piece of white paper and the material examined under a microscope. In the material so obtained were many tiny, semitransparent, oval objects with one, or usually two, dark spots symmetrically located in the axis of the oval, somewhat after the manner of *Ailanthus* fruits.

The writer is completing a monograph of the fossil plants at Florissant, Colorado. Among the unsolved mysteries of the flora were the objects called *Ranunculus florissantensis* Cockerell.[2] It was seen early in the study of the flora that these fossils could not be those of *Ranunculus* seeds, but their true relations were a puzzle. The Florissant material is on thin, light-colored slabs of volcanic lake shales and consists of many hundreds of small brownish dots scattered over the surface of the bedding planes. These dots, when examined with a microscope, show an oval shape with a stemlike extension, in appearance like a thin membranous wing or semitransparent case, in which are embedded two (or, more rarely, one) round, seedlike objects. They resemble the seeds of members of the Bignoniaceae, but certain obvious differences render them distinct.

It happened that on the day the *Ruppia* balls were brought to the herbarium the writer also took his Florissant fossils to Miss Carter, hoping for a hint as to their identity. She examined them under a binocular microscope and at once recognized their correspondence to the small objects from the Borax Lake *Ruppia* balls. The fossils had reposed in the Colorado mountains for some twenty million years and very briefly in the museum of paleontology at Berkeley, waiting to meet their living counterparts in the University herbarium on one particular day in the summer of 1948—a coincidence almost beyond believing. Both the recent and fossil materials were brought to the attention of various specialists in entomology and botany, but no one recognized them. Finally, they were sent to the office of the California Fish and Game Commission in Berkeley, where they were identified as the egg cases, or ephippia, of the tiny, fresh-water crustaceae called water fleas (*Cladocera*), probably belonging to the genus *Moina*.[3] Thus *Ranunculus florissantensis* must be changed from the plant to the animal kingdom and the naming left to some competent biologist. The identification was due to the fortunate meeting of a slab of fossil shale from Florissant, Colorado, and a *Ruppia* ball from Little Borax Lake, California

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REFERENCES

1. ESSIG, E. O. The *Ruppia* Balls of Little Borax Lake. *Sci. Mon.*, 1948, 66, 467–71.
2. COCKERELL, T. D. A. A Fossil Buttercup. *Nature*, 1922, 109, 42–43.
3. WARD and WHIPPLE. *Fresh Water Biology*. New York: Wiley, 1918. P. 705.

Professor E. O. Essig's article "The *Ruppia* Balls of Little Borax Lake" in the June issue of THE SCIENTIFIC MONTHLY was quite interesting in that it described a natural formation of fibers very similar to that encountered in a pilot-plant operation at Armour Research Foundation of Illinois Institute of Technology. In the pilot-plant operation, a dilute slurry, or suspension, of various fibers such as wool or cotton is agitated in a tank. It was found that under certain conditions of agitation the fibers instead of remaining evenly distributed in suspension would roll up into compact balls resembling very closely the *Ruppia* balls shown in Professor Essig's article. Such ball formation was undesirable in the pilot-plant operation, and considerable study was necessary before their elimination was accomplished. It was found that small localized eddies which moved the fibers about in a circular pattern started the formation of the balls. Once the formation started the balls rapidly increased in size by rotating and winding up additional fiber. Elimination of the ball formation was accomplished by changing the agitation so that small eddies were prevented. In view of the above laboratory duplication, we believe Professor Essig's conclusion that the *Ruppia* balls are formed by wave action is essentially correct.

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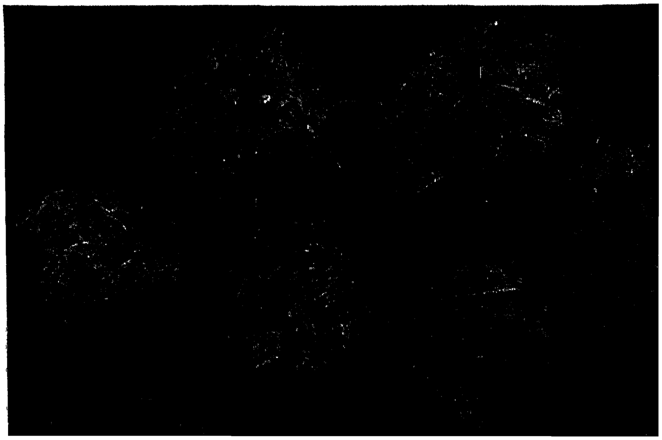
Fiber ball formations in the laboratory.



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SCIENCE IN THE SERVICE OF AGRICULTURE*

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Chief, Division of Plant Pathology and Botany, University of Minnesota, Agent, U. S. Department of Agriculture, and 1949 President, American Association for the Advancement of Science, Dr. Stakman has recently returned from a scientific mission to Japan. A group composed of five American scientists reviewed, under the auspices of the National Academy of Sciences, progress being made by Japanese scientists.

AGRICULTURE is fundamental to human subsistence, an obvious fact too often ignored by our lawmakers, educators, and the general public itself. A primary service that science must render to agriculture is to emphasize the importance and peculiar character of this basic industry, for indeed man is dependent on plant growth—on photosynthesis—for his very existence on earth. All his food and much of his raiment and shelter come either directly or indirectly from plants. Animals, important though they are, are essentially transformers of plant products and not primary producers. Modern industrial civilizations could not exist without the products of mines and factories, and man himself could not exist without the products of our fields and forests.

Because farming is a complex biological enterprise, subject to tremendous climatic and biotic hazards over which the individual farmer can exercise little or no control, a successful agriculture needs the services of all science. Government-supported science is especially needed because in most countries farming is carried on in many relatively small units. By its very nature, farming cannot be concentrated like processing and manufacturing. In the United States alone there are about 6 million farms distributed over an area of about 3 million square miles, with so many kinds and combinations of plants, animals, soils, and climates that nearly 500 types of farming can be recognized.

Hundreds of kinds of plants are extensively grown, and thousands more are grown to some extent. More than 27,000 Latin-named species and varieties are listed in Bailey's *Standard Cyclopedia of Horticulture*. Everyone knows something about varieties of apples, including differences in size, shape, color, texture, flavor, and keeping quality, and that there are numerous varieties of most other fruits, vegetables, and flowers. The differences between varieties of virtually all our cultivated plants are important both to the producer and to the processor, even though the bulk of consumers do not distinguish between them. Wheat is not simply wheat. There are a number of major botanical groups, such as the common bread wheats, the macaroni, or durum, group, the club wheats, and others. And there are smaller groups within the major ones. Within the common bread wheat group, for example, there are distinct market classes, such as hard red winter, soft red winter, and hard red spring, each with its peculiar growth habits and commercial uses. Within these classes, in turn, there are many commercial varieties; in 1935 the Department of Agriculture listed more than 200 varieties of wheat as being grown in the United States. Some of these may look very

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much alike but yield flour of quite different quality; some may look almost exactly alike but differ greatly in susceptibility to plant diseases. Varieties of flax may be similar in appearance but different in resistance to diseases or in amount and quality of oil produced. Discriminative choice and scientific applications play decisive roles long before farm products reach the market place.

Differences in varietal appearance and quality are so obvious in many plants as to be common knowledge; but few people realize that the grower must know the special soil and climatic requirements of the varieties that he grows and the special uses to which they may be put. Varieties often are suited only to very special soil and weather conditions, and the range of adaptability cannot be determined readily by the individual grower. There still is much to be learned about the physiology and pathology of a single kind of organism, and the problems often are complex and difficult of solution. Plant scientists must solve problems not only of many kinds of crop plants but those of dozens or hundreds of varieties within each kind. Animal scientists are confronted with similar situations. The almost bewildering complexity of plant and animal materials produced, and the wide variety of soil and climatic conditions that affect their production, obviously create numerous difficult problems for the scientist to solve.

Soil and climate are basic factors in agriculture because they determine the amount of food and feed that can be produced. Agriculture can be supplemented somewhat by aquiculture but only to a limited extent. Whether considered on a world-wide, nation-wide, or farm-wide basis, the soil is man's most valuable single asset. Unfortunately, its nature has too long been misunderstood, and it has therefore been lavishly squandered. The total amount of agricultural land in the world is neither unlimited nor inexhaustible. Intelligent land use, based on scientific facts and principles, is therefore of primary importance.

It is platitudinous to say that soil is extremely complex, physically, chemically, and biologically. And yet only those scientists who have studied soils intensively can realize how complex they really are and how rapidly they can deteriorate, or even disappear, under poor management. The formation of soils is a long process, including volcanic and glacial action, the weathering of many kinds of rocks, the effects of various kinds of plant growth, and the activities of myriads of microorganisms, often extending over centuries of time. And all too often man destroys in a few short years the soils that nature has so laboriously built by a long, slow process of evolution.

Soil conservation is one of the most important nation-wide problems in the United States. The present rate of soil deterioration in some of the richest agricultural regions is clearly a matter for alarm. This is no partisan or hyperbolic statement; it is based on official statistics and on extensive personal observations. The story of man's vandalism is written for all to see, not only in the older parts of the country but in some of the newer areas where the forest and prairies and plains have only recently been transformed into croplands, and still more recently into badlands. Nature's terrible drama of the dust bowl became so shocking a human tragedy as to force even the most apathetic into a realization that man cannot violate natural laws with impunity. The economic and sociologic consequences of a slower and more insidious debilitation of the soil were dramatized by the author of *Tobacco Road.* There are too many Tobacco Roads in the United States. Too many soils have lost their granular structure and have become as unproductive as so much cement. Water has leached out the fertility of many soils, and sheet erosion has carried the soil itself away from hillsides and the tops of gently undulating lands. One of the most distressing sights from an airplane is the endless succession of bald spots on many once-rich agricultural lands. Nature punishes man severely for his ruthless destruction of the natural vegetational cover that helped build and protect the soil. Thousands of acres of fertile fields have been so badly cut and gashed by gullies as to make them impassible to man and beast without bridging. In many areas gullies 10–100 feet deep have ruined thousands of farms within the lifetime of many of us.

Realism and common sense are beginning to replace the visionary optimism of the boomers and boosters who had the naïve conceit that land in the United States was unlimited in quantity and imperishable in quality. Time was when land was so plentiful that it could be exploited without endangering the national welfare. But that time is long past.

The pioneers who cut the forests, plowed the grasslands, drained the swamps, established the cattleman's empire, and discovered the sheepman's paradise aided in the phenomenal development of the United States. But they reckoned too little with the future and the penalties involved in the destruction of 50 million acres of cropland and the virtual destruction of an additional 50 million: 100 million acres of land—enough to feed and

clothe 50 million people moderately well. Soil scientists of the Department of Agriculture estimate that water and wind remove annually, from croplands and pastures, about 3 billion tons of soil, containing about 43 million tons of phosphorus, nitrogen, and potassium, which is fifty to sixty times as much as is replaced each year by commercial fertilizers.

We now have in the United States about 400 million acres of cropland, exclusive of grazing lands not suitable for cultivation—about enough to feed and clothe 160 million people with our present standard of living and productive efficiency. Much of this land is subject to erosion. Common decency to the nation and its future generations demands that everything possible be done to protect and preserve this national resource, and that it be done promptly and scientifically.

In 1936, in an address delivered at the 164th dinner of The New York Farmers, I made the following statement:

Agriculture is a far more precarious undertaking than most other enterprises, because it is biological in nature, and various factors over which the farmer has almost no control can very quickly destroy not only the crops that he tries to produce but even the soil on which he produces them. A study of these factors and of the means of reducing their devastating effects is one of the principal obligations of research.

One of the primary needs is a body of facts to serve as a guide in a sane and sensible land utilization policy. Everyone knows that many good forest lands have been converted into poor farms, that many good range lands have been converted into still poorer farms, that many good swamps have been converted into nothing except fire and flood hazards. These marginal lands should either be taken out of production or the type of agriculture should be adapted to existing conditions. It sometimes is said that research, designed to help solve problems on these marginal lands, is wasted. But what about the people living on these lands? They are there. Are they to be abandoned to their fate, merely because they were misled into settling these lands by the need for increased agricultural production and the lure of high prices? We hear much about marginal lands and even about marginal farmers, to say nothing about marginal scientists. The criterion of what constitutes marginal land is a shifting one. When many of these lands were settled they were not marginal, because prices were high and it was possible to operate them profitably. They are marginal now, but will they be ten years hence? Whether they are then marginal lands, sub-marginal lands or above-marginal lands will depend partly at least on prices paid for what is grown on them. If they produce ten bushels of wheat an acre, they are sub-marginal if the price of wheat is only forty cents a bushel; but if the price is $1.25, they may be profitable. It is evident, of course, that extensive areas should never have been ploughed, because they yield good crops too infrequently altogether. But I want to protest against the scornful criticism that often is levelled against the people who ploughed them. Their mistake was in believing those who painted the lurid pictures of the new Canaans where milk and honey and gold flowed from the earth below and the sky above. Does land classification and zoning to prevent similar mistakes in the future transcend the legitimate functions of government and interfere too much with personal liberty? Mistakes were made, but the settlers themselves were not wholly to blame. In any case the problem is to correct those mistakes as scientifically as possible Some readjustment is necessary, but it should be made as painless as possible

The concept regarding marginal lands was not long in changing. The tremendous demand for food and other agricultural products for ourselves and other peoples stimulated efforts to make all lands yield to maximum capacity, even at the risk of serious soil impoverishment. Phenomenal wartime agricultural production was attained partly because of providential weather, for which man can take no credit, and partly because of the industry of the farmer and his intelligent use of the tools given him by science and technology.

World War II led to stocktaking of past progress and present agricultural and industrial potential. For the present, the result was reassuring. As concerns agriculture, it seems clear that man's constructive efforts can counterbalance his destructive practices if he puts common sense, science, and technology to work. Comparison is made between the two world wars for the obvious reason that in both every attempt was made to utilize all available knowledge, skills, materials, and technics to attain maximum production.

During the war years 1942 to 1945, inclusive, American farmers produced about 2 billion bushels more corn than would have been possible during four years of World War I; in a single year of World War I stem rust destroyed about 300 million bushels of wheat in the United States and Canada, but losses were comparatively small during all of World War II; it is estimated that the efficiency of animal production during World War II was 25 percent higher than in 1919; the use of farm machinery in World War II released enough land previously used for work animals to feed 16 million cattle or 26 million hogs, so badly needed for dairy products and beef and pork; an idea of the number of men released for other types of national service by the mechanization of farming can be gained from the fact that in 1900 the production of 100 bushels of wheat required 108 hours of man labor and only 47 hours in 1940. This progress was made possible almost entirely by utilizing the results of research and invention.

It is impossible—probably even undesirable—to try to apportion credit to the various guilds of

scientists and technologists who accumulated the facts, produced the materials, or elucidated the principles that helped make the truly marvelous agricultural production of World War II possible. True it is, however, that the production of superior varieties of crop plants, the selection of better breeds or lines of farm animals, wider knowledge and better practices with respect to soil conservation, fertilization, and tillage, greater attention to the principles of plant and animal nutrition, more efficient control of plant and animal diseases and pests, and more extensive use of farm machinery all contributed their share. Important also is the fact that more knowledge had accumulated regarding the prevention of deterioration of foods and feeds and better methods had been devised for processing and preserving them. It is impossible to discuss in detail the contributions made in each of the various fields; only a few examples can be given. The resulting lack of balance is unfortunate but almost unavoidable.

Corn, wheat, and oats are outstanding examples of crop plants that were so improved as to contribute greatly to agricultural production during the war years. There are many others, but these illustrate progress attained through the application of the principles of genetics to plant improvement.

Without hybrid corn there could have been no 3.2-billion-bushel crop in 1946. This corn helped feed 80 million cattle, more than 44 million hogs, and enough chickens to lay 55 billion eggs. The 600 million extra bushels that go to the credit of hybrid corn in 1945 and the 2 billion during the war years constitute an astonishing record. But more amazing is the scientific record, especially the rapidity of the progress after the underlying principles were developed and applied. Experiments in crossing varieties of corn were made as early as 1881, but inbreeding experiments were begun as late as 1905, and the crossing of inbred lines about 1908. As corn is normally cross-pollinated, there was a popular belief that self-pollination inexorably led to deterioration. Indeed, selfed lines did decrease in vigor, but it was found that surprising results could be obtained by first selfing lines to attain relative uniformity and then crossing them in various combinations. Neither inbred parent amounted to much. But they were good parents: they complemented each other in such a way as to produce superior offspring. The process was carried further, and double crosses were made. As an example, selfed lines A, B, C, D are used as parents; A is crossed with B, and C with D. Then $A \times B$ is crossed with progeny of $C \times D$. It sounds simple, but not all combinations are good. It is estimated that of the 30,000 lines developed by 1939 only 2.4 percent were useful in corn improvement. Some, however, were very good. Corn breeders have learned how to use the results of single cross tests for calculating the potential yielding ability of double crosses, thus enabling them to emancipate themselves from much of the laborious testing that was previously necessary.

Information basic to the development of hybrid corn was originally obtained as a result of natural curiosity regarding the effects of inbreeding. The scientific study of corn genetics and its practical application in breeding followed. The results have enriched the science of genetics and the agriculture of many countries. In the United States only 0.1 percent of the corn acreage was planted to hybrids in 1933; now it is almost 100 per cent in the corn-belt states, with an average increase of about 20 percent in yield over that of the open-pollinated varieties grown 15 short years ago.

In the interim between the two world wars the terribly destructive stem rust of wheat was brought under at least temporary control in the hard red spring wheat region of the United States and Canada through the use of newly produced rust-resistant varieties and the eradication of rust-susceptible barberry bushes. It is true that there were extremely destructive epidemics in 1935 and 1937 because of a series of extraordinary meteorologic events that enabled unusual amounts of rust to spread northward from Texas, and because of the appearance of a new parasitic race of the rust that ended the short but useful career of the best spring wheat variety then grown. Other varieties were ready, however, to meet the new menace, and they helped make the badly needed billion-bushel wheat crops possible. The wheat varieties of World War I never could have equalled that performance. The production of these rust-resistant varieties was made possible only because plant breeders and plant pathologists had learned enough about the genetics of the wheat and of the rust fungus to proceed intelligently in the work of breeding and testing.

The oat varieties of World War I could not have performed as did those of War II, even though the latter barely survived the war. A number of superior varieties were obtained by crossing the varieties Richland and Victoria. Richland contributed stem-rust resistance, and Victoria contributed crown-rust resistance. These new varieties became extremely popular in most oat-

growing states in the early forties because they often yielded 20–50 percent more than the varieties that they replaced. Moreover, they seemed to have eliminated the disease hazard. They served well until the end of the war. Then their demise was almost as rapid as their rise. They are now doomed to oblivion because of their extraordinary susceptibility to a disease that was unknown until they became widely grown. There is genetic linkage between the factors for crown-rust resistance in Victoria oats and those for susceptibility to the new disease, which simply means that if Victoria contributes crown-rust resistance when crossed with other varieties it also contributes susceptibility to the new disease: the bad goes with the good. But breeders and pathologists had been crossing still other oat varieties because they knew that the Richland-Victoria group of hybrids was menaced by parasitic races of the stem-rust fungus that increased in prevalence as the acreage of the new varieties increased.

The varieties White Tartar and Bond contributed rust resistance to the newest varieties that are grown today. Although now performing well, these new varieties are known to be susceptible to some parasitic races of the rust fungi that are present in the United States in only a few regions and in small amounts. As there is precedent for the assumption that these races of fungi may become more widespread and prevalent, still more crosses are being made in an attempt to meet the menace if it materializes.

And so the succession of new varieties continues: New conditions, new varieties, which may in turn bring new problems along with their new values. The improvement in varieties of almost all important crop plants through the application of scientific principles during the past half century has been almost miraculous. Not only have acre yields and quality improved, but the dangers of violent fluctuations in production have been diminished through the development of varieties that resist plant diseases and insect pests and that are better adapted to the soils and weather in the regions in which they are grown.

Science has given agriculture hundreds of improved varieties of plants within the past few decades. But these varieties cannot perform to their maximum capacity unless grown on suitable soils and under appropriate climatic conditions. There is little use in attempting to grow a good variety on bad soil and under weather conditions to which it is not suited. And so we come back to the soil. Without soil there are no plants; without good soil, even good varieties cannot thrive; and on really bad soil even good varieties are likely to be bad; consequently, soil scientists must provide the information requisite to the proper nutrition of the improved varieties.

It is common knowledge that some plants, such as clover and alfalfa, will not grow well on acid soils and that certain kinds, such as blueberries and cranberries, like an acid soil. Well known, also, is the fact that all plants require nitrogen, phosphorus, potassium, calcium, and certain other nutrient elements and that they must be available in proper proportions. Progress is continually being made in determining the peculiar requirements of various kinds of soil and plants.

Plants, however, also require minute quantities of boron, copper, sulfur, manganese, zinc, and certain other elements for normal growth. Only a few parts in a million parts of soil may be necessary, but sometimes they are very necessary. The causes of many mysterious and destructive deficiency diseases of plants have been revealed by increasing knowledge regarding the role of these minor elements. Thus, a dry rot of sugar beets and certain other plants is now known to be caused by lack of boron and can easily be prevented by supplying it to the soil. Likewise, the internal cork of apples, internal browning of cauliflower, top rot of tobacco, and cracked stem of celery can all be prevented by adding small amounts of boron to the soil. A slight excess, on the other hand, is deleterious. Deficiency of copper causes wilting of the upper leaves of tobacco and unthriftiness of certain other plants. The addition of small amounts of manganese to certain soils makes the difference between a good crop of tomatoes and virtual crop failure. Deficiencies of iron, copper, and cobalt in soils are known to retard the development of cattle that feed on plants grown on such soils. Until recently these minor elements were not added to commercial fertilizers, but now, with more precise information, attempts are being made to add them in the proper amounts. The complicated relationships between the minor elements and soil productivity and the effects on plant growth and animal nutrition are strikingly depicted in a book compiled and published by the Chilean Nitrate Educational Bureau, Inc., New York (*Bibliography of the Literature on the Minor Elements and their Relation to Plant and Animal Nutrition,* 4th ed., Vol. I. Pp. 1,037). The abstracts were originally obtained from scientific journals and have been put into one volume for the convenience of investigators. Even casual perusal of the titles and contents of the 10,000

abstracts indicates the complexity of the problems, the magnitude of the work involved in attempting to solve them, and the prodigious amount of information that has already been obtained.

Minute quantities of certain elements in soils may be poisonous to plants or animals. Among them are arsenic, selenium, thalium, and molybdenum. The mystery of the so-called alkali disease is now solved. Beginning about the middle of the nineteenth century, it was observed that animals in certain areas of the range country of western United States became debilitated and often died in large numbers. Many causes were assumed, among them alkali poisoning. It was not until 1928 that it was found that the disease appeared when animals ate grain or forage produced in certain areas. This led to the discovery that the poisonous vegetation contained small amounts of selenium, that concentrations of one part per million might kill animals, and that some plants take up the selenium from the soil and others do not. The selenium-bearing soils are dangerous when there is not enough rainfall or irrigation water to wash out the selenium.

The soil is a vast chemical and physical laboratory and a beehive of microbial activity. Bacteria, fungi, protozoa, and other forms of plant and animal life help make the soil what it is. Microorganisms decompose plant residues, thus furnishing humus; some enrich the soil by fixing atmospheric nitrogen, either as symbionts in the nodules on roots of legumes or as free-living forms; some parasitize the roots of higher plants. There are many kinds, with many kinds of interrelationships. A struggle for existence is going on continuously, with consequent shifts in the relative numbers of each kind of organism. Some are mutually beneficial; others are antagonistic. The total and relative numbers are affected also by soil fertilization, cultivation, and other tillage practices. Soil microbiologists are continually studying the activities of these microorganisms and finding out what can be done to promote the activity of those that help make soil a suitable medium for plant growth.

Even when the best varieties are grown on the best soils, crop plants still are subject to the destructive effects of unfavorable weather, insect pests, and plant diseases. There are violent fluctuations in total production of many crops because of winter injury, droughts, floods, hail, and heavy winds. The principal food crops of the United States are grown in a region in which the weather is continental, with extremes of temperature and wide variations in rainfall. In the upper Mississippi Basin an annual range of 150° in temperature is not uncommon. In this area, the range in a single state has been as great as 160°. Summer temperatures of more than 100° F. are common in many areas. Such temperatures, especially when combined with low humidity and high wind velocity, often cause severe injury to the most heat- and drought-resistant plants. Extremely low winter temperatures under some conditions may cause severe and extensive winter injury to fall-sown grains and perennial forage crops. The terrible drought years of the 1930s created not only visions of deserts but actual deserts. Small wonder that acute problems of agricultural economics and sociology result from such violent fluctuations in production. The annual production of wheat in Kansas has varied from more than 200 million bushels to less than 60 million; that in North Dakota from more than 150 million to about 20 million; that for the country as a whole from more than a billion to less than half a billion. In 1932 we produced almost 3 billion bushels of corn; in 1933, less than 2.5 billion; and, in 1934, only about 1.5 billion. In 1946 the production was about 3.2 billion bushels, but bad weather reduced it to 2.4 billion in 1947.

Although science has not yet learned to control weather, it has helped reduce its destructive effects by providing better-adapted varieties of plants and better tillage methods. The substitution of sorghums for corn in the drier parts of the Great Plains, and of hardier varieties of wheat for less hardy varieties in much of the Great Plains area, has helped greatly to ensure production. The continual introduction or production of better winter-hardy and drought-resistant grains, perennial forage grasses, and legumes is helping to ensure and stabilize production. Even the production of nonshattering varieties of grain has reduced grain losses and made possible the use of combines, which cut and thresh the grain in one operation, thus saving a great amount of labor. The development of varieties that resist unfavorable weather has been of value to production even in those areas where weather is not usually severe. This, and the development of soil-management practices appropriate to the region, have made possible the expansion of agriculture to the western belt of the Great Plains area, thus making millions of additional acres available for producing foods and feeds.

Insects, like weather, may cause devastating damage in a short time. Hordes of grasshoppers have often ruined the crops over wide areas. Chinch bugs, the Hessian fly, the cotton boll wee-

vil, the Japanese beetle, and many other insects can debilitate or destroy plants on a large scale. Great progress has been made in efficiency and economy of controlling many of these insects. The cotton boll weevil, for example, which almost ruined the cotton industry in large areas of the South, has been brought under reasonable control by a combination of suitable varieties and airplane dusting with insecticidal dusts. Varieties of wheat have been produced that are far more resistant to the Hessian fly and chinch bugs than those formerly grown. There has been a remarkable record of achievement in the production of more effective insecticides, such as the well-known DDT and others. But many of these insecticides bring with them new problems, for they may be injurious to certain kinds of plants and may poison the soil when used in large quantities. They may even be dangerous to man when not completely removed from fruits and vegetables. There still is urgent need for extensive research on the better control of insects.

At best the situation with respect to insects is bad enough, but new ones are periodically introduced into the country despite the best protective efforts of Federal and state plant-quarantine services. The introduction of the Japanese beetle and the European corn borer are recent examples of destructive insects that were imported into the United States by man. Scientists in general realize the dangers of introducing new insect pests, and governmental agencies are doing what they can to prevent it. Public apathy, ignorance, or perverseness, however, often nullify their efforts. There must be wider appreciation of the importance of the plant-quarantine regulations.

Plant diseases caused by viruses, bacteria, and fungi are a continual menace to all kinds of crop plants. Some of the most destructive and typically epidemic, such as the rusts of wheat and other small grains and the late blight of potatoes and tomatoes, can spread with frightening rapidity and devastating effect.

As an example, stem rust of wheat and other grains and grasses is caused by a fungus that multiplies by means of several kinds of spores. Those that spread the disease from wheat to wheat are cylindrical in shape and about 0.001 inch in length. On a single acre of moderately rusted wheat there are about 10,000 billion of them, and they can be carried far and wide by the wind. During a period of moderate to strong south winds in 1925, countless numbers were blown northward from Texas, Oklahoma, and southern Kansas and in a week infected an area of 250,000 square miles. The individual farmer is helpless to prevent the spread of these invisible enemies, and so is everyone else. About 40 years ago, plant scientists, therefore, began an intensive search for resistant varieties. Extensive testing and breeding work was done, and a number of resistant varieties were produced, only to lose their rust resistance in some places and at certain times. Intensive study of the rust finally revealed the fact that it was carrying on an extensive breeding program of its own and that there are varieties of stem rust just as there are varieties of wheat, except that the rust varieties are microscopic in size and therefore harder to recognize. Later it was found that within the varieties there are rust races that can be distinguished only by their parasitic effects on varieties of wheat and other grains. More than 200 races of the wheat variety of stem rust are now known. A wheat variety may be immune from some races, moderately resistant to others, and completely susceptible to still others. As the prevalence of races may vary from place to place and from year to year, a wheat variety may be resistant here but not there, and then but not now. Worse still, new races are produced continually by a process of hybridization between existing races, for the rust fungus, like wheat, has a sexual stage. Fortunately, the sexual stage can develop only on a relatively useless plant, the common barberry; hence the barberry-eradication campaign, which has reduced the number of races and delayed the onset of rust early in the growing season in the northern half of the United States. As rust can exist independently of barberries in certain areas of southern United States and Mexico and then spread northward in the spring and early summer, barberry eradication must be supplemented by the use of resistant varieties of wheat and other grains to attain practical control of the rust. It has, however, been impossible to eradicate all barberries, and numerous new rust races are continually being produced, especially in some of the eastern and northeastern states and in certain other areas where barberries are still numerous. Some of these races are much more virulent than those now prevalent in grain-growing areas. It is possible, but not certain, that some of these virulent races may increase, become established, and attack varieties that are now resistant. Accordingly, plant scientists are trying to prepare against possible future emergencies by making extensive series of crosses in an attempt to produce varieties that can resist all races of rust now known, whether important at present or not.

Crop plants are continuously threatened by myriads of pathogenic organisms. There are thousands of species, varieties, and races. More than 3,000 species of plant rusts are known. Each species of pathogen may comprise numerous races, and new ones are continually being produced by natural processes of mutation and hybridization. Even though only a relatively small percentage of the new ones may be more dangerous than those now in existence, past experience shows clearly the imperative necessity for continually being on the alert to detect evidence of the presence of new diseases or new forms of old ones, so that they may not become established before science has provided the basis for controlling them.

Despite many difficulties, great progress has been made in controlling plant diseases. The production and utilization of resistant varieties of plants, even though they may be of only temporary or regional value, have greatly reduced the danger of catastrophe from plant-disease epidemics. Moreover, many diseases can be controlled at least fairly well by appropriate crop rotations, the treatment of seeds and other propagative plant parts with fungicidal chemicals, and the spraying or dusting of growing plants with similar substances. During the past 30 years improvement in fungicides has paralleled that in insecticides. Merely as one indication of the value of devising proper seed disinfectants and methods for applying them is the fact that the treatment of seed corn to prevent the rotting of seed and the development of seedling blights and certain other diseases sometimes results in almost as great an increase in yields as that resulting from the use of hybrid corn.

Man himself has often changed plant-disease situations to his own detriment. Some of the most destructive diseases that occur in virtually every country of the world are there because man brought them there. He brought them on the propagative parts of plants, on packing materials, and in innumerable other ways. The ravages of the chestnut blight, white pine blister rust, citrus canker, and Dutch elm disease in the United States are a few spectacular examples of the danger of introducing into one part of the world pathogens relatively unimportant in their native habitat, which became extremely destructive when taken to a new climatic or biotic environment. Experiences with imported plant diseases strongly reinforce what has been said in connection with insects about the urgent necessity of adequate plant quarantines.

When once soil, weather, and man have combined to produce good plants, as scientifically as possible, the problem of the proper utilization of the plant products still remains. As animal production is one of the most important phases of agriculture, the methods of conversion of feed and forage into meat and milk and other animal products should be as scientific as possible. To detail the progress that has been made in animal nutrition would be impossible in an article of this length. Efficiency is dependent partly on the breeds of animals used and on the way in which they are fed No one needs to be reminded that the scrub cattle on scrub pastures of a few years ago were considered to be doing well to produce 800 pounds of beef in 3 or 4 years. With the modern beef breeds, a steer is expected to attain a weight of 1,000 pounds in 12–18 months, depending on how and what he is fed. It is estimated that the efficiency of animal production has increased about 25 percent since the end of World War I. This is due principally to animal improvement and more scientific feeding.

Continual progress is being made in learning more about the nutritional requirements of different kinds of farm animals and the reasons why feed or forage from certain regions often gives better results than that from others. Phosphorus deficiency is a fairly common disorder, and the addition of 6–14 grams of phosphorus a day to feed for calves and cows in southern Texas has had remarkable results. Animals, like plants, require some of the minor elements and sometimes suffer from deficiency. Copper, cobalt, and manganese, for example, are all needed. Cobalt deficiency is not uncommon in some areas of the United States, particularly in the southeastern states, certain localities of the Lake states, and in the Northeast. The addition of one ounce of cobalt sulphate to each hundred pounds of feed has relieved emaciated cattle from their unthrifty condition. Scientific feeding of animals is becoming more and more scientific.

Evidence is accumulating that crossbreeding of animals may result in improved vigor and quality. Progress is slower in breeding animals than with plants, but progress so far made with beef cattle, swine, sheep, dairy cattle, and poultry shows clearly that desired combinations of qualities can be obtained. New breeds of pigs made to order for lean bacon, or for lard, or for hams of a certain size already have been produced. The Columbia breed of sheep has been developed by the Department of Agriculture as a result of crossbreeding between the Lincoln and Rambouillet followed by inbreeding to fix the type. The new breed combines the mutton- and wool-producing

qualities of the two parents into a single dual-purpose animal. Crossbreeding Zebu and Aberdeen Angus cattle to produce animals that can thrive in the subtropical climate of the Gulf states is in progress. Hybrid corn; why not hybrid animals?

One of the great advances in animal production has been the development of methods of artificial insemination so that the beneficial influence of superior sires may be more widely diffused.

Increased knowledge of animal diseases has kept the disease hazard within reasonable limits in the United States for a number of years. Hog cholera, which in 1897 killed one pig in seven in the United States, is still present but is now controlled by vaccination. Other diseases, such as anthrax, brucellosis, swine erysipelas, tuberculosis, and mastitis, have been kept under reasonable control by a combination of sanitary procedures, the use of vaccines, and of drugs such as the sulfa drugs and penicillin. Foot-and-mouth disease, which appeared in limited areas in this country in the past, has been eradicated and then kept out by appropriate quarantine measures. It has, however, been introduced into Mexico recently and has spread rapidly. Whether it will be introduced into the United States again cannot be predicted, but every effort is being made to exclude it. Veterinarians are of course confronted with new situations resulting from the appearance of new diseases. One example is the destructive Newcastle disease of poultry, which was first found in the United States in 1944. How it got here is not known, but extensive research is being done in an attempt to devise satisfactory control measures.

An attempt has been made to give some idea of the service of science to agriculture, and to indicate especially progress between the two world wars. The complete potential of a country may be estimated, but it is seldom used in a country like the United States except in times of extreme stress. Just as science serves agriculture, so must agriculture serve the nation. As one phase of national preparedness, we must make sure that agriculture is in sound condition. Science should therefore help to formulate agricultural policies based on facts instead of fancies, on sound judgment instead of hysteria and prejudice. Science in the service of agriculture may help provide perspective in attitudes and actions. The following, written in 1936, is pertinent:

> A few years ago we heard much about the increasing difficulty of producing enough in this country. The population was increasing rapidly because of immigration and a relatively high birth rate, industrialization and urbanization were proceeding rapidly, the standard of living was rising, and it seemed that there might soon be too many mouths to feed. The doctrine was preached that we might soon be pressing heavily on means of subsistence. Then, with kaleidoscopic suddenness, there came a change. We heard we were producing more than we could consume, that agricultural production had outstripped consumption. Agricultural scientists even were blamed for having shown how to increase the efficiency of production without having considered what was to be done with the increased amount that was produced. We ploughed cotton under; we killed pigs; we took land out of production, and many popular prophets wanted to take agricultural scientists out of production, too. Then came bad seasons: Drought, insect plagues and plant disease epidemics reminded us forcibly that farming deals with living things and that there are tremendous climatic and biotic factors before the destructive fury of which mere man is pathetically helpless. A farm is not a factory; it is a place where living things are encouraged to grow. And nature, not man, is the final arbiter as to the amount we will produce. Man has not yet completed his conquest of nature; nature can upset man's little calculations with astounding suddenness. Now we again hear little about overproduction but we hear much about the necessity of maintaining reserves. First the country is glutted with people, then it is glutted with food, and I suppose it is always glutted with talk, including mine. Have opinions merely changed or have situations changed? Both. (164th dinner, The New York Farmers.)

What changes in a decade! It will be interesting to continue the statement ten years hence. More science is urgently needed; and ethics could help, also.

To avoid diffuseness and glittering generalities, this discussion has dealt largely with the importance of agriculture in the United States. What has been said has equal relevance to international questions in agriculture. Can we develop a scientific and ethical attitude toward global problems of human subsistence? Until we do we can hardly expect to see the emergence of One World. And if we wish to attain this goal we shall have to face facts and alleviate desperate situations with scientific realism and true justice.

A CONTINUED SEARCH FOR THE BEGINNING

WM. T. SKILLING

Drawings by Frances W. Wright, of the Harvard Observatory

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KANT'S NEBULAR THEORY—1755

AN EARLY attempt to account scientifically for the sun and its family of planets was made about the middle of the eighteenth century by the great German philosopher Immanuel Kant. This was the first of such theories to seem plausible enough, and to be worked out in sufficient detail, to attract much attention.

The keen, analytical mind of Kant started with a great unorganized nebula at rest in space. An English book published a little earlier by Thomas Wright had suggested that the solar system might have resulted from a nebulous mass of matter such as the telescopes of that day revealed in some places in the sky. This thought, though it received little public attention, acted as a trigger to Kant's lively imagination. He thought out the steps by which it seemed probable to him that a nebula might change to a family of planets, with a sun at their center, and published his theory anonymously under the title *Natural History of the Universe.*

The Philosopher was not trained in physical science, but he knew the more common laws of chemistry and physics. He pictured the molecules of gas as being drawn to each other by chemical and gravitational attraction This would set up motion in the mass, and the smaller particles would be drawn into combination with the larger ones. Little clusters that formed in this way would grow larger and larger, as snowballs increase, until finally the sun and the planets would be formed.

Kant's theory was in violation of laws of dynamics that forbid the developing of itself of any circulatory motion like that of the planets from a nebula originally at rest. Even if such a system possesses some circling motion to begin with, the amount of such motion (the "moment of momentum") can never become more nor less without help from the outside.

Kant's a priori reasoning did not stop with a consideration of inert matter, for he pictured man as developing on other planets than the earth. He thought the outer planets, being formed first, would have a higher type of life than the earth. He apparently did not take into account the fact that a planet so remote from the sun as Saturn, the outermost one then known, would receive only about 1 percent as much heat as the earth does, and in consequence its temperature would make life as we know it impossible. The natural state of water on Saturn would be the solid form, and liquid water would be as much a novelty as liquid lead or iron is with us.

Kant's theory never produced much effect except in the nature of opposition to it. He was a keen-minded philosopher, but he was not a physicist.

THE NEBULAR HYPOTHESIS OF LAPLACE—1796

Nearly half a century after Kant published his discussion of a nebular origin of the sun and planets, Laplace, the greatest of French mathematicians, also starting with a nebula, proposed a theory that captured and held the field of cosmogony for a hundred years. In some respects Laplace's nebular hypothesis, as it is called, was similar to that of Kant. But he assumed that the original nebula from which the solar system developed had already some rotary motion. This would explain the fact that the planets now all revolve around the sun in the same direction and nearly in the same plane.

Although Laplace was both a mathematician and a physicist, he admitted that his hypothesis was based on neither observation nor calculation. It was originally published rather obscurely in an appendix to a popular book on astronomy. Nevertheless, there have been few advances in science that have aroused so much general interest and valuable discussion. Kant's theory had not only laid the groundwork for Laplace's theory, but the discussions of it prepared the public mind for accepting the theory of Laplace.

Laplace's nebular hypothesis did indeed help to explain most of the peculiarities of the solar system. The heat of the sun, for example, could be accounted for, as Helmholtz pointed out in 1854,

by the gradual increase of pressure as the matter within the widespread nebula contracted. Contraction against internal pressure produces heat.

The chief new points in Laplace's theory as compared with that of Kant were: (1) Laplace started with a nebula of very high temperature, slowly rotating. The loss of heat by radiation caused it to contract, and the motion of the planets around the sun could be accounted for. (2) Laplace assumed (without careful analysis of the matter) that as the mass contracted a condition would be reached at which centrifugal effect at the equator, working against contraction, would just balance and offset the inward force of gravitation. He thought that on further contraction of the inner mass a ring would be left behind and that the part within the ring would continue to shrink and leave a space between the ring and the main mass within. He assumed that in the course of time the ring would collect into a planet. After further contraction, another such point of balance would be reached, and a ring, later changing to a planet, would form; and so the process would go on until all the planets were made and the rest of the material had condensed to a sun at the center.

Two objections to this assumption of rings are now known: first, that such a ring, if gaseous, would dissipate rather than solidify into a planet, for the expansive force of the gas would be greater than the power of so small a mass to hold it by attraction; second, after a ring began to form it would be continuous instead of a succession of rings. If the rings were made of solid particles instead of gas, the result would be a thin disk, like Saturn's ring. (The gaps in Saturn's rings have been supposed to result from the disturbing attraction of Saturn's satellites.)

Another fatal error in the nebular hypothesis is found in the quantity of circular motion ("moment of momentum") of the planets as compared with the motion of the sun rotating on its axis. There is no way of explaining why the planets, which have only about one seventh of 1 percent of the total mass of the solar system, have about 98 percent of this quantity of motion, whereas the immensely greater sun has only 2 percent. (The quantity of circular motion of a body is the product of its mass, the velocity at which it moves, and its distance from the center, *mvr*.)

The rock upon which the nebular hypothesis was finally wrecked was one of the very arguments that had been considered a strong point in its support, the fact that the heat of the sun could be accounted for by compression as it contracted. But this argument failed, for it gave a method by which the entire time that the sun had been shining at its present rate could be calculated. It placed the too-short limit of 25 million years on the past lifetime of the sun, assuming that compression was its *only* source of heat—and no other source was then known.

THE PLANETESIMAL HYPOTHESIS OF CHAMBERLIN AND MOULTON—1900

In 1899 T. C. Chamberlin, head of the Department of Geology at the University of Chicago, became convinced that geological evidence gave proof that the earth itself, to say nothing of the sun, is far older than this limit of 25 million years. (It is now inferred from the character of radioactive rocks that the earth has been at its present size for 2 billion years.)

In 1900 Chamberlin and his associate, F. R. Moulton, in the Astronomy Department of the University of Chicago, undertook a thorough analysis of the nebular hypothesis to determine whether it gave a satisfactory explanation of the known facts relating to the age of the earth. It was found that there was no way in which the present system of planets could have acquired their great rotational momentum leaving in the immensely more massive sun only enough to account for the present slow rotation of the sun. They concluded that some outside influence was necessary to explain the existing revolutions of planets and rotation of the sun They pictured a passing star as furnishing the disturbing force of attraction that resulted in the present system of sun and planets—that drew them from the sun and endowed them with rapid motion in nearly concentric orbits around their still slowly rotating central sun.

As such a star approached the sun from a great distance it began to raise a tide in the gaseous material of which the sun is composed, as the moon raises a tide in the ocean. The tide would mount higher and higher as the star came nearer, and its growth would be aided by the eruptive forces in the sun that now cause prominences hundreds of thousands of miles above the sun's surface. These star tides would be far greater than our moon tides, for a star as great as the sun would have 27 million times as much mass as the moon and correspondingly more attractive force at any given distance.

As the star swept by the sun, at perhaps no greater distance than some of our planets are now, the material drawn out toward it would be attracted by the star and so would be given the cross-motion with reference to the sun that the planets have. The orbital motion of the planets would thus

be accounted for. But, falling behind the fast-moving star, the scattered material of all sizes of particles, from molecules to partly grown planets, would be drawn around the sun in obedience to the sun's gravitational force. The visiting star would go on its way, doubtless taking with it material similarly drawn out from it by the sun, which it could use for making a family of planets. Long ago it would have become indistinguishable, as seen from the earth, from any other star. The larger masses of matter going around the sun would sweep up the smaller particles and grow to be the planets, in the course of time, as they now are.

This theory was called by its authors "The Planetesimal Hypothesis," from the word "planet" and an ending that suggests the various sizes of the innumerable particles composing the material as it was when first drawn off from the sun. Chamberlin and Moulton in using the word "hypothesis" indicated that the great question regarding the origin of planets has yet to be settled beyond question.

Among others who have suggested modifications of the two-star theory, Sir James Jeans, in 1918, proposed his somewhat different version of the same story under the title "The Tidal Theory." The visiting star of Jeans' tidal theory came at an earlier time, when the sun was vastly more expanded, and he emphasized the effect of tides rather than of solar eruptions.

Then, too, H. N. Russell, of Princeton University, criticized the entire theory that material so drawn off from the sun could have condensed into planets that would have remained with the sun. Lyman Spitzer, following this thought, has shown that if material to make the planets were removed from the sun some of it would have come from a depth at which its temperature would have been around 10,000,000° F. He found by computation that such highly heated gas raised suddenly by a passing star out of the sun, where the sun's powerful gravitational force had kept it from expanding, would, upon release, expand with terrifically explosive effect. Most of the gas would have been blown entirely away from the region of the solar system, and what was left would be in the form of a tenuous gas that could hardly be expected to condense into planets.

By way of illustration, if we will merely consider an auto tire blowout, where a break in the casing gives the air room to expand, we can get some faint conception of an explosion of gas at millions of degrees, suddenly lifted out of the sun and given elbow room to expand. The break in the rubber would only reduce the pressure on the air within from about three atmospheres to one. Pressure in even the surface of the sun is caused by a gravitational force 28 times as great as the earth's downward pull. The temperature of the air in the tire causing its outward push is only about 300 centigrade degrees above absolute zero; compare the energy of gas at such a temperature with the energy of solar gases at thousands or millions of degrees. We scarcely need computation to realize the effect! As the English geophysicist Harold Jeffreys remarks, the great difficulty is "to explain the fact that the solar system exists at all," and that "the great trouble is to find even a single solution with any degree of probability whatever."

That an event happening some 2 billion years ago, as indicated by analysis of certain radioactive minerals in the geologically ancient rocks, should be well-nigh impossible to trace in detail is the only comprehensible thing about the problem. With the rapid modern increase in scientific knowledge it is not surprising that in time theories prove more or less unsatisfactory, and that new ones are put forth. The most recent of these theories, announced early in 1948, gives promise of possible success and of certain vigorous discussion. This is the "Dust Cloud Theory" and is sponsored by Fred Whipple, astronomer at the Harvard Observatory.

THE DUST CLOUD THEORY OF WHIPPLE—1948

Ever since the photographs and the arguments of the late E. E. Barnard, of the Yerkes Observatory, proved the presence of vast quantities of opaque obscuring matter in the Milky Way, dust has been a major problem with which the astronomer must wrestle. For example, earlier estimates of distances that were based on the falling off of light coming from stars of known intrinsic brightness had to be reduced when it was found that a good part of the dimness resulted, not from distance, but from the passage of the light through dust clouds. This has been notably the case where the line of sight led through the Milky Way. Indeed, the original measurements of the total width of our disk-shaped stellar system have been reduced from about 300,000 light-years to 100,000 or less.

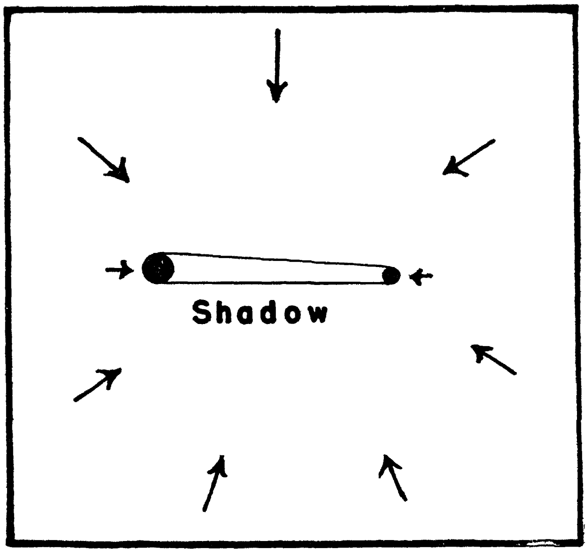
The Dutch astronomer J. H. Oort, who developed the method of measuring the rotation of our stellar galaxy, has calculated the amount of interstellar matter, both dust and gas, to be as great as the total quantity of material in the stars themselves. A part of the gas, and doubtless all the solid matter, are already put together as compounds. What could be better building material for stars and planets?

But aside from such a priori logic there are some observational evidence and calculations that make Whipple's dust cloud theory seem highly probable. Moreover, though there has been too little time since it was first proposed to have all its implications followed out to definite conclusions, this dust cloud theory seems to be freer from questionable features than earlier theories.

The important principle in this theory, on which to base an explanation of the formation of stars and planets from a dark nebula, is that light exerts a pressure against whatever it strikes. The stellar system is full of crisscrossing starlight radiated in all directions from the estimated 100 billion or so stars of all degrees of brightness, some of them more brilliant, some less so, than the sun.

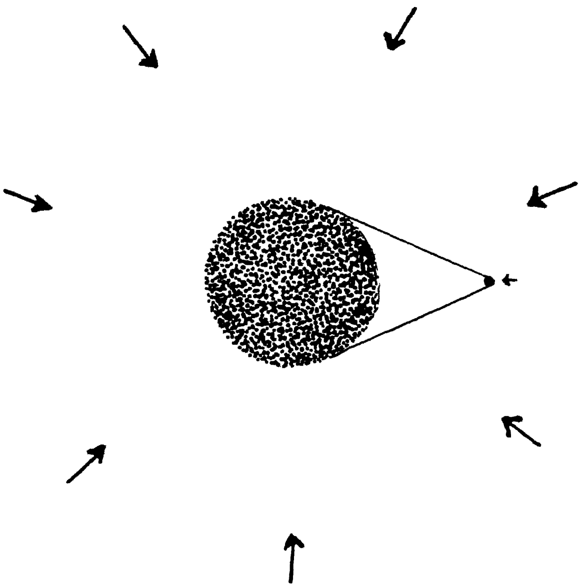
To Lyman Spitzer, of Princeton, is attributed the suggestion that light pressure could be the force that collects dust particles into cosmic clouds. This was a very important step in the theory that pressure of light may have been the initiating force by which particles of matter were driven together to form not only clouds, but stars and planets as well.

Radiation pressure of the sun upon the earth is less than 0.5 milligram per square yard. But this amounts to 2.6 pounds per square mile, and to 65,000 tons against the whole side of the earth facing the sun. This could cause no measurable effect upon the motion of the earth tending to drive it farther away from the sun.



Suppose starlight pressure to be equal from all directions acting on a particle alone in space Two particles close together would be protected on one side by each other's shadow and so driven together by unequal pressures. The mutual shadow is analogous to a partial vacuum surrounded by air pressure.

Although light pressure is so slight that its effect is unnoticeable upon large objects, it is apparently this force that acts strongly enough upon the fine particles of which a comet's tail is made so that, after the material has once been ejected from the head, the tail points away from the sun (no matter whether the comet is moving toward the sun or away from it), as if a wind from the sun were blowing the light material. Light would naturally have an effect like this on the dust of a nebula among the stars. From indications of their light-scattering effect, the particles of a cloud are estimated to average about 1/50,000 inch in diameter. But because of the greater distance of the stars from the clouds the effect of any one star would be less than that of sunshine on the relatively close comet.



The larger of two particles is less protected from light pressure than the smaller particle. Small particles in a nebula are therefore engulfed by larger ones.

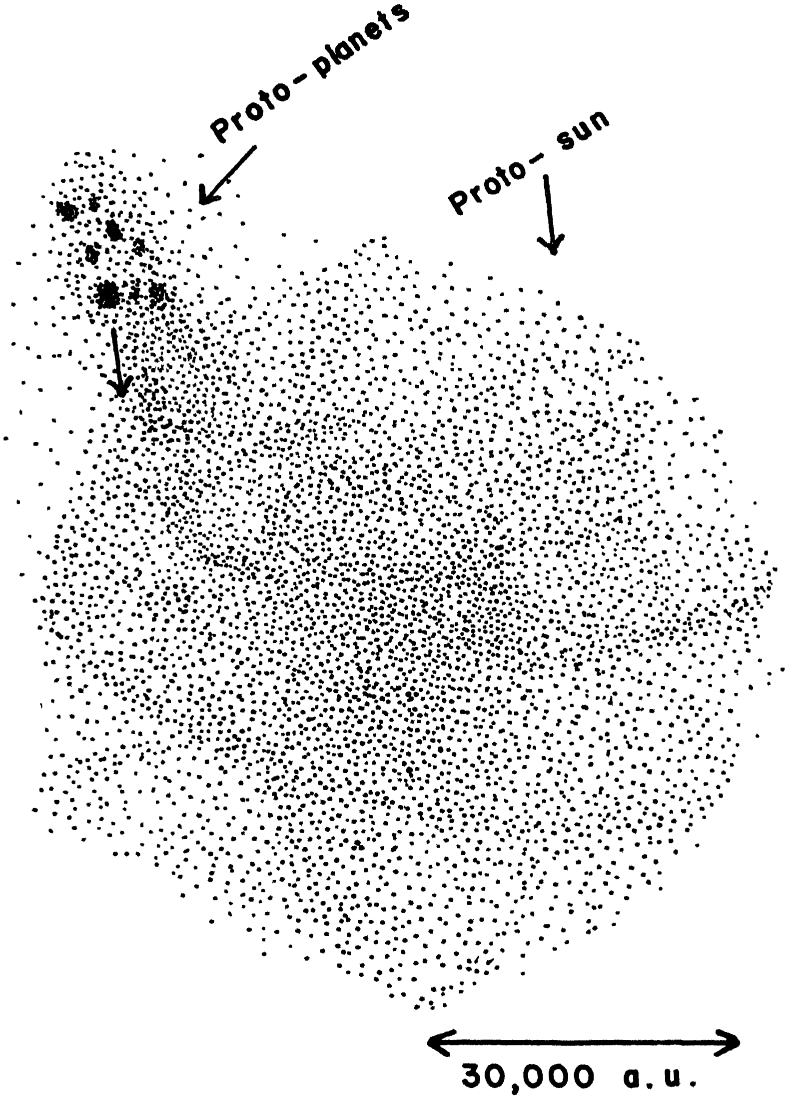
How can light pressure coming from all directions bring even two adjacent particles closer together, to say nothing of helping to build dust clouds, stars, and planets out of such particles? We must remember that, if light falls on one side of a particle from a star in any certain direction, then on the other side of the particle there will be its shadow, so far as that star is concerned. Another particle near by in line with the shadow may be held in balance in all other directions by light coming from other stars, but there will be no pressure on this particle coming from the star behind the one that casts the shadow, for that star's light does not reach the shadowed particle. Hence the particle will yield to this unequal pressure and will tend

to move into the shadow. Indeed, *any* two particles close together will each have a shadow thrown out toward the other star; they will each cut off light coming from opposite directions. The particles will therefore be *mutually* impelled toward each other.

In time the two particles should coalesce and become one. Their shadow, being larger than the shadow of a single particle, will have more power of drawing to it other surrounding particles, and so in time a larger and larger nucleus will be built up. As yet no detailed theoretical study has been made of this final phase of the nebula's collapse into sun and planets.

The apparent attractive force of a shadow is the same as the attractive force that a vacuum seems to exert. When a vacuum is created everything around rushes into it. The explanation is similar, also. Pressure being removed from one side allows the air pressure or light pressure on the other side to exert itself in that direction. The vacuum and the shadow tend to become filled Thus, light pressure from all directions tends to bring scattered particles closer together and in time form a denser and denser and larger and larger dust cloud.

The next important step in the changing of such a developing cloud into a star or a planet would be when the mass forced together by light pressure became so great and so dense that its gravi-



In a nebula having little or no rotary motion about its center, gravitational forces may yet develop individual or group motions in many directions. The largest group of particles (the *Proto-planets*), having a common and dominant motion, would grow as it swept up lesser groups of different motion.

tational attraction toward its center would become stronger than the force of light pressure around the cloud. Then the outer part would be left behind as gravitation made the inner portion shrink away from it toward the common center. Assuming that we start with a dust cloud having the mass of our sun, it can be calculated that this gravitational pull would become greater than the outer pressure of starlight when the size of the cloud had become about 1,500 times the size of our present solar system.

At this point in the process of the nebula's contraction we must begin to think of the possibility of some of the outer part condensing into a system of planets. Planets could grow from light pressure as the central sun did. So far there is no very decided concentration of matter at the center. Not much heat has been developed; the cloud as a whole has little if any rotary motion; neither is it homogeneous either in density or in what motion there is of the various parts. Here and there in the outer portions the material has condensed into many fairly dense clusters of particles. These condensations are small as compared with the great central mass, and they move about in different directions.

In the course of time these minor clusters cancel out each other's motion by collision or close passage and are gradually drawn into the central mass, the "proto-sun." But suppose one large and rapidly moving group of partially condensed bodies, the "proto-planets," escaped destruction. It would continue to circle around the central mass, all its parts going in the same direction. Gradually these parts, the future planets, would spiral inward, more or less depending upon how much interference they encountered. This interference would put them at different distances from the central sun and would give them more and more nearly circular orbits, since friction tends to change elongated orbits into circles. The planetary orbits now are nearly circular. Likewise, the orbits are nearly in the same plane, a result that can be expected from the fact that the planets are all descendants of one parent cluster of particles with a common dominant motion. Also, the inner side of each planet, the side toward the sun, would meet with more resistance, since the amount of stray material remaining would increase toward the center and hence more of this additional material would be swept up by the inner side of the planet. This would tend to give the planets a direction of rotation similar to their direction of revolution around the sun.

During the comparatively short time of a few hundred years, when the sun was reduced from about the size of the solar system to near its present size, great heat would have been developed by compression of the gases, always mingled with the solid matter of the nebula, and the gases made by the vaporization of all the solid dust particles as the temperature increased. In the last stages of collapse the sun's temperature would rise to thousands of degrees at the surface and to millions at the center. This intense heat would set off atomic changes that would have enabled the sun to maintain its temperature and rate of radiation by means of atomic energy without further help from compression. Masses of planets were too small to have reached temperatures that would release atomic energy.

During the final few months or few years of the collapsing period, while the fiery atmosphere of the sun still extended out far enough to engulf the inner planets, the satellites of Mercury and of Venus, if they had satellites, would have boiled away, together with much of or all their atmospheres, and probably much of the mass of the planets themselves. The outer planets, as far out as can be observed, are well supplied with satellites, and most of them have vast quantities of atmosphere.

The earth, probably, did not escape this "bath of fire" and was hot, perhaps molten. It is relatively devoid of hydrogen and helium and the lighter compounds of hydrogen, so abundant on Jupiter and Saturn.

An observational evidence pointing toward the probable correctness of the dust cloud theory was noticed by Bart J. Bok, astronomer at the Harvard Observatory. He called attention to the fact that there are many small, round, dark clouds among the vast, widely extended nebulae of the Milky Way. Upon measuring the angular sizes of these and estimating their density, Bok found that their masses are quite comparable with the masses of stars. These may be stars in the making. They had often been noticed before, but no significance had been attached to them. They fit well into the dust cloud theory.

An advantage that a nebular theory has over a collision theory is that it goes back a step further into the past. It attempts to account for the sun as well as for the planets. But, in any case, whether we begin our theory after the sun had somewhat the form it now has or whether we go back to the parent nebula, we are still far from the very origin. Even a nebula is a more or less highly organized entity itself. It is made of dust and gas, and these in turn are made of molecules, or at least atoms.

And even an atom is a highly organized microcosm, consisting of matter and energy.

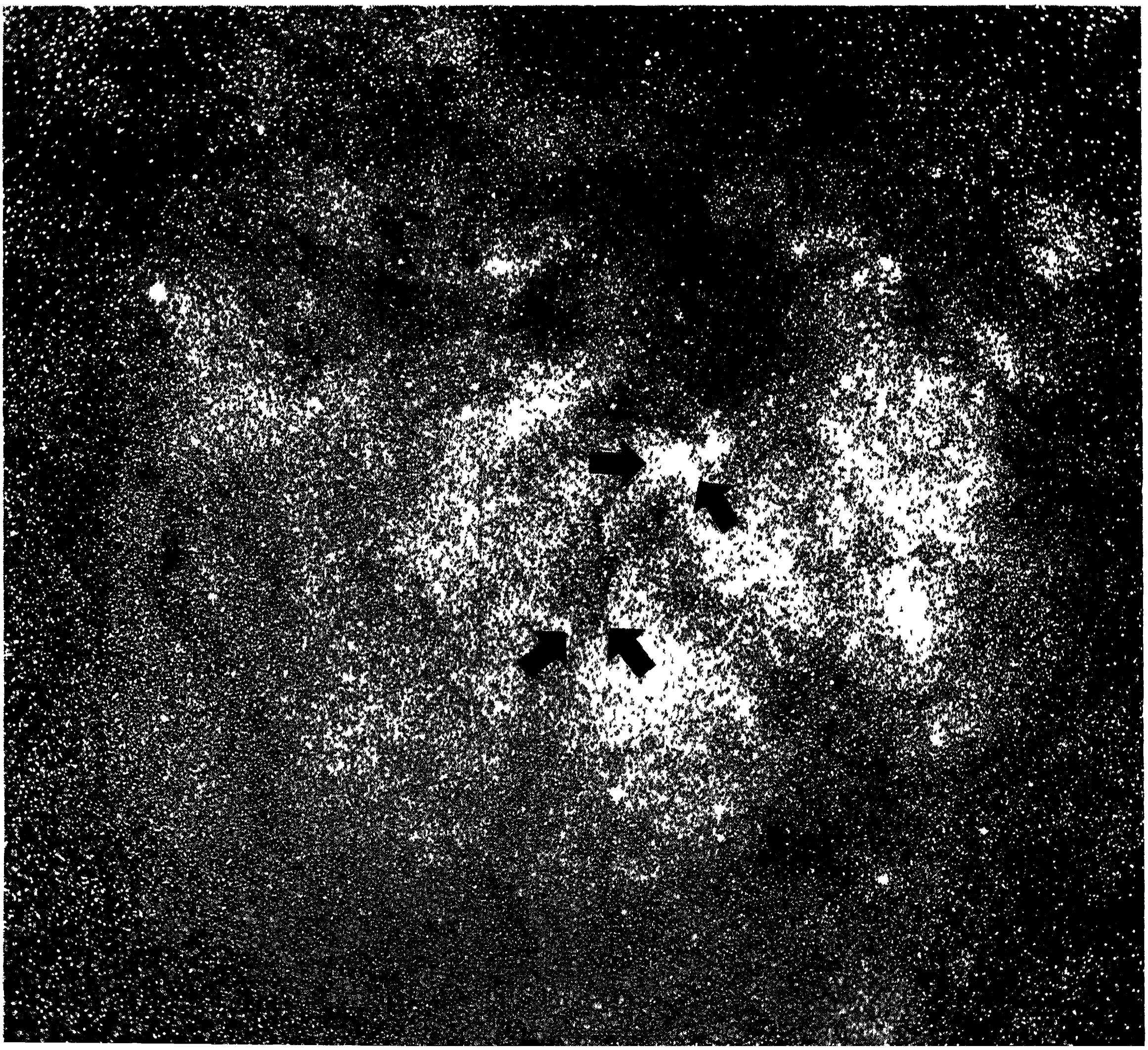
Even after an atom is stripped of all its electrons, the central nucleus, still the same chemical element that it was, possesses properties that so far have defied explanation. What is the tremendous force by which the protons and neutrons of the nucleus are held together? One important step has been taken in man's conquest of the nucleus: its parts can be separated or added to, to form other elements, as was first done in 1919 by Sir Ernest Rutherford when he changed a few atoms of nitrogen into oxygen, and later in a larger way by Ernest Lawrence, and finally on a commercial scale when at Chicago on December 2, 1942, a chain reaction was started in a graphite pile by a group of atomic-bomb experimenters.

The strange relationship between matter and energy has been traced back far enough to find that electrons, which seem to be particles of matter, behave like energy, which travels in waves. Einstein's famous equation, $E = Mc^2$, even suggests the identity of matter with energy.

However far toward origins the scientist may go, he can still see that he has not yet reached the very beginnings. If now or in some future time he may have accounted to the limit of his knowledge for the stars and planets, the atoms and electrons, he will still be in the realm of the finite. From there he can but look, with Lowell, toward the Infinite, where

> . . . behind the dim unknown,
> Standeth God within the shadow, keeping watch above his own

Star Cloud in Scutum Sobieski. Contains the cluster M11. Arrows at top show globular cluster, those at bottom, dark circular nebulae (Yerkes Observatory photograph.)



THE EXPLOITATION OF MINERAL RESOURCES*

T. S. LOVERING

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THE search for minerals and the exploitation of rich deposits have contributed, and will continue to contribute, much toward directing the course of world powers. It is my hope that if we understand the social and political influence of mineral resources now, we may avoid the disastrous consequences of mineral depletion suffered by many once-powerful nations. The efficiency and adaptability of an industrial civilization are clearly dependent on minerals as well as on men. In a sense minerals are the food of industry: iron and fuel are its chief sources of strength and energy; but aluminum, copper, lead, and zinc are the "bone builders" essential to its growth, and manganese, chromium, nickel, molybdenum, tungsten, and many minor elements are metal vitamins absolutely essential to its health. A few countries, including both the Soviet Union and the United States, have an abundance of iron and coal, but none has a satisfactory supply of all the essentials to the well-balanced diet. All suffer from a deficiency in some of the metal vitamins; the United States is quite dependent on foreign sources for manganese, chromium, nickel, tin, and several others.

Mineral deposits have many unique characteristics. Their seemingly haphazard position is fixed by some geologic accident of the remote past and not by our convenience; they are finite, nonrenewable resources, and once taken from the ground there is no second crop; during exploitation the unit cost of production rises, especially as a deposit nears exhaustion; continuing production requires discovery of new deposits—or extensions of known bodies, year after year; and, finally, most mineral products are either expended in their first use, as with coal and oil, or else last for decades—even centuries—and may be reused over and over, as are iron and gold. The nonexpendables accumulate through the years in reservoirs of potential scrap.

As some mineral deposits are found only in sedimentary rocks and others only in close association with igneous bodies, the rock formations exposed in a country suggest its probable mineral resources. Moreover, sedimentary deposits of the same age commonly contain similar deposits over wide areas, as witness the coals of the Carboniferous period in North America and Europe, and the remarkably similar sedimentary iron ores of pre-Cambrian time in the Lake Superior region, northern India, eastern Brazil, and central Labrador.

Ores related to igneous rocks show far less tendency to age correlation than do the sedimentary deposits; instead of a time relation, a geographic localization is apparent, and many well-defined mineral provinces have been recognized in which the ores affiliated with igneous rocks are characterized by a certain suite of metals and the absence of others. In Bolivia tin is a common metal in the mineralized areas associated with quartz porphyries of Tertiary age; a large number of mineral districts in the United States are also associated with quartz porphyries of that age, but tin ore is completely lacking.

The special conditions of environment and geography that cause peat, or salt, or bog iron beds to form today are restricted to small areas irregularly scattered about the globe; so, in general, have they been in the geologic past. The haphazard position of present-day volcanoes in so-called volcanic belts suggests the unsystematic and restricted occurrence of ores related to ancient igneous bodies. The spotty distribution of mineral deposits is most unfortunate for a disunited world; even such comparatively well-distributed sedimentary formations as coal have a very erratic occurrence on our planet; South America, Africa, northern and southern Europe, and much of Asia contain almost no first-rate black coal; in contrast, about half the world's reserves are concentrated in a small part of the United States. Mineral deposits genetically related to igneous rocks are, as a rule, even more localized. Some striking examples of this group are provided by nickel, molybdenum, and quartz crystal. Nearly all the world's supply of nickel

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comes from a single district in Canada; for many years a mine at Climax, Colorado, has produced most of the world's molybdenum; and Brazil supplies virtually all the quartz crystal used.

Examples of this sort could be multiplied many times. The inequality in the distribution of high-grade deposits of the different metals and minerals is tremendous and has profound economic, political, and social consequences. Civilization and culture do not remain static; they advance or retrogress. The exploitation of easily recovered mineral resources has commonly coincided with a period of national power; their depletion with national decline. Nothing should be clearer to a scientist, however, than the fallacy of the belief that "history repeats itself;" history is full of similarities and parallels, but not of repetitions.

Although history does not repeat itself, a pattern may be seen in it that is well worth thought, if we wish to use history as a guide. I shall call attention very briefly to a few of many illustrations of the influence of mineral wealth on the rise and fall of nations. Human activity has been determined by the accident of mineral occurrence far more than most historians recognize. Flint was sought for the hunter, clay for the potter, metal for the wealthy, even at history's dawn. Later, as tribes merged and nations evolved, liquidation of those quick assets of a country, its mineral wealth, yielded revenue that many times led to power. But commonly we fix our attention on the power, not on the ores in which it had its roots. The success of Alexander the Great came not alone from his genius, but from the well-equipped army with which he started his campaigns—equipment and men available because of the many thousands of talents of gold accruing to him and to his father from the Macedonian gold bonanzas. Some thirteen centuries later, the long-lived Rammelsberg silver mines, discovered about A.D. 920, were a vital factor in the birth of the Holy Roman Empire; bullion from these mines provided both Henry the Fowler and Otto the Great with much of the capital used in their successful campaigns. Mineral deposits also contributed immeasurably to the expansion and power of ancient Egypt, the success of the exploring Phoenicians, the civic and military vigor of ancient Athens, the rise and fall of Venice, and the brief hegemony of Spain.

The influence of metals since the industrial revolution has been all too evident. It is not a mere coincidence that Great Britain was the most powerful nation in the world during the nineteenth century, when she led the world in mineral production. During this time she reached the peak of domestic mining, followed by the inevitable period of declining output, during which she acquired foreign properties and increasingly depended upon imports of ores to run her industrial plant For a century and a half prior to 1850 Great Britain produced over half the world's supply of lead, and from 1820 to 1840 furnished almost half its copper. As the island kingdom approached the zenith of its power in the later half of the century, its iron production increased from one third to one half the world's total. The peak of domestic production of lead was reached in 1856, of copper in 1863, of tin in 1871, of iron ore in 1882, and of coal in 1913.

The United States has passed the peak in domestic mining for only a few metals, but we can say with confidence that the high point was reached for mercury in 1877, and for lead in 1925; it is quite probable that the peak for some other metals was passed in 1943. Acquisition of foreign sources of supply by American capital is proceeding rapidly, paralleling British moves a few generations earlier. The cost of delivery at United States industrial plants, however, will be substantially more than in the era of lush production from domestic sources of supply.

The Soviet Union has mineral resources of major importance and was late in starting large-scale exploitation. It is unlikely that the Soviet Union has yet achieved peak production in any of its important resources. Its rapid industrialization and fast-growing population provide internal markets demanding constant expansion of mineral production—or procurement. Like other industrial nations, it has an ever-growing concern for the position, extent, and availability of mineral resources at home and abroad.

The scattered world sources of all the many minerals required by industry need not be enumerated, but the adequacy of the total known supplies is a matter of deep interest to those aware of the profound social effects implicit in the exploitation of mineral deposits today.

Estimates of reserves of currently and potentially usable ore in the United States were made by the Geological Survey and the Bureau of Mines during the war and are now available in the book *Mineral Resources of the United States*. Mr. McKelvey, of the Geological Survey, recently called my attention to the fact that many of these estimates are roughly proportional to the average concentration of the various metals in the earth's crust. If we multiply the average percentage of any element in the accessible lithosphere by a billion, the result approximates the tonnage of the known reserves of

our more plentiful ores in the United States. It is interesting to note the general relation between these figures, the official estimates of reserves, and our concern for supplies. About 4 percent of the continental crust of the earth is iron; our known reserves are about 2 billion tons of iron in ore, and several times this amount in potential ore. Although some iron is imported, there is little official concern with our iron ore supply. Copper, of which we often have a small exportable surplus, makes up 0.01 percent of the earth's crust; a billion times 0.01 is 10,000,000, and our reserves in this metal are about twice this figure. Cadmium is present to the extent of only about 0.00005 percent, and this figure times a billion tons gives us 50,000 tons, just equal to the estimated reserves of this rare metal. The United States produces her needs in cadmium and at the present rate of consumption has fifteen years' supply in sight.

Turning to those metals for which our reserves are less than the amount calculated by this empirical method, we find our mineral deficiencies accented. Our reserves of manganese are about 7 percent of the calculated amount, of nickel 1 percent, of tungsten 0.4 percent, of tin 3 percent, and of platinum 2 percent; all these would be classed as strategic metals for the United States in the event of another war.

We have no adequate published estimates of world reserves of most metals. For a rough approximation, however, the figure of ten billion times the average percentage of the elements in the earth's crust suggests the world's commercial reserves, except those like coal and phosphate rock that are primarily the result of long-continued biologic processes of concentration. World reserves of such resources may be larger by one to three orders of magnitude.

As the grade of material designated "ore" is dropped, the reserves increase, but the cost of the metal won from the lower grade is also larger. This added cost is a measure of the additional energy—mechanical, chemical, mental, and physical—required to get the desired product from the lower-grade deposits. This cost means a lower standard of living than that possible with cheaper raw materials.

At the United States level of consumption for 1945, our coal, phosphate rock, iron ore, and molybdenum ore seem adequate to last for several generations, with only slightly lowered grade; if we depended solely on our known domestic resources and used current technology, our copper, aluminum, zinc, gold, and possibly our petroleum might last for twenty years; but manganese, vanadium, and lead would be used up in from one to ten years; platinum, antimony, mercury, tungsten, chromium, nickel, tin, mica, graphite, asbestos, diamonds, and quartz crystal are present in such small quantities as to be negligible.

All industrial countries should expect their domestic ores to provide gradually diminishing and more expensive raw materials and should expect to supplement them with increasing imports of foreign ores and metals. Concern over adequate dependable sources of supply will become more and more evident in national policies as domestic raw materials become scarce. Stock piles may be built up for emergencies, but they are essentially a military expedient and in no way take the place of cheap domestic ores. The sooner we can dispense with them and depend on normal economic foreign trade, the better it will be for all. Unfortunately, much of the scarce supply of "strategic minerals" now finds its chief market in armaments, where metals are withdrawn from constructive use; they will continue to make this negative contribution to society until the peoples of the world achieve the mental disarmament which alone can do away with the occasion for all war.

It is known that world reserves for mineral materials also range from some that are adequate for thousands of years to others that may be used up in one or two decades. From the long-range view the world is in short supply of zinc, lead, tin, mercury, platinum, petroleum, quartz crystal, mica, and industrial diamonds.

We in the United States would like to see the standard of living of all people raised to that which we enjoy. It is interesting to note that we make up about 6 percent of the world's population and, at a conservative estimate, we use about 40 percent of the world's output of all mineral products. Although part of this is exported, our unsatisfied internal market could easily absorb the export equivalent. If China, India, and the rest of the world were supplied on the same basis, the world output would have to be increased about 700 percent, and the reserve of the grade currently used would seem alarmingly small.

The world's reserves appear sufficient for many years under the present economic conditions, but it could happen that our grandchildren will be hard-pressed to find economic supplies of many of the industrial minerals of today. If we can foresee a time of increasing social and economic strain, ten, fifty, or even a hundred years in the future, that could be eased or eliminated by starting work on a problem today, we must act or our civilization will fail of its opportunity to raise man to his poten-

tial stature. Toynbee likens civilization to men struggling from ledge to ledge up a precipice; using this simile, we can say that the United States stands with momentarily secure footing and may with little exertion boost civilization well along toward its unseen goal.

Strategic minerals and metals are commonly defined as those for which we have no substitutes, which are essential to industry, but which cannot be produced at home in quantity sufficient to satisfy industrial demand. It is true that most of them have no satisfactory substitutes at present, but when we say a material is essential to industry and that it has no substitute, two unstated qualifications are often forgotten: a given material may be essential in producing the most desirable product, or may be essential to successful commercial competition in world markets. Belligerent insistence on completely adequate supplies of strategic materials at a "fair" price from a foreign country has proved expensive for several nations in the past. It will usually be profitable to pay more or to accept somewhat less satisfactory substitutes and forego commercial success in competition for restricted markets. The discovery of satisfactory substitutes for any material in short supply is a major contribution to both national economy and world harmony.

Where great disparity exists in the cost, efficiency, or utility of products fabricated from readily available substitutes, a dual effort should be made to acquire and conserve the strategic material for the essential uses and at the same time to find better substitutes. The wholesale use in jewelry of world-scarce platinum, one of our most useful irreplaceable research metals, seems short-sighted. The use of helium, gold, uranium, and thorium have all come under strict government control in the United States within our generation. This trend will probably continue both here and abroad; profligate use of strategic minerals should be curbed by wise regulatory restrictions.

If the industrial nations are to avoid conflict over the scattered major foreign sources of cheap raw materials, everything possible must be done to lessen the need for those strategic minerals that now seem vital to their economy and that will probably be in continuing and increasingly short supply. At the same time each industrial nation should husband its known resources and search diligently, intelligently, and hopefully for new supplies.

The United States is now the richest and most powerful nation in the world, as Great Britain was in the days of our grandfathers. I see no unavoidable calamitous decline in store for us, but I think we all realize the probability of trouble ahead if no preparation for lean decades is made during the fat and prosperous years of the present. We can well afford to spend much on research that prepares for future needs now easily foreseen. We cannot afford to do otherwise.

Geological research and metallurgical research on low-grade, currently noncommercial sources should anticipate the demands of the future. The land, the sea, and the air will all richly repay concentrated study; in them almost inexhaustible reserves of certain elements are assured. Not only should we learn to mine and concentrate low-grade materials in the most efficient way, we should also devise the most economic metallurgical techniques of extracting the contained metal, oil, or other compounds sought. The infant science of alloys—of foretelling the physical characteristics of combinations of elements—should grow into one of our most useful aids if adequately developed. It is a conservation policy of obvious importance to seek substitutes among the abundant elements for our deficient special-use metals.

New deposits continue to be found from time to time, and discovery of a single rich body of one of our essential minor metals could assure an abundant world supply for generations. Unfortunately, however, the rate of discovery of mineral deposits has been decreasing for many years. The fundamental research that should lead to discovery of the more securely hidden ore bodies has moved forward at the pace of a sleepy snail. Tremendous opportunities for progress exist in the borderland fields where physics, chemistry, and geology overlap, but hardly a handful of men are at work on the many critical problems that must be solved there before the art of ore-finding moves toward a scientific technique. Nor is the oil geologist much in advance of his admittedly backward brother, the mining geologist. A major cause of our slow progress in the past has been the difficulty of finding research scientists adequately trained in physics or chemistry *and* geology.

It is possible, however, to accomplish amazing results when groups of men specializing in different disciplines coordinate their efforts. Such group research needs subsidies and should have both short- and long-range objectives. It often yields, however, almost unbelievable results in a comparatively short time, as witness the many militarily successful developments of the war years and since. The most spectacular group research has been done in gaining understanding of the room temperature, one atmosphere microcosm, and in devising engineering applications of this knowledge. In contrast,

much laboratory work at high pressures and elevated temperatures will have to be done before we understand the origin and localization of oil and ore deposits. We know little more now than a century ago about the source of ores, the chemistry of their formation, the reasons for their concentration in ore bodies, their relation to several associated barren phases of mineralization—usually lumped together and dismissed as "alteration"—or that amazing phenomenon called replacement, a chemically unreasonable process that changes the substance but preserves faithfully the original texture, form, and volume apparently without benefit of stoichiometric relations. Obviously our success in finding additional supplies would be greatly enhanced if modern systematic research were directed toward elucidation of the genesis of metallic ores—comparable with the great work done by the Geophysical Laboratory of the Carnegie Institution on the origin of igneous rocks. Similarly we could make great strides in preparing for the future by laboratory work on the genesis of vital nonmetallic minerals. Such investigations, together with expansion of the current studies on synthetic crystals that are yielding such instructive and practical results, promise eventually to relieve some of our most severe special mineral shortages. The techniques are now available that should lead to commercial synthetic mica and commercial radio-grade quartz crystal. Ultimately, the more difficult problems of synthetic asbestos, diamonds, and other important industrial minerals should be solved.

Present progress in the purely geologic aspects of oil, ore, and other mineral deposits is much more satisfactory. Empirical knowledge of the relation of mineral deposits to geologic structure, to the chemical and physical make-up of associated rocks, and to the geological environment increases steadily. This is at present our chief hope of improving the discovery rates. I believe that the field man has already made a good start on the hard task of finding ore hidden well below the surface.

The use of sensitive chemical methods for finding soil-covered ore bodies or detecting minute additions of ore minerals in barren rock over blind ore bodies promises to be a fruitful area of geochemical research. This field owes much to the excellent pioneer work done in it by Scandinavian and Soviet scientists.

Geophysical prospecting—geologic interpretations based on the use of instruments to determine physical properties of rocks in place and their response to various fields of force—has been much more successful in helping the geologist find oil than ore. The comparatively simple structure of most oil fields contrasts with the complexly broken inhomogeneous terrane where so many of the nonsedimentary mineral deposits are hidden, and geophysics is inherently better adapted to unraveling the simple than the complex. High costs and less than fair success in getting ore have resulted in the generally conservative attitude of mining companies toward geophysical work, but I expect geophysics to play an increasingly useful—though limited—role in future exploration for ore. It will continue to contribute much to the discovery of new oil fields and should be used more and more in the appraisal of ground water, a valuable mineral that is fast being depleted in many areas.

Earnest, thoughtful research, carried on in the field by the geologist, chemist, and physicist, checked constantly against laboratory work, will suggest new methods of prospecting. There should be at least one adequately financed group devoting full time year after year to the job of devising, testing, and making available new techniques without the disheartening insecurity that accompanies uncertain appropriations, or the unsettling stimulus of possible financial jackpots for each new idea. The nation and the world community need fewer patents and less secrecy, and a little more interest in the unselfish solution of their common problems. As new methods of prospecting are devised they should be field-tested—with drill or excavation. The essence of the scientific method lies in rigorously testing new theories; progress is difficult when theories are plausible but neither disproved nor established.

In addition to scientific research and to technological progress, we desperately need wise planning for economic and equitable utilization of the world's widely scattered deposits. At the present time, however, much of the basic data required for such planning are missing. Our knowledge of the world's reserves is of the crudest sort. Inventories of our own deposits are admittedly inaccurate, but far better than those that can be obtained from most other countries for years to come. Geologic work, exploration, and ore estimates should be greatly accelerated so that an inventory of our planet's resources will be at hand soon—in time for the next generation to use in planning the make-up of those global economic units that will most effectively utilize our natural resources to the maximum benefit of all mankind. We are well warned that we must plan for tomorrow if catastrophe is not to ride us down, but, with confidence that we shall solve tomorrow's problems, I suggest that it is none too soon to give thought to the years to come after tomorrow.

THE PHANTOM WATERFALL

(THE DRY FALLS OF THE COLUMBIA RIVER, GRAND COULEE, WASHINGTON)

BARBARA WHITNEY

They say there is seen in the moonlight
And heard when the night is still
A cataract's mighty thunder
And the mist where the waters spill—
Raging and foaming in anger,
Hurtling down from the wall
They have carved in torrential flowing—
The Phantom Waterfall!

Through the towering cliffs of Grand Coulee,
Blocked by the glacial field,
The Columbia turned in her coursing,
And the torrent plunged and reeled . . .
In the script of the prehistoric
She wrote on the lava there
The romance of an old, old river,
Cascading her long white hair . . .
Mightier than Niagara,
She carved, through the myriad years,
The gigantic rocks of the canyon
Where fell her giant tears . . .
And lo! When the glacier melted
And skies were warm again
She turned back to her ancient channel
To flow on in the sun and the rain . . .
And the horseshoe falls of her making,
She left them mute and dry,
A testament to her story
For the retrospective eye . . .
For the mind of man that is probing
The long, long ages past . . .
And the heart of man discerning
Eternity's circle, at last
Closing upon its beginnings—
Chaotic and misted and gone—
Night into morning and noontide,
And evening and night into dawn . . .

And they say that sometimes in the moonlight,
Sometimes when the night is still,
You can hear the voice of the Phantom,
Where the white ghost waters spill!

ATTITUDE MEASUREMENT AND THE QUESTIONNAIRE SURVEY

ARNOLD M. ROSE

Before joining the Department of Sociology and Anthropology at Washington University Dr. Rose was associate director of the Carnegie-Myrdal study of the Negro (the results of which were published in American Dilemma*) and project director for the War Department's Research Branch. His article points out some of the weaknesses of commercial polls, so strikingly demonstrated in the 1948 Presidential election.*

MUCH of the basic raw material of social science consists of the beliefs and attitudes of men. These presumably exist in men's minds, and one task of the social scientists is to get a fair representation of them down on paper. Traditionally, this work of the social psychologist and the sociologist has been known as "attitude measurement," although recent commercial use of some of the techniques has popularized the term "public opinion poll."

To some outsiders, the public opinion poll looks deceptively easy: you just ask people questions, add up their answers, and you know what they are thinking. To other outsiders, a correct representation of men's thoughts is an impossibility: how can you ever know what people are really thinking simply by asking them a few questions, when it is so easy to dissemble and when even a psychoanalytic probing of two years' duration sometimes fails to get beyond all the conscious or unconscious defenses? Three decades of research and testing give the lie to both types of critics: it takes many technical devices and a good deal of skill to measure attitudes with validity and reliability, yet we know that it can be done. The early doubt that you could "really" get at attitudes through questionnaires was justifiably based upon a lack of concern on the part of opinion measurers for anything but a check mark or a verbal statement. As attitude measurers became more interested in the meaning of their raw data, this criticism became less valid. This must not be taken to mean that every published study of attitudes or every newspaper poll should be taken at face value. There are still numerous charlatans in the field, both with and without college degrees, who find it either profitable or prestigious to publish unsound figures. This has been especially true since reports on public opinion have become salable goods—to businessmen, to special-interest groups, or to the general public through newspapers and magazines.

MAJOR REQUIREMENTS

Let us briefly review the major requirements of a sound public opinion poll. In the first place, the questions must be capable of being answered adequately. There is little purpose in asking people questions for which they do not have an answer, or for which they cannot readily formulate an honest and complete answer. To rely on a refusal to answer is not sufficient, since some people seek to avoid the appearance of stupidity or ignorance by giving an answer when they really have none, and other people hesitate to appear dogmatic or overcertain by stating their true attitudes, and so say that they "don't know." Words serve as a vehicle for the question, and most words have at least a bit of ambiguity. It is essential that the words have a minimum of ambiguity as well as that they be simple and understandable to persons of little education. Even the person who has had long experience with formulating questions will occasionally be amazed when he poses a question, which he thinks is straightforward and unambiguous, to a variety of people and finds several variant interpretations. American culture is so far from being homogeneous that even the meanings of words shift markedly from one group to another. The skilled attitude measurer is aware of these cultural variations in so far as they apply to the more common words. Yet no reputable investigator will permit a questionnaire to go into the field without having "pretested" it on a range of individuals, representing the principal social groups, and experimenting on word selection with them.

There are other problems connected with questionnaire formation. Even when the question is completely and correctly understood, its specific wording can influence the direction of the answer, in most instances. It is only when people have thought through all the ramifications of an issue, and have reconciled their conflicting motivations toward it, that they will not be influenced by the

subtle connotations of the words constituting the question. Most attitude measurers have sought to meet this problem by choosing only "neutral" words and in other ways seeking to avoid the "biasing" of a question. Other investigators feel that no question is unbiased in an ongoing social situation and therefore seek to probe an attitude with a battery of five or six questions, some biased one way and others another way. This has led to the development of the "attitude scale" (which we shall consider later). The recognition that answers to biased questions may be significant also led to experimentation with questions that indirectly reveal attitudes of which the respondent may be completely unaware. Life itself is full of suggestive influences, and it may be as important to know whether an individual will respond to a subtle suggestion as it is to know how he will respond to a straightforward unbiased question.

The complexity of human motives has been exploited in questionnaire formation in other ways. Numerous studies have shown that the order of questions in a questionnaire will influence the response, and investigators must not only be wary of this, but must adjust for it by varying the question order in any one study. The content, or subject matter, of the question is also important. Kornhauser has demonstrated, for example, that many studies of attitudes on labor issues used a content that sought only to get at negative attitudes toward organized labor. If, instead of asking questions about strikes and monopolistic practices, the studies posed questions about security and bargaining equality, the conclusions regarding the status of unions in American public opinion would have been quite different.

After a questionnaire is formulated, a second major set of problems arises. How, and under what circumstances, shall the interview be conducted? There are innumerable empirical "rules" on how to establish a good relationship with an interviewee, varying from an admonition against interviewing anyone where a third person can hear the answers to an injunction against expressing the interviewer's own attitude either by word or subtle gesture. Some critics who have done little or no interviewing themselves question whether people will truthfully answer questions, especially when the questions become intimate and personal. Those who have experience in interviewing know that it is possible to question successfully about any subject if the interviewee is "approached in the right way." With very few exceptions, people seem to like to talk to strangers, especially if they feel their answers have some value, if they understand the purpose of the interviewer, and if they are convinced that they will be strictly anonymous. The interviewer must be sympathetic toward the interviewee so as not to inhibit him from expressing an unusual attitude, but nevertheless must maintain his social distance, since it is the impersonality of the interview situation that seems to bring out full and frank answers. Studies have been made to check the validity and completeness of answers secured by good interviewers when interviewing on very touchy and personal subjects, and it is startling to see how correct the information is.

The problems of interviewing remain for most investigators, however, because of the difficulty in securing or training good interviewers. Checks have had to be devised to curb interviewer cheating. A disturbing finding was that interviewers drawn from low-income levels secured answers different from those brought in by interviewers who had middle- or upper-income backgrounds. Conscientious investigators are now training their interviewers more carefully and are trying to select interviewers with backgrounds representative of the general population.

There is a difference of opinion as to whether interviewers should rigidly follow a fixed schedule of questions or should be free to develop their own questions after a fixed first question sets the subject for discussion. The fixed schedule reduces interviewer bias and the need for high interviewing skill. It also has the advantage of permitting easy classification and tabulation of the answers for quick reading. The flexible schedule permits probing for subsurface and complex attitudes. It allows the respondent to state his attitude in more than a phrase, and prevents misunderstanding about the meaning of questions. The resolution of this disagreement about the rigidity or flexibility of questions seems to have taken the form of a general recognition that both forms have greatest value for different problems and different subjects.

A new difficulty has arisen to complicate interviewing in recent years. When people have strong fears, they are too suspicious to be readily interviewed. This is why it has always been impossible to conduct a public opinion poll under a dictatorship. People will not talk freely to an interviewer who might be a threat to them. In the United States, until recently, this was significant only when interviewing Negroes, who were so afraid of expressing their true opinions to a white person that they either "played dumb" or told the white interviewer what they thought he would like to hear. Reliable investigators always met this situation by employing Negro interviewers and clear-

ing their survey with Negro defense organizations or leaders. Since 1946 the same situation has arisen with a significant number of white respondents: they are suspicious of strange interviewers as possible Communists. Interviewers for such nationally known polling agencies as Gallup and Roper can easily identify themselves, but the problem remains for independent social scientists. The only partial solution that has been devised has been to clear studies with churches and civic organizations.

A third important factor in public opinion research is the selection of respondents. Only the Census Bureau has the resources to interview every family in the United States, and then only once in ten years. For all other studies, a sample of respondents must be selected, and the sample must be representative of the population about which the investigator wishes to generalize. The rules for drawing a representative sample have been well worked out by statisticians, but they are difficult to apply when human beings constitute the sample. When physical objects, or even plants or animals, are the subject of investigation, it is a relatively easy task to pick cases at random, making sure that each unit has the same chance of being selected as any other unit. When the people of the United States are the subjects of investigation, however, it is impossible to line them up for purposes of picking a random sample or to obtain equal access to all of them. There is not even a list of their names and addresses from which a sample list could be selected and then traced.

Two types of methods have been contrived to meet the problem of selecting a representative sample of people. One is the "quota" method, by which a sample is devised to match the distribution of the population in certain known traits, such as age, sex, region of country, rural-urban residence, etc. (This information is available from the decennial census, corrected for postcensal changes and for demonstrated errors.) Then interviewers are sent out to interview people who have the specified traits: they fill up the quota of each age-sex-regional, etc. type of person as fast as they happen to come across persons of that type. This method of sampling is widely employed by commercial pollsters and was the one chosen by the one biologist who has elected to do a major social science study—Kinsey, in his study of sex behavior. The method has one grave weakness, however, in that the people *within* each of the quotas may not be representative. This is of no great importance if the biases of nonrepresentativeness within the quotas are not related to the subject matter of the interview. But if aggressive people happen to be chosen more frequently than passive people, it does make a good deal of difference, for example, if one is questioning about attitudes toward war. If working women are not available to a poll-taker because they are working during the daytime, it will make a great deal of difference in an election poll, since working women vote differently than housewives. The careful social scientists have therefore devised another method of sampling, called the "area" method. This involves dividing the population into small geographical areas, selecting some of these areas as representative on the basis of known traits, and using carefully controlled random sampling of people within each of the selected areas. This method is more difficult and more expensive to employ, especially in rural areas where the dispersion of population makes random sampling hard to accomplish. But it seems generally to be more foolproof than the quota method.

The problems of the attitude measurer are not done when he has devised his questionnaire, selected his sample, and secured his interviews. Then he has to boil down his mass of information into a comprehensible and readable form. This involves the steps known as "coding" and "analysis." Coding is essentially a process of classifying the diversity of answers, and the words in which the anwers are couched, into a reasonable number of categories. Actual deviations must not be lost, but meaningless diversity must be submerged. The planning of a code requires skill and knowledge; the application of a code takes a great deal of tedious work. The principal step in analysis is deciding how the coded answers are to be tabulated. Should answers to a given question simply be added up, or can answers to one question make sense only when put in context of answers to another question? Are the facts for the population as a whole what is important, or is significance to be secured only by comparison of answers from different elements in the population? Do answers to a single question have any validity, or is it necessary to combine into a scale the answers to a series of questions all trying to get at the same attitude? These are some of the questions the analyst must answer as he tackles each study. He cannot wait to solve these problems after the interviews are complete, but must have his solution fairly well worked out before he regards his questionnaire as complete and before he collects a single interview.

The interpretation of findings and the presentation of results offer a final set of challenges to the attitude measurer. No uniform standards have as yet been devised to handle this stage of the operations. Nor probably can any ever be, since each

subject of study is somewhat different from every other. A few general principles have been set forth, and a variety of devices for presentation have been examined, but these are too detailed to go into here.

USES OF THE QUESTIONNAIRE SURVEY

What sort of information is the product of all these operations? Although the questionnaire survey grew up in an effort to measure attitudes, it now has a far broader use than that. In addition to detecting attitudes, it can reveal expectations, wishes, activities, facts, estimations, and so on. Let us translate these vague terms into examples of concrete uses to which the questionnaire survey has been put.

One is the simple determination of facts, such as the amount of liquid savings in the hands of the general public, which the U. S Treasury regularly asks the Survey Research Center at the University of Michigan to determine. Sometimes facts that are easily detected separately have no significance until put in context by a questionnaire survey. Certain Army officers in the Mediterranean Theater were disturbed at the sloppy appearance of American soldiers from the Replacement Depot at Pozzuoli until a survey determined that not only were there no QM laundry facilities available at the Depot, but that there was no soap available at the PX and that the military police did not allow soldiers to take clothing out of this camp for fear they would sell it on the Italian black market. The Kinsey study of the distribution of different types of sex expression is another example of fact determination, where the main interest lies in comparison of different groups in the population.

The preference type of study is most frequently employed by large industrial concerns. A great deal of money is spent every year to determine what various consumers find most pleasing to the eye, to the nose, to the palate. Government has also used the preference survey when it has a choice and wishes the decision to be made in accord with the preferences of the citizenry. In planning its postwar education program, the Army used a sample interview survey as the equivalent of getting a filled-out commercial order. The social scientist seldom has any direct use for a preference survey, except when preferences can be used as indices of attitudes. Many of the early studies of attitudes toward minority groups, for example, involved the respondents' making statements of preference for one group as compared to another, in different social situations. The psychologist L. L. Thurstone developed a complicated technique for transforming such preference statements into a scale of attitudes reflecting the status of minority groups, vocations, and other rankable social traits.

The knowledge type of questionnaire survey is very much like a school examination, except that it is given to the usual representative sample of the population and its purpose is not to rank. Rather, its purpose is to determine whether a publicity campaign of one sort or another is successful in reaching the public or whether those who have greater knowledge about something also have attitudes more favorable to it. Government policy-makers—as well as manufacturers—are interested in such information. Social scientists have found use for many studies of the opinion or personality correlates of knowledge. In general, they have found that those with more knowledge about a given subject have fewer emotional prejudices against that subject.

Other types of cross-classification are employed in an effort to get clues to the causation of attitudes and behavior. In one study of venereal disease, not only was there cross-classification of incidence of this disease with knowledge of preventive techniques but also with facts about drunkenness when having extramarital intercourse and with beliefs about the ability of newly discovered drugs to cure the disease. The findings confirmed the hypothesis that little knowledge, drunkenness, and overconfidence in modern drugs were strongly associated with the incidence of venereal disease. Correlations by themselves are insufficient for imputing cause, and so experiments were carried out to test the tentative findings. Information was given about techniques of preventing venereal infection and about the uncertainties of available drugs, with the object of determining whether this resulted in a reduction of the venereal rate.

Most experimental studies in the social sciences require attitude measurement at every phase. The usual procedure has been to administer a questionnaire to matched experimental and control groups. Then the social stimulus is directed at the experimental group, sometimes quickly, but more often over a period of time and without obvious connection to the questionnaires. After this, the same or equivalent questionnaires are administered to the same groups, and changes in the attitudes of the experimental group are compared to changes in the attitudes of the control group, if any. The questionnaire may be repeated after an interval of several months to see whether the effect of the stimulus has worn off. Sometimes several stimuli are directed at different experimental groups at the same time, and a comparison made of their relative influence. Such, for example, was a study of

the relative effectiveness of emotional and logical propaganda. For the particular groups and subject matter chosen, it was found that the emotional appeal had greater success than the rational appeal, but that the latter was more effective than no appeal at all. Some experimental studies are "evaluational" when the stimulus investigated is a program that someone desires to evaluate.

Another type of attitude survey might be called a "definitional" study. Definitions are usually set by a tradition or by operational convenience, but sometimes it is valuable to conduct a little study before formulating a definition. One such study might be to determine whether people generally make the same distinctions between terms or set the same limits to terms that the scientist does. A certain study translated the numerous current definitions of the term "morale" into a questionnaire in order to determine the extent to which high morale in one sense was related to high morale in another sense. Such diversity was discovered that it was necessary to formulate several distinct definitions of morale and to note that they were not the same thing. Some investigators find important social science data in popular definitions, and they conduct surveys where the questions allow the respondent to formulate his own definitions.

A final type of study that must be mentioned has prediction as its primary purpose. One prediction study may simply be an extrapolation of attitude trends. Another may be based on influences from causes discerned from the correlational or experimental types of studies. A workable schedule on such a basis has been prepared by Burgess and Cottrell to predict the future marital adjustment of an engaged couple. A third may be a questionnaire survey in which respondents are asked to state or estimate their future plans. Some valuable clues to the future of the consumers' market have been discovered by asking people their plans for buying radios, automobiles, or household equipment. The election pollsters have this last type of purpose—to predict simply by asking people what they intend to do. But the method is risky if not carried out in conjunction with questions about motivation and intensity of attitude, and if careful analysis is not made of the "undecided" voter.

Sometimes a prediction study can be used as a a selective technique to prevent the predicted events from occurring. Such was a study carried on in the Army during the second world war to predict which newly inducted soldiers would develop psychoneuroses. After it was determined which traits, experiences, and reactions predisposed a man to psychoneurosis, those inductees who had a high score on the predisposition test were sent to a psychiatrist, who either rejected them for Army service or marked them down for further observation.

There is an increasing range of uses for the questionnaire survey. Each time there is a new use or a new criticism, there is development or refinement of techniques. In 1944 Hadley Cantril brought together existing knowledge about techniques in this field in his book *Gauging Public Opinion*. Developments have been so rapid, however, that already many sections of the book are out of date. Even the periodical literature is somewhat behind, as one can observe when he attends the annual meetings of the World Congress for Public Opinion Research, the American Association for Public Opinion Research, the American Statistical Association, the American Sociological Society, or the American Psychological Association. The presentation and discussion of new techniques are carried on under the most favorable conditions of open-mindedness and high interest. Those practitioners with a scientific orientation know their own limitations. The field appears to have a most interesting future.

EARLY HISTORY OF INFRARED SPECTRORADIOMETRY

W. W. COBLENTZ

Dr. Coblentz (Ph D, Cornell, 1903) has been a physicist with the National Bureau of Standards since 1905. He is a five-time medallist: the Potts medal of the Franklin Institute; the Janssen medal of the Paris Academy of Science; the Scott medal and premium of Philadelphia; the Rumford gold medal of the American Academy of Arts and Sciences; and the Ives medal of the American Optical Society.

MODERN equipment for measuring the spectral absorptive, emissive, and reflective properties of substances consists of three parts: an assortment of powerful sources of thermal radiation; an assortment of sensitive radiometers for measuring the intensity of the total or the spectrally dispersed radiation from these sources, including equipment for magnifying the response of the radiometer and automatically recording it upon a sheet of paper; and a spectrometer, consisting of suitable lenses or concave reflecting mirrors for collimating these rays, and a prism or grating for dispersing the radiation into a spectrum.

Thus, a relatively inexperienced technician, after receiving a little instruction on how to operate this apparatus, is now able to trace upon a sheet of paper the spectral transmission curve of a substance in fewer minutes than it required hours with the spectroradiometric instruments available some four to five decades ago. The recent jump to a higher level of attainment in the detection and measurement of thermal radiant energy is so sudden and spectacular that, to the newcomer, it gives the impression that history is just beginning—that he has "discovered America."

In this connection the dependence of advancement in one field of investigation upon the progress in another branch should not be overlooked.

Thus, in 1916, at the Spring Meeting of the American Physical Society, in the discussion that followed the announcement of improvements in electronic amplifier tubes, a fellow-member fairly leaped to his feet to point out the application of the device in connection with a thermocouple for measuring the heat of the stars. But it required another quarter of a century of effort to produce the electronic amplifying and recording apparatus that has recently come into general use in connection with thermal radiation meters.

It is interesting and instructive to trace the bypaths leading from the original foundations upon which present-day spectroradiometry is based. Some of these paths are long in years. All are based upon the application of discoveries of properties of materials: chemical, electrical, optical, thermal, each one of which has an interesting history that is encyclopedic and is briefly summarized in early handbooks of physics.[1] More complete references are to be found in Poggendorff's[2] *Who's Who in Science,* which goes back to the very beginning, some 500 years B.C.

In retracing these bypaths to modern radiometry it is important to keep in mind that experimental science was then in its beginning; that these early researchers were untrained in laboratory methods as we know them today; also that, although then as now their interest was international, intercommunication was slow and mainly through publication. Hence the overlapping, repetition, and sometimes misdirection of effort in the early researches in radiant energy are understandable, especially in view of the obscurity of the subject and the lack of facilities for making the requisite apparatus.

One of these by-paths is the investigation of the electrical properties of matter and the foundation of the science of electricity, which began with the discoveries of the Italian physicists Luigi Galvani and Alessandro Volta at the end of the eighteenth century.

This was followed, in 1823, by Seebeck's[3] discovery of the production of a thermoelectric current on heating the junction of two metals; and his use of a thermopile of many junctions of bismuth and antimony for measuring temperatures, in place of a mercury thermometer, in establishing a temperature scale.

Modern methods of measuring thermal radiant energy, using a thermocouple or thermopile receiver to intercept the radiation, had their origin in Seebeck's experiments with immersion thermoelectric thermometers.

The first big advance in the investigation of the infrared transmissive properties of substances began half a century after the discovery of galvanism (in 1780) and about six years after the discovery of thermoelectricity, when Leopoldo Nobili,[4] professor of physics in the archducal museum at Florence (the old habitat of the Accadèmia del Cimento) published an illustrated description (dated

Reggio, Dec. 2, 1829) of a compensated thermopile consisting of 6 elements (couples) of bismuth and antimony, mounted in a circle, in a cylindrical wooden housing, with the 6 exposed junctions extending about 2 or 3 mm above the cement support. The junctions were soldered with tin and blackened with candle smoke.

To demonstrate cooling, he placed this arrangement under a bell jar and removed the air with a vacuum pump. We can appreciate his comments on this as a detector of radiant heat: *"Je dirai presque incroyable."*

In the second part of this same paper (dated Reggio, Apr. 24, 1830) Nobili[4] tells of providing Macedonio Melloni, professor of physics at the University of Rome,* with one of his thermopiles having 6 elements, which, however, did not seem sufficiently sensitive radiometrically. He therefore constructed a new thermopile consisting of a bundle of 16 couples mounted in a cylindrical metallic enclosure which was attached to a pedestal that could be turned in any direction. In this arrangement, which later became known as the "Melloni thermopile," the ends of the junctions were exposed to radiation, and a fixed conical reflector was used to increase the intensity of the radiation incident on the receiver.

Encouraged by the great sensitivity attained with the thermopile of 16 couples, Nobili constructed a thermopile consisting of a bundle of 40 couples, which was perfectly symmetrical in that both ends could be exposed to radiation. Two conical reflectors were provided that could be opened and closed at will, thus permitting a simultaneous comparison of the radiation of two objects mounted at opposite ends of his optical bench.

In view of the great sensitivity of these new thermoscopes Nobili[4] (*l.c.*, p. 234) announced that *"M. Melloni et moi"* were undertaking a series of researches which would be published soon.[5]

Seeing that these two researchers, at times, were separated only relatively short distances, even in those early days of inconvenient travel, one can envision the enthusiasm of Nobili and the 14 years younger Melloni in making these investigations.

In this connection it is to be noted that Nobili[6] invented also the astatic combination of magnetic needles, whereby he greatly improved the sensitivity of his galvanometer.

* According to Poggendorff,[2] Melloni was professor of physics at the University of Parma from 1824 to 1831, when, during the political revolution in Modena, Parma, and the Papal States, he escaped to Paris, where he continued his researches until 1839 when he was called to the University of Naples.

The objectionable feature of having the unexposed junctions directly back of the exposed ones (the receiver) was recognized by Nobili,[7] who already in September 1834 described a new linear thermopile, consisting of a single line of 4 "extremely small" elements of Bi-Sb, mounted sinusoidally and crosswise, the central line of receivers (details not described) slightly overlapping but not touching; total height 15 mm; covered with a movable slit, opened 0 55 mm, for studying diffraction.

But Nobili was almost eighty years ahead of his time. For suitable thermopile material all he lacked, to be modern, was pliable bismuth wire,† which became an article of commerce about 1910, and pliable antimony-tin wire, which came some years later. It is therefore understandable why Nobili's new type of linear thermopile did not come into general use until almost eighty years later.

Nobili's[5] investigations in collaboration with Melloni were published in 1831, after the latter went to Paris. His untimely death in August 1835, at the age of fifty-one, occurred at an interesting period in infrared radiometry when Melloni,[8] using a thermopile of 27 couples, of bismuth and antimony (forming a square receiving surface of 4.24 cm^2), discovered the high transparency of rock salt to infrared radiation. Thereupon he made the first prism of this material and, using a linear thermopile of 15 couples of bismuth-antimony, mounted on a crude graduated circle (but no lenses or mirrors), explored the infrared solar spectrum (*l.c.*, p. 366 and Plate 3). In another arrangement Melloni used a prism and lens of rock salt, in a stationary position, and moved the thermopile straight across the spectrum.

Nobili did not describe the difficulties in making (casting) the bars of bismuth and antimony. Melloni[8] speaks of the oxidation of liquid antimony, and the great fragility of both metals, which were in the form of bars 32 mm long, 2.5 mm broad, and 1 mm thick. The great heat capacity of the junctions retarded thermal equilibrium; this was accelerated somewhat by thermal conduction to the "cold" junctions that were directly back of the exposed junctions.

In a subsequent paper Melloni[9] gives an illustration of the assembled thermopile, galvanometer, and other accessories, which is copied in earlier handbooks of physics[1] and is familiarly known as the "Melloni apparatus."

† "In itself a remarkable achievement," as Professor E. F. Nichols expressed it, when he wrote me for the loan of a sample and inquired where such wire was obtainable.

If we are bewildered by the sudden vista of new problems presented by the recent accomplishment of atomic fission, we can envision the reactions of investigators at the end of the eighteenth century: of the British William Herschel on observing that his mercury thermometer was heated when held just outside of the visible red end of the spectrum of sunlight; of the American Benjamin Thompson, later known as Count Rumford, while engaged in boring cannon, trying to reconcile his observations of the heat developed with the then-prevailing idea that heat is a material substance; and of another American, John William Draper,[10] who unknowingly made the first observations on the radiation of what is now called a black body when he heated various metals and nonmetals in a gunbarrel and found that all became luminous at the same temperature.

The foregoing citations are only a few of the steps leading to the development of modern spectroradiometry.

SOURCES OF RADIATION

The earliest investigators had only sunlight for use as a powerful source of visible and what we now recognize as near infrared radiation.

Included in the terrestrial sources, the Leslie cube of hot water, the Bunsen burner (invented in 1835), and a blackened sheet of copper (also an incandescent spiral of platinum), heated in the flame of an alcohol lamp, were used; also the Drummond lamp (consisting of an oxy-hydrogen flame impinging upon a solid rod of chalk or zirconium oxide); and, much later, the carbon arc lamp. After 1880 the carbon filament incandescent lamp was available.

A convenient fairly reliable source of infrared radiation, in the form of the Welsbach gas mantle, composed of thorium oxide (for strength) and a small amount of cerium oxide as an emitter of light, did not come into use until 1890; and a powerful source of ultraviolet radiation, in the form of a high-pressure "hot quartz" mercury arc lamp, for laboratory use, did not become available before about 1905.

Nevertheless, it is remarkable what was accomplished in studying the optical properties of matter, using these sources of radiation. Indeed, as will be noted presently, by using different sources of luminous and nonluminous radiation Melloni found that the transmission of radiation through rock salt did not depend upon the source, indicating, sort of intuitively, a marked difference in spectral energy emission of the source; and, as noted above, he promptly assembled a combination consisting of a slit, a lens and prism of rock salt, and a thermopile which he moved linearly through the spectrum. But, because of the low sensitivity of his thermopile, he continued his investigations of the transparency of substances to infrared radiation by using the undispersed radiation of four sources, consisting of a Locatelli (wick, oil) lamp, without a glass chimney; a spiral of platinum heated to high incandescence in an alcohol flame; a thin, blackened copper plate, similarly heated to 390° C; and a Leslie cube of blackened copper heated with boiling water.

RADIOMETERS

The early measurements of radiant heat were made by noting the expansion of air in a blackened glass bulb or a pair of glass bulbs, connected with a capillary tube containing a short column of mercury, known as the Leslie (also Rumford) differential thermometer.

The first great advance in the measurement of thermal radiation came in 1829 when Leopoldo Nobili[4] constructed the first bismuth-antimony thermopile, which he combined with his improved galvanometer with astatic needles, discovered in 1825.[6]

After further improvements in the astatic galvanometer and in the thermopile, by Nobili[7] in 1834 and by Melloni[9] in 1835, this device continued, for years, to be the most serviceable radiometer for lecture room demonstrations of the properties of radiant energy, and for researches in thermal radiation.

In 1857 Svanberg[11] observed that a blackened spiral of copper wire, 0.21 mm in thickness, forming one arm of a Wheatstone bridge, when exposed to the radiation from his hand, or the walls of the room, produced a deflection of the galvanometer needle. Svanberg considered the possibility of using his galvanic differential thermometer in place of the Melloni thermopile (the name by which it was popularly known). But, like the thermopile, the area of the receiver was too large, and it was too insensitive for measuring narrow bands of spectral radiation.

In that respect the Langley[12] bolometer, developed almost half a century after the thermopile, and consisting of a thin, narrow strip of platinum, which forms one arm of a modified Wheatstone bridge, remains to this day one of the most sensitive, quick-acting radiometers ever devised for precise spectral radiometry.

Other types of thermal radiant energy meters of high sensitivity, which came into use between 1887 and 1897, were improvements, by Pringsheim[13] and

by Nichols,[14] in the Crookes[15] repulsion radiometer; also the Rubens[16] thermopile of copper-constantan (also of iron-constantan), and the Boys[17] radiomicrometer, all of which instruments were studied and intercompared by Coblentz.[18]

The high sensitivity of the present-day (thermocouple) radiometer is attained mainly by connecting it with recently developed electronic or optical devices that amplify the response of the receiver.

For the early beginnings in producing a sensitive radiometer we have to thank Leopoldo Nobili, the modest professor of physics in the archducal museum at Florence who, in 1825, while still a captain of artillery, in presenting a description of his new astatic galvanometer[6] before the academy of sciences of Modena, concluded by saying (*l.c.*, p. 125): "I do not attach great importance to this idea; but merely to indicate a means whereby 'avec le temps,' meteorology may be enriched with a new instrument." Incidentally, in the same volume in which Nobili[4] describes his new thermopile, in two papers dated a month earlier (Nov. 1, 1829), this gifted experimenter[19] gives a theoretical and experimental analysis of the electrophysiological effects upon the nerve of a frog; and he proposes to treat the two maladies paralysis and tetanus by means of electricity; the former by inducing artificial tetanus by the action of an interrupted current, and the latter by the action of a continuous current to benumb the nerve. This appears to be the forerunner of one branch of present-day (electro) physical therapy.

SPECTROMETERS

As already noted, the forerunner of spectroradiometry was Melloni's[8] systematic measurement of the diathermancy of a large number of substances to 4 sources of radiation—2 luminous and 2 nonluminous. This disclosed the great transparency of rock salt to the infrared radiation emitted by all these sources (Tr = 92%), thereby indicating the way to spectroradiometry with lenses and prisms of rock salt.

Although these early (1834) spectral radiation measurements of Melloni[8] with a lens and prism of rock salt showed the possibilities in using dispersed radiation, the low intrinsic sensitivity and the great width (2.5 mm) of the thermopile receiver militated against the use of a spectroscope. Hence, even as late as 1866 a species of filter radiometer was used to isolate wide bands of spectral radiation.

As employed by Tyndall,[20] this consisted in the measurement of spectral bands of radiation isolated by a filter; for example, a solution of iodine in carbon bisulphide, which has a wide band of high transmission extending from about 1μ to 3μ in the infrared.

Considered historically, progress in the production and measurement of spectral radiation has been by leaps and bounds. It begins with Isaac Newton's spectral dispersion of sunlight by means of a crude glass prism, way back in 1666. Then there is a leap of 134 years to the time when, on placing a sensitive mercury-in-glass thermometer in different parts of the solar spectrum, William Herschel[21] observed a rise in temperature of 2° in the violet, increasing to 7° in the red and to 9° in the infrared. By means of his concave speculum metal mirrors he showed that the infrared rays (as we now call them) can be reflected, like the visible rays; and even with these crude methods he studied the spectral transparency of numerous materials.

Groups of scientists, like a flock of sheep, are inclined to browse in the same field. Groping along blindly, they made observations several years before and after Herschel's measurements that did not agree with his as to the location of the maximum heating; some finding the maximum in the visible, others finding the maximum in the invisible, part of the spectrum. This was clarified by Seebeck,[22] who explored the solar spectrum with the blackened glass bulb of a Rumford differential air thermometer and found that the position of the maximum heating in the solar spectrum depended upon the composition of the prism (crown or flint glass), and that maximum effect is obtained when the prism is kept at minimum deviation.

In the meantime, investigators were busy with the determination of the refractive properties of liquids, glasses, quartz, etc. It was then realized that part of the disagreement in the location of the maximum heating in the solar spectrum, as observed with prisms of different kinds of glass, was because the dispersion is not uniform with wave length. Mathematical formulas were set up correlating refractive indices and wave lengths. Using sunlight reflected from a speculum metal heliostat (but no lenses) and passed through prisms of crown glass and of rock salt, Müller[23] examined the infrared solar spectrum with a thermopile, and found that it extended to 1.77μ or 4.8μ, depending upon the formula used in making the calculations. He was the first to convert his energy measurements from a prismatic to a normal spectrum (using a graphical method, later amplified by Langley); he found the maximum emission in the visible, which must be considered accidental, though true as later established by Langley's refined measurements.

Furthermore, as is now evident from the Lang-

ley bolographs, the disagreements in the measurements made by these early experimenters (some finding that the solar spectrum terminated at about 1.1μ, others observing solar radiation out to 1.7μ) occurred because of the almost complete absorption of solar radiation by atmospheric water vapor in the region of 1.1μ, 1.4μ, 1.9μ and beyond 2.6μ, depending upon the solar height (the season, and air mass traversed). This would be accentuated in the high latitudes where the observations were made.

Looking back at all this groping, and what appears (in many instances) misdirection of effort, it is to be noted that as early as 1817 Fraunhofer[24] mounted his prism on a theodolite having achromatic glass lenses, thus forming what is now called a spectrometer. Yet, as just noted, even as late as 1858 Muller[23] muddled along through the solar spectrum without using any lenses.

As already mentioned, the only powerful source of radiation then available was the sun. Presumably these early investigators realized that part of the uncertainty in their measurements of the distribution of energy in the infrared solar spectrum was the lack of achromatism of their rock salt lenses.

For a heliostat mirror these early investigators used speculum metal. Concave mirrors of speculum metal would have been too heavy, and glass mirrors backed with tinfoil-mercury amalgam would have been impracticable. After 1835, when Justus von Liebig succeeded in producing brilliant deposits of silver on the surface of glass (by heating formaldehyde with an ammoniacal solution of silver nitrate), it was possible to make mirrors with silvered surfaces. Incidentally, this was the result of Liebig's search for a substitute for the then common household mirror made of tin-mercury amalgam, the manufacture of which was poisonous to the processor.

The preparation of glass mirrors backed with a film of silver is a relatively simple process, but the deposition of a clean, tightly adhering, highly reflecting layer of silver on the surface of a plate of glass is a tedious process that did not attain high perfection until after 1880.

I have never seen an explanation of the great delay in the use of a spectrometer made of silvered concave glass or speculum metal mirrors. Presumably it was because of the great weight of the metal and the expense of making a suitable spectrometer axis; also, the expense and difficulty of producing (and conserving) silvered surfaces, which were not covered when not in use and hence deteriorated rapidly. Of course, evaporated metal surfaces were unheard-of in those days.

In 1872 Draper[25] used a Silbermann speculum metal mirror in his heliostat, and a concave glass mirror (silvered on the front surface) rigidly mounted on a table to reflect different parts of the solar spectrum upon the thermopile. This was accomplished by manipulating a series of shutters, and proved to be a poor arrangement.

A few years later a spectrometer, consisting of a prism and lenses of rock salt, was used by Jacques[26] in measuring the distribution of energy in the spectrum of various sources of radiation. He had much difficulty in maintaining good optical surfaces.

These experiences mark the early beginnings of the second epoch in the measurement of spectral radiation, when there was an increasing awareness of the necessity of making improvements in spectroradiometry.

In view of the fact that gratings were engraved on concave mirrors of speculum metal, it is understandable why similar mirrors of speculum metal were used, and why silver-on-glass mirrors came into use so slowly, especially in a smoke-laden city.

Between the years 1880 and 1886, while still at the Allegheny Observatory, Pittsburgh, Langley measured the spectral and total radiation from the moon; also the radiation from the sun and artificial sources. For this purpose he used his newly invented bolometer[12] as a radiometer in combination with silvered mirrors and spectrometers with a Roland grating;[27] also with lenses and prisms of rock salt, or of a glass that was especially diathermanous out to 2.5μ. After coming to Washington (in 1888), in addition to his duties as secretary of the Smithsonian Institution, Langley built an Astrophysical Observatory, introducing innovations in spectroradiometry that were far ahead of his time.[28]

Among other improvements these included: (1) a large Foucault siderostat mirror of glass, silvered on the front surface; (2) a large concave silver-on-glass mirror on a fixed pedestal to project and focus the solar spectrum upon the bolometer; (3) a large rock salt prism (19 cm high) with a large, flat, silvered mirror, mounted on the axis of rotation of his spectrometer (the mirror keeping the beam at minimum deviation as the solar spectrum traveled across the bolometer); and (4) a photographic registration of the galvanometer deflection resulting from the heat absorbed by the bolometer in different parts of the solar spectrum.

Since the incident solar beam is practically parallel, only one mirror is needed as used by Langley and his successors. With his rock salt prisms, figured by his friend John Brashear, Langley was

able, for the first time, to obtain a fine resolution of the D lines in the solar spectrum.

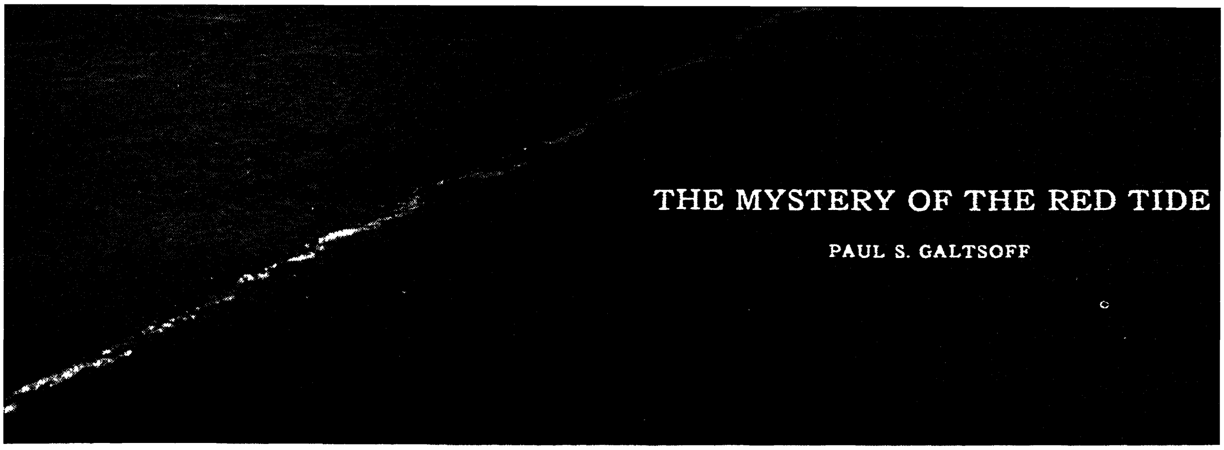
The first use of mirrors instead of lenses in a laboratory spectrometer was made in 1883 by Pringsheim,[29] who combined two fixed silvered mirrors of plano-concave glass with a Rutherford grating mounted on a spectrometer axis which was rotated to focus the spectrum on a Crookes radiometer.[13] With this apparatus he explored the infrared solar spectrum to 1.5μ. Although continued cloudy weather prevented him from making an extensive series of measurements, enough was accomplished to point the way to building mirror spectrometers, by skilled technicians, for general laboratory use.

Thus, soon after 1890 the second epoch of infrared spectroradiometry began. It marks the first real beginning of a quantitative investigation of the spectral infrared absorptive, emissive, reflective, and refractive properties of materials, which has continued for over half a century. That the discovery of the spectral absorptive properties of materials would have a practical application could not then be foreseen. Or, if foreseen, this method of spectrochemical analysis would have been too costly. Furthermore, the real need did not arise until three or four decades later.

About 1940 we entered the third epoch of spectroradiometry. The practical methods of application of the information on instruments and methods of spectroradiometry and on infrared absorption spectra of substances, accumulated during all the intervening years, have developed by leaps and bounds in recent years, and have attained a high state of perfection in a remarkably short time. The regrettable thing is that this sudden advance could not come in peaceful surroundings, and be used wholly for peaceful purposes.

REFERENCES

1 Jamin, M *Cours de Physique.* (4th ed., M. Bouty) Paris: 1891; Wullner, L. C. *Handbuch Experimental. Physik.* (5th ed.) Leipzig: 1895, **2,** 192; Preston, T. *Theory of Heat.* London: 1894; Kayser, H. *Handbuch der Spectroscopie.* Leipzig: 1900, **1;** Rubens, H. *Rapports, Congrès Internat. de Physique.* Paris: 1900, **2,** 141.
2. Poggendorff, J. C. *Biographisch—Literarisches Handworterbuch zur Geschichte der exacten Wissenschaften.* Leipzig: 1863, **1, 2;** 1898, **3.**
3. Seebeck, T J. *Pogg. Ann. der Phys.* 1823, **6,** 1; *Denkschr. Berlin Akad. Wiss.* 1822–23.
4 Nobili, L. Description of a *Thermo-Multiplicateur* or Electric Thermoscope. Bibliothèque Universelle. *Sci. & Arts,* Geneve, 1830, **44,** 225.
5. Nobili, L., and Melloni, M. Recherches sur Plusiers Phénomènes calorifiques Entreprises au Moyen du Thermomultiplicateur. *Ann. Chim. et Phys.,* 1831, **48,** (2), 187
6. Nobili, L. Description of a New Galvanometer with Two Needles. Bibliothèque Universelle. *Sci. & Arts,* Geneve, 1825, **29,** 119.
7. Nobili, L Description of Two New Thermoelectric Piles and Their Use in Calorific Researches. Bibliothèque Universelle. *Sci. & Arts,* Geneve, 1834, **57,** 1.
8. Melloni, M. Memoir on the Transparency of Different Solids and Liquids to Infrared Radiation. *Ann. Chim. et Phys.,* 1853, **53,** (2), 5; 1834, **55,** 337.
9. Melloni, M. *Pogg. Ann. der Phys.,* 1835, **35,** 112.
10. Draper, J. W. On the Production of Light by Heat. *Phil. Mag.* 1847, **30,** (3), 345.
11. Svanberg, A. F. Galvanic Differential Thermometer, *Konigl. Svensk. Vetensk. Akad. Handl.,* 1849, 109; *Pogg. Ann. der Phys.,* 1851, **84,** 411.
12. Langley, S. P. Actinic Balance. *Amer. J. Arts & Sci.,* 1881, **21,** (3), 187.
13. Pringsheim, E. Crookes Radiometer. *Wied. Ann. der Phys.,* 1883, **18,** 1.
14. Nichols, E. F. New Type Crookes Radiometer. *Wied. Ann. der Phys.,* 1897, **60,** 401.
15. Crookes, W Repulsion Radiometer. *Proc. Roy. Soc. London,* 1874, **22,** 32.
16. Rubens, H. Thermopiles. *Z. für Instrumentenkunde,* 1898, **6,** 65.
17. Boys, V. Radiomicrometer. *Proc. Roy. Soc. London,* 1887, **42,** 189.
18. Coblentz, W. W. Instruments and Methods of Radiometry. *Bull. Bur. Stand.,* 1908, **4,** 391; 1912, **9,** 7; 1914, **11,** 131; 1918, **14,** 507; 1921, **17,** 187.
19. Nobili, L Experimental and Theoretical Analysis of the Electrophysiological Effects. . . . Bibliothèque Universelle. *Sci. & Arts,* Geneve, 1830, **44,** 48, 165.
20. Tyndall, J. On Calorescence. *Philos. Trans. Roy. Soc. London,* 1866, **156,** 1; *Contributions to Molecular Physics in the Domain of Radiant Heat.* London Longmans, Green, 1872.
21. Herschel, F. W. Experiments on the Refrangibility of the Invisible Rays of the Sun. *Philos. Trans. Roy. Soc. London,* 1800, **2,** 255, 284, 293; **3,** 437.
22. Seebeck, T. J. Ueber die ungleiche Erregung der Wärme im prismatischen Sonnenbilde. *Abhandl. der Berliner Akad. d. Wiss.,* 1818–19, 305; 1822–23.
23. Müller, J. Wellenlänge und Brechungsexponent der äussersten dunklen Wärmestrahlen des Sonnenspectrums. *Pogg. Ann. der Phys.,* 1858, **105,** (4), 337, 543.
24. Fraunhofer, J. Determination of Refractive Indices of Different Glasses. *Gilbert's Ann. der Phys.,* 1817, **56,** 264.
25. Draper, J. W. On the Distribution of Heat in the Spectrum. *Amer. J. Sci.,* 1872, **4,** (3), 161; *Philos. Mag.,* 1872, **44,** 104.
26. Jacques, W. W. Distribution of Heat in the Spectrum of Various Sources of Radiation. *Proc. Amer. Acad. Arts & Sci.,* 1879, **14,** 142.
27. Langley, S. P. The Experimental Determination of Wave Lengths in the Invisible Prismatic Spectrum. *Memoir, Nat. Acad. Sci.,* 1883, **2,** 149.
28. Langley, S. P. *Ann. Astrophys. Obs.* Washington: 1902, **1.**
29. Pringsheim, E. Infrared Solar Spectrum. *Wied. Ann. der Phys.,* 1883, **18,** (3), 32.



THE MYSTERY OF THE RED TIDE

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THE blue color of the open spaces of the ocean, like the color of the clear sky, is a physical phenomenon caused by the scattering of light against particles smaller than the shortest wave length of the visible spectrum. This explanation, consistent with present scientific evidence, implies that light may be scattered either by the molecules of water itself or by highly dispersed minute particles suspended in it. The pure and highly transparent water of the open sea contains no special coloring substance that could give it its remarkable hue.

As one approaches the shore the change in color and in the transparency of water becomes noticeable. Dark-blue shades give place to greenish or yellowish coloration and the water becomes more turbid. The presence of a water-soluble "yellow substance" derived from numerous small algae (phytoplankton), which thrive in coastal areas, causes the change in color, and pigment-bearing microorganisms, which abound in plankton, greatly increase its turbidity. The muddy appearance of large stretches of the sea near the mouths of big rivers is due to mineral particles brought in with land drainage. Water containing a large amount of suspended material, which changes its normal blue color, is called "discolored."

Pronounced discoloration of water caused by rapid multiplication or by swarming of microscopic organisms is often referred to as "blooming." Although it may frequently be observed in freshwater ponds and in the reservoirs supplying our cities with drinking water, blooming rarely attracts the attention of the layman. It does, however, provoke his indignation, and bitter complaints against the "oily" flavor of drinking water (caused by the presence of diatoms, flagellates, and green and blue-green algae), or against the unpleasant taste resulting from chemical treatments used to destroy these undesirable although harmless organisms. The incidents of the blooming of the sea are more rare. They usually excite public curiosity and concern, especially when the water along the beaches or in the vicinity of coastal towns becomes red and the discoloration is accompanied by mortality of fish, shellfish, birds, and other denizens of the sea.

The exact conditions that provoke blooming are not known. Under normal situations there exists in the sea a balance between the populations of different species of animals and plants comprising its plankton. The equilibrium is rather unstable, for the ratio between the various organisms is not fixed. It changes almost daily and may easily be upset by a rapid growth of a population of a single species, almost to the exclusion of all others. If the rapidly multiplying organisms contain green, red, or other pigment, the color of the water changes accordingly.

Above: Line of dead fish, photographed from the air at about 500 feet altitude, marks the path of the red tide off the Florida coast. (*St. Petersburg Evening Independent* staff photo.) *Left:* Dead fish floating in water, as seen from low-flying plane. (*St. Petersburg Times* photo.)

* Article published by permission of the Director, U. S. Fish and Wildlife Service.

An excellent illustration of blooming is the repeated rapid propagation of a diatom, *Aulacodiscus kittoni,* along the shores of Copalis Beach, in the state of Washington. Outbursts of rapid reproduction of this diatom may occur several times during one year. They can be easily observed from the shore. Persons who have studied the phenomenon report that under certain combinations of meteorological conditions the growth of the diatom is so rapid that within a few hours brown patches of an almost pure culture of it appear on the surface of the sea. They are carried by the waves and deposited on the beach in layers several inches thick. Blooming of *Aulacodiscus* usually lasts for only a few days and has no harmful effect on fish or shellfish. After it has ceased, normal equilibrium between the various species of plankton is re-established until the next outburst, which apparently occurs after light rain. The exact conditions that favor rapid growth of the *Aulacodiscus* population are, however, not known. It is interesting to note that in this case the blooming is restricted to a rather narrow surf zone several hundred feet wide, and does not extend farther offshore.

Rhizosolenia, a common pelagic diatom, has been frequently reported as the cause of the blooming in various parts of the ocean. Along the western coast of Japan the swarming of this alga in August–September, forms extensive "oily patches" on the surface of the water (*Gaiya*), and, according to Ussachev, from September to December the surface of the water of the Sea of Azov is covered with such a thick film of this alga that the sea has "an appearance of a placid swamp with typical odor and dark brown color."

Swarming of a colonial flagellate of the genus *Phaeocystis* is often responsible for brownish discoloration of water in moderate latitudes. Several species of this slime-covered alga, whose colonies are visible to the naked eye, have different temperature tolerances. The range for *Phaeocystis poucheti* is between 1.0° and 11.6° C, whereas *Ph globosa* prefers warmer water—between 6.3° and 16.7° C. Swarming of *Phaeocystis* is of great practical significance to fishermen in the North Sea, for the alga is extremely offensive to herring and they are never found in the areas where *Phaeocystis* is abundant (Savage).

Dark- and bluish-green water is usually caused by the blue-green algae (Myxophyceae). The cells of these plants contain two pigments: the blue-green phycocyanin and the red phycoerythrin. The proportions of the two pigments vary in different species, some of them having only one pigment. The preponderance in plankton of the species rich in phycoerythrin may cause red discoloration. Thus the names of the Red Sea and of the Vermillion Sea (Gulf of California) reflect the fact that the blue-green alga *Trichodesmium erythraeum* abounds in their waters. Other species of *Trichodesmium* are often found in very large numbers in the open sea around Japan, in the Brazilian current, in the waters of the West Indies, and in the Gulf of Mexico. It has been reported that *Trichodesmium* film may extend in the open ocean over tens, or even hundreds, of square miles and

Masses of a diatom, *Aulacodiscus kittoni*, deposited by surf on tidal flats of Copalis Beach, Washington.

that its presence, which can be detected from the deck of a ship by a noticeable odor of chlorine, calms down the waves (Delsman). The growth of blue-green algae in the brackish waters of the Azov and Baltic Seas is so prolific during the summer that the surface of the sea "has an appearance of a green meadow" (Knipowitch).

Red discoloration of sea water may be caused by a great variety of organisms. Swamps along the seashores of Denmark are often full of red water because of the prolific growth of purple sulphur bacteria (*Chromatium, Thiopedia,* and *Thiocystis*), and the red water of the Sicilian "Lake of Blood" is caused by the luxurious growth of *Thiopolycoccus ruber, Thiopedia rosea,* and other species (Forti). *Rhodothece pendens, Rhabdomonas rosea,* and other sulphur bacteria color the sea near Helgoland "as though with rose-red milk of sulphur" (Ellis). Several species of sulphur bacteria are commonly found in the lagoons along the northeastern coast of the Black Sea and in marine bottoms off the northern coast of European Russia (Issatchenko).

Red color caused by sulphur bacteria is apparently harmless and has no effect on marine populations. A serious situation results, however, from the outburst of propagation of various species of dinoflagellates, which frequently cause red or brownish discoloration of large areas of the sea. In many instances the red color of the water caused by these microorganisms occurs simultaneously with mortality of fishes and oysters.

One of the earliest published records of red water, or red tide, is that of Charles Darwin, who, during the voyage of H. M. S. *Beagle* in 1832 off the coast of Chile, took a sample of water which had a "pale reddish tint" and, examined under a microscope, "was seen to swarm with minute animalculae darting about and often exploding."

Along the coast of Peru the frequent occurrences of red water are invariably accompanied by extensive fish mortality and liberation of H_2S. Locally, the phenomenon is known as *aquaji*. Gunther, who studied the plankton and the discoloration of water in this area, states that the outburst of red water is provoked by a sudden rise in temperature and convergence of the abnormal countercurrent known as El Niño. The exact cause of the Peruvian red water has not yet been ascertained, but it is probably due to dinoflagellates, for Gunther maintains that the orange-colored water contained large numbers of flagellates with red pigment.

Similar occurrences of red water, frequently associated with fish mortality, have been reported from the Malabar Coast of India, from Australia, various places in Japan, South Africa, the west coast of the United States, British Columbia, the Gulf of Mexico (west coast of Florida) and Narragansett Bay. Unfortunately, nowhere has systematic study of the red-water phenomenon ever been made; the observers usually record the time and location, occasionally give temperature and salinity data, and sometimes state the identity of the microorganisms responsible for discoloration.

Japan has paid more attention to the study of the red tide (*akashiwo* in Japanese) than any other country, probably because of the extensive damage caused by red water to valuable pearl-oyster grounds. According to Aikawa, between 1899 and 1934 there were twenty-four recorded occurrences of discoloration of water in Japan, sixteen of which were accompanied by mortalities of fish and shellfish. Of these instances, three were caused by *Noctiluca,* nine by other dinoflagellates, five by diatoms, and one by blue-green algae. The cause of the others was not determined. Ago Bay (34°17′ N. and 136°47′ E.) was the place of the most frequent outbursts of red tide, which appeared there seven times, causing some damage to pearl oysters and inflicting particularly severe losses in the winter of 1933–34. Next in frequency was Tokyo Bay, where discoloration was recorded three times. The records summarized by Aikawa show that in Japan red tide may take place at any season of the year.*

Along the coast of California, near La Jolla, red discoloration of water is of common occurrence. It was reported for the years 1906, 1907, 1917, 1924, 1933, and 1935. The zone of discoloration sometimes covers a large area extending for about three miles along the coast. Red water is often observed along the seacoast of the state of Washington and of British Columbia. Undoubtedly there are many other instances of red tide that remain unrecorded.

Red discoloration is also caused by swarms of *Noctiluca,* a naked dinoflagellate widely distributed in the Atlantic and Pacific Oceans; by dense populations of a marine ciliate, *Mesodinium rubrum*; by local concentrations of medusae (*Aurelia* and various siphonophores); by copepods (*Calanus finmarchicus*); and various species of crustacea of the group of Euphausiidae, which constitute the principal food of whales. There is no evidence that any of these forms are responsible for mass mortality of fish.

Several species of armored and unarmored dino-

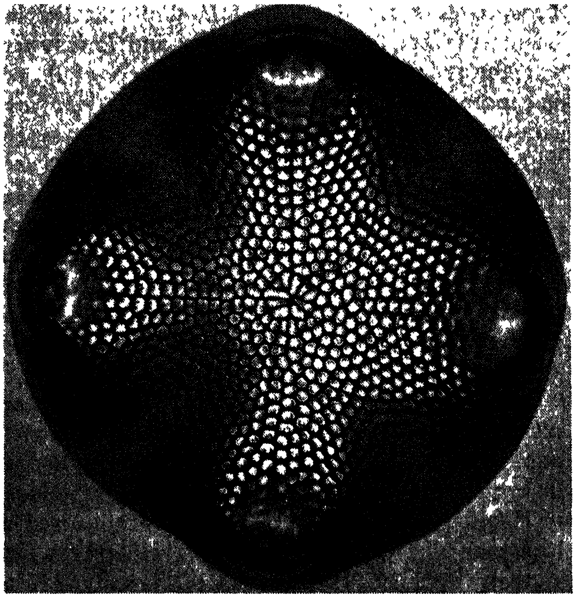
* I gratefully acknowledge the courtesy of Dr. Charles J. Fish, who made available to me abstracts of Japanese publications translated in his office.

flagellates listed as the cause of red water require particular attention. *Gymnodinium splendens, G. sanguineum, G. mikimoti, Cochlodinium catenatum, Polykrikos schwartzii, Pouchetia rosea, Gonyaulax polygramma,* and *G. catenella* have been frequently observed in Japan; *Peridinium triquetra* in S. Africa; *Prorocentrum micans* in California; *Gymnodinium splendens* and *Gonyaulax alaskensis* in Puget Sound; *Glenodinium rubrum* in Australia; *Amphidinium fusiforme* in Delaware Bay; and *Gymnodinium brevis* along the west coast of Florida. In the absence of a comprehensive study, one may consider that the list is not complete and that some of the identifications made in the field are only tentative. Some of the above-listed dinoflagellates are apparently nontoxic. Thus, there is no evidence on which to suspect *Amphidinium fusiforme, Glenodinium rubrum, Polykrikos,* and *Pouchetia.* The evidence against the others is mostly circumstantial, the accusations being based on the fact that the species was predominant in the red water at the time of mass mortality of fish or shellfish. The proof that several species of *Gymnodinium* and *Gonyaulax* are toxic is provided, however, by both field observations and laboratory experiments.

The rate of propagation of any unicellular organism that multiplies by division is very rapid. The division of each mother cell gives rise to two daughter cells, which in turn repeat the process at regular intervals. Since there is no death of the parent cells, the unicellular organism reproducing in this manner may be considered potentially immortal. The growth of the population of such a form increases with geometrical progression and may be expressed in a general formula, $2^{tp} = N_t$, where t is the number of days since the beginning of propagation, p is equal to the number of generations per day, and N_t is the total number of individuals comprising a population on a given day after the beginning of propagation. The formula is employed by Wernadsky to express the universal property of living matter, namely, its ability for unlimited expansion. The formula does not take into consideration the mortality caused by accidental death or predation by other organisms. Under actual conditions in the sea, these two factors are of great importance, for they provide the mechanism that maintains an equilibrium between the various members of the planktonic community.

With favorable physical conditions and abundant supply of nutrient material, the growth of a population of marine protozoa may be extremely rapid, approaching its theoretically possible rate. The space available for the expansion of a growing population in the sea is almost unlimited, and the reserve of nutrient material may last for several weeks, or even months. Eventually, changes in the temperature and in other factors of the environment, and the exhaustion of the food supply, slow down the process and the blooming gradually ceases.

An increase in the total number of autotrophic organisms in the sea water cannot occur without



Aulacodiscus kittoni.

an adequate supply of nutrients. Plankton samples collected during the outbreak of red water off the Florida coast showed that red discoloration was invariably accompanied by the predominance in plankton of *Gymnodinium brevis* (new species, described by Davis). Several species of diatoms, copepods, marine Cladocera (Evadne), and lamellibranch larvae were also present, but in the patches of red or brownish water the *Gymnodinium* was far more abundant than all the rest of the planktonic organisms put together. Woodcock estimated during one of the field trips that samples of deep-red water contained as many as 56 million *Gymnodinium* per liter. The water was slimy with mucus, apparently derived from broken *Gymnodinium* bodies.

Since *Gymnodinium* is autotrophic, it would be logical to conclude that an additional quantity of nutrient salts, and particularly of phosphates, became available to sustain its rapidly growing population. Partial confirmation of such an assumption is found in the chemical analyses of samples of red water made by Ketchum, of the Woods Hole

Oceanographic Institution. He found that surface samples collected in a red-water area contained from five to ten times as much total phosphorus as had ever been encountered in the oceanic waters uncontaminated by land drainage or domestic pollution.

Two explanations suggest themselves. High content of total P may be due to the swarming of *Gymnodinium* in the upper stratum, whereas the propagation of this organism may have taken place in the entire column of water. In this case the phosphorus extracted from the deeper layers was simply transported to the upper level. On the other hand, if the propagation of the *Gymnodinium* was restricted to the surface layer, it means that there was an additional supply of phosphates in the sea. Present observations do not answer the question but point to the necessity of a comprehensive study of vertical and horizontal distribution of total phosphorus and other nutrients in the area affected by red water.

The possible source of the additional supply of phosphorus is of great interest. The southern half of Florida abounds in phosphate rocks that are extensively mined. Large quantities of phosphate, used as fertilizer, are loaded from a platform on one of the islands near the Fort Myers area and shipped by water. Whether there exists a way by which the phosphate deposits are washed away or in other manner reach and fertilize coastal waters can be determined only by further studies. At present, further speculation along these lines does not seem to be justifiable.

The occurrence of discolored water along the west coast of Florida in 1946–47 can serve as an illustration of a typical outbreak of poisonous red water. Unfortunately, at the first signs of mortality, in the latter part of November 1946, no competent biologist was engaged in any field studies in this section of the coast, and the beginning and spread of mortality can be reconstructed only from oral or written reports of fishermen and owners of shore properties. About November 20, mackerel fishermen noticed dead or dying fishes in the patches of red or brownish water fourteen miles offshore from Naples. The "Red Tide," as it was immediately called, spread northward, reaching in December and January the inshore waters near Sanibel and Captiva Islands. Fish continued to die, and millions of dead carcasses were floating in the water or were cast ashore. All the beaches in the Fort Myers area became littered with dead bodies of fish, which accumulated at the rate of more than 100 pounds per linear foot of shore line. In February 1947, carcasses of fish were washed ashore on Englewood Beach, marking the northernmost extent of mortality; Cape Romano appeared to be the southernmost limit. The strip of discolored water was confined to the coastal zone, extending from five to eight miles offshore. Beyond this area the sea water was clear and of normal dark-blue hue.

According to the observation conducted by the Marine Laboratory of the University of Miami, all kinds of animals perished in the red water, including a small number of turtles and porpoises (Gunter, Williams, Davis, and Smith). A great many common commercial and noncommercial fishes were recognized in windrows piled on beaches or among the carcasses floating in the water. Likewise, the pelagic and bottom invertebrates succumbed to the unknown poison. Large numbers of dead shrimp were noticed, as well as common blue crabs, fiddler and mud crabs, barnacles, oysters, and coquinas. Observations made by me in March 1947, around the Fort Myers area, disclosed that about 80 percent of the edible oyster *Ostrea virginica*, grown on piles, were dead. Clean inner surfaces of their shells, free from any fouling organisms, indicated that death had occurred only recently. No mortality was observed among the hard-shell clams, *Venus mercenaria*, and no reports were received of the destruction of ducks, gulls, and other birds inhabiting the inshore waters, although later on some of the residents of Largo, Florida, told me of thousands of sea gulls and pelicans "which died from eating the fish poisoned by the red tide." Unfortunately, there was no opportunity to verify these statements.

Red water gradually disappeared, and normal conditions were restored in March 1947, but a few scattered outbreaks of red discoloration and fish mortality were reported in April from Key West, Marathon, and Cape Sable.

Early in July 1947, the red tide reappeared in the same place. On or about July 6 the streaks of red water, three to six feet wide and about one hundred feet long, were observed along the beaches near Venice, Florida, and in areas to the south. Within a week the area of discolored water extended from the tide line to approximately fifteen miles offshore. With the surface current it slowly spread northward and by July 30 reached Anna Maria Key, South of Tampa Bay. By August 5 the red danger extended about fifteen miles north of Clearwater. Fish killed during the second outbreak probably exceeded the number destroyed during the first one. An accurate estimate is impossible, but considering the numbers washed up on beaches and the carcasses carried out by the

Mass of dead fish deposited at edge of water at high tide. (Photo by John Evans, *St. Petersburg Times*)

tide, it is highly probable that from one to two hundred million pounds of fish were destroyed. Tightly packed masses of dead fish in bands one hundred to two hundred feet wide and extending over a great distance along the coast were observed several miles offshore. W. Anderson, of the U. S. Fish and Wildlife Service, who made field observations at the time of summer mortality, states that when flying or sailing over the affected areas "one was seldom out of sight of dead fish."

Spectacular and disastrous as it was, the outbreak of the red tide in Florida was not without precedent. Since 1844, the year of the earliest record, the discoloration of sea water, accompanied by an extensive mortality of fish along the east coast of the Gulf of Mexico, has been reported several times. One of the more recent outbreaks, in 1916, in the Fort Myers area, fully described by Taylor, will be discussed later.

In the mind of the layman, mass mortality of fish appears to be convincing evidence of the poisoning of water by some unknown and powerful chemical dumped into the sea, or by release of poisonous gases from the bottom as a result of an earthquake or volcanic disturbance. The fact that red water contains myriads of living and rapidly multiplying microorganisms contradicts this popular belief. It is reasonable to expect that water containing strong chemicals deliberately dumped into it, or poisoned by volcanic gases, would be much more toxic to delicate unicellular organisms than to fish, shellfish, and crustacea, which in general have greater resistance. Deprived of plankton the sea water would appear crystal clear and highly transparent. We know, however, that red water is very turbid and overloaded with plankton.

One might suspect that death of fish or shellfish in red water is due to suffocation, caused by the occlusion of their gills. This explanation, often advanced, is not confirmed by my observations during the recent outbreak of red tide in Florida. The gills of dead mullets and thread herrings collected in red water near Fort Myers, Florida, were not covered by any foreign material, and the internal organs appeared to be normal.

Frequently, references are made to the odor of H_2S emanating from red water. These statements lead to the belief that the death of fish may be due to the lack of oxygen. It is true that decomposition of great masses of fish and plankton may exhaust the supply of oxygen—but field observation shows that this takes place only in limited areas, close to the shore. No deficiency in oxygen content of water sufficient to be detrimental to fish was found in the affected area in Florida.

Judging by the description given by fishermen and residents along the sea beaches, death of fish entering red water comes rather suddenly. As the fish (mullet, for example) enters the patch of red water it begins to act rather strangely, coming to

the surface, whirling around, then turning on its side or floating stomach up, and finally sinking to the bottom. This suggests that some unknown substance in red water specifically affects the nervous centers controlling equilibrium and respiration.

There is convincing evidence that this substance is derived from plankton. Experiments performed by the Miami Laboratory showed that sheepshead minnows (*Cyprinodon variegatus*) died in 22–46 hours after being placed in a tank filled with water containing live *Gymnodinium brevis,* whereas there was no mortality among the fishes placed in a similar tank filled with water from Biscayne Bay, in which *Gymnodinium* was absent.

A series of experiments performed by me at the Woods Hole Laboratory proved that a toxic substance can be extracted from red plankton by acidified alcohol. The residue, redissolved in sea water, was lethal to killifish (*Fundulus heteroclytus*) in a concentration of 1: 250, in which the fish died within 1 hour 25 minutes. *Fundulus* placed in this solution soon loses equilibrium; it turns on one side and unsuccessfully attempts to right its body. Irregularity of respiratory movements and floating with belly up appear after a more prolonged exposure and are followed by death if the fish is not removed from the poisonous solution.

Less pronounced effects, shown only in a loss of equilibrium, are apparent even in the dilution 1: 1,000. Greater dilutions (1: 2,000) of red extract and extracts prepared for comparison from plankton consisting mostly of diatoms collected in Woods Hole Harbor were harmless.

The effect of the red-water extract was also tried on small toadfish (*Opsanus tau*) and on young horseshoe crabs (*Limulus polyphemus*). In all these cases the test animals perished in a short time, the time of their survival depending on the concentrations used.

Red-water extract was found also to have a pronounced effect on the development of sea urchin (*Arbacia*) eggs. By using a portion of the same sample of red water that I used to prepare the extract, Corman has demonstrated that in 1: 5 dilution of red water the rate of cleavage of sea urchin eggs was retarded by 27 percent and that the eggs were cytolized in 1: 2 dilution. Comparing his results with my data, Corman thinks that there may be some parallel between the toxicity of red-water extract to *Fundulus* and the retardation of cleavage of *Arbacia* eggs by the untreated sample.

Visitors to the coast of California are familiar with the signs prominently displayed on rocks and along the beaches warning them against the consumption of mussels from the first of May to the end of November. The reason is that during this period the mussels may be poisonous. The symptoms of mussel poisoning in man or animal may appear immediately after the consumption of toxic shellfish. They are entirely of nervous origin and begin with a prickly feeling, followed by numbness in the lips, tongue, and fingertips. The progress of the intoxication is accompanied by muscular incoordination with ascending paralysis. In severe cases death from respiratory failure may occur 2–12 hours after the consumption of the fish. Gastrointestinal disturbances are rare, although vomiting may develop in severe cases.

During the periods of low toxicity of mussels small amounts of poison are frequently ingested with the shellfish meat without any ill effect on humans. The mussel poison is, however, the strongest one known to science and one for which no antidote has yet been discovered. The substance is stable in acid or neutral solution, withstands heat, but can be gradually destroyed by boiling in alkaline solution. The exact chemical nature of mussel poison has not been determined, but it is known that the toxic principle belongs to the class of alkaloids such as strychnine, muscarine, and aconitine. Since the poison has no odor or flavor, ordinary inspection will not distinguish poisonous shellfish from normal ones.

The presence of poison is usually established in the laboratory by making tests on white mice or kittens. Prevention seems to be, therefore, the only safe method of protection. This consideration led the Health Departments of the Pacific Coast states to establish a quarantine for mussels in summer months and at such other times as laboratory tests show them to be dangerous for human consumption. Research conducted with the support of the William Hooper Foundation of the University of California provides evidence that the original source of mussel poisoning is a plankton containing large numbers of *Gonyaulax catenella,* a dinoflagellate that thrives during the summer months and frequently is the cause of the red water along the coast of California.

Like other lamellibranchs, mussels feed by filtering large quantities of water. They ingest the *Gonyaulax* and store its poisonous substance in the digestive gland without suffering any ill effect themselves. The *Gonyaulax* theory is based primarily on the correlation between the rise and fall of mussel toxicity and the increase and decrease in the abundance of *G. catenella* in coastal water. The toxicity of mussels completely disappears shortly after the disappearance of this microorganism.

Direct evidence of the toxicity of another species

Bulldozers were used to bury dead fish littering Florida beaches. (*St. Petersburg Independent* staff photo)

of dinoflagellates was demonstrated in 1938–39 by H. J. Koch, who showed that mussel poisoning in the marine canal between Bruges and Zeebrugge, Belgium, was caused by the ingestion by these mollusks of a microorganism described by Woloszynska and Conrad as *Pyrodinium phoneus*, n. sp. Taking advantage of a strong positive phototropism of this dinoflagellate, Koch collected large quantities of *Pyrodinium* and extracted from them a highly toxic substance which killed white mice in 90 seconds. Furthermore, normal mussels placed for 18 hours in water to which Koch added known numbers of *Pyrodinium* acquired strong toxicity, directly proportional to the concentration of the dinoflagellate.

The question of whether the toxic principle of red water, so destructive to fish, is identical with the paralytic shellfish poison, or whether we are dealing with two different chemicals, remains unanswered. Samples of Florida red water which yielded extracts toxic to fish were sent by me to the Food and Drug Administration of the Department of Agriculture for bioassy. The results were negative, however. Likewise, no shellfish poison was detected by the Fisheries Technological Laboratory in College Park in clams grown near Fort Myers in the area affected by red water. Present observations are not yet sufficient to arrive at a definite conclusion regarding this question.

The outbreak of the red tide in Florida in 1946–47 was accompanied by a strange phenomenon which greatly added to the discomfort and distress of the residents of the beaches and islands of the coastal area. With the onshore wind and breaking of the surf an odorless but highly irritating "gas" emanated from the water. It caused spasmic coughing, a burning sensation in the throat and nostrils, and irritation of the eyes. Local residents, experimenting with samples dipped from the sea, observed that strong coughing was produced by inhaling vapors emanating from the water heated over the kitchen range. For several days, when onshore winds persisted, life on Captiva and other islands of the affected area was very uncomfortable. Virtually the entire population was sneezing and coughing and suffering from other symptoms resembling those of a heavy cold or hay fever. The disagreeable experience strengthened the general belief that poisonous gases, dumped into the sea, were coming from the water. The "gas" did not, however, affect persons living a few hundred feet from the beach.

The occurrence of an irritating substance in dis-

colored sea water was not entirely new. It was reported from the same area by Taylor, who investigated the mortality of fishes on the west coast of Florida in October–November 1916. Taylor writes that "the gas was very violent and many people telephoned for medical assistance for 'cold in the head,' 'sore throats,' 'cold in the chest,' etc. I myself, have suffered quite acutely for the past five days, but the worst of the gas seems to be going now." Another occurrence of the "poisonous gas" coincident with a mortality of fishes was recorded in Texas by Lund in 1934. Woodcock, who studied the transfer of water droplets over the air in the red-water area in Florida, suggested that the irritating substance may be projected into the air when small bubbles burst on the surface of breaking waves. Indirect evidence in support of this view was found in the following tests. Spraying into the nose of a small amount of sea water containing 15–56 million *Gymnodinium brevis* per liter caused coughing and sneezing and produced a burning sensation in the nose and throat. A similar reaction was produced by inhaling samples of red water heated to about 80° C or slightly higher. At this temperature, clouds of fine bubbles rise to the surface and burst. The irritating effect ceases as soon as heating and bursting stop. Tiny water droplets arising from heated red water were caught by Woodcock on the surface of a microscope slide covered with hydrophobic silicon film. Examining the droplets under the microscope, in a special chamber which prevents evaporation, I found that they contained yellowish granules, in shape and color similar to the granules found in the cells of *Gymnodinium*. It is tempting to infer that these droplets contained the irritating substance, but more detailed study is needed before this question can be settled.

Is it possible to control the outbreak of red tide and prevent the mass destruction of fish and other marine forms? The question may be answered with certain reservations. Although control of any condition in the sea presents great difficulties, it may be not entirely impossible. Our ability to understand the cause of a phenomenon and to foresee its occurrence is the first step toward solving the problem of its control. Several attempts to stop the red tide, or at least to mitigate its effect, were made by Japanese scientists. Taking advantage of the known fact that dinoflagellates are very delicate creatures and can be killed by low concentrations of poisons, they applied weak solutions of copper sulphate, calcium hypochlorite, and free chlorine to destroy them in harbors and bays. To protect the valuable pearl oysters from contact with the poisonous plankton, they used simple mechanical methods and covered the oyster beds with matting. In the absence of detailed reports, it is difficult to appraise the practical results of these efforts. It is claimed by the Japanese, however, that they were successful.

From a purely theoretical point of view, chemical control of the red tide appears to be feasible. Red patches of water containing rapidly growing populations of dinoflagellates can be easily spotted from the air. By spraying or dusting them from a specially equipped airplane, growth of the dinoflagellates probably may be stopped, or at least localized. It appears doubtful, however, that any control would be possible after the red water has spread over many hundred square miles of the open ocean.

The red tide may be considered as a special case of the blooming of the sea. The study of this phenomenon leads into interesting and but little explored fields of science. It may reveal intricate and very complex conditions which determine the numerical relationship between the different marine populations and therefore greatly increase our knowledge of marine ecology The human health aspects of the phenomenon are fascinating because we are dealing with such a powerful poison. Studies of its origin, chemical composition, and mode of action may be the objects of fruitful scientific investigation and lead to important discoveries. Let us hope that explorations along these lines, which can be undertaken only by a large research organization, will no longer be neglected.

MEDICAL RESEARCH: OPERATION HUMANITY

ANDREW C. IVY

Vice president, professor of physiology, and head of the Department of Clinical Science at the University of Illinois (Chicago), Dr. Ivy (Ph.D., Chicago, 1918) has taught at Loyola, Chicago, and Northwestern Universities. His article is based on an address delivered during the Centennial Celebration of the AAAS, September 13–17, 1948.

MEDICAL research is a clear and simple example of "One World of Science." The knowledge derived from such research is directly applied in the practice of medicine, a field of human endeavor unified throughout the world. Medicine is the only world-wide profession that pursues the same humanitarian aims and uses the same methods everywhere. It possesses such catholicity of method, and there is such a universal demand for its services, that the physician can render his service in the same environment in every country in the world.

This universality is natural because, first, disease is a curse on all living things. It attacks all persons, irrespective of their position in society. When the methods of prevention and cure of a disease are truly known, they are universally applicable for the eradication or amelioration of the disease. Second, next to the conquest of hunger, the conquest of disease has been of greatest concern to all peoples. The benefits derived from the conquest of power and space, and the production of more comfortable living conditions, are empty luxuries without health to enjoy them. Third, sympathy for the sick, suffering, and dying is a widely felt sentiment, and it is everywhere recognized that the physician has reverence for the life of the individual and, through the individual, for the life of the community and of all humankind. The moral obligation of the medical and allied professions to provide assistance to the suffering has been respected by every civilized group except when insane fanaticism has suppressed all moral restraints. In warfare, the doctors, nurses, medical corpsmen, and Red Cross workers of belligerent nations are classified as noncombatants. It is understood that the participation of medical personnel in hostilities is for the purpose of salvaging, not destroying, life. Amidst havoc and conflict, medicine is now the only scientific agency that knows no distinction between friend and foe when disease and suffering are at hand. It is clearly recognized throughout the world that medicine and medical research constitute an operation for humanity, and for humanity alone.

THE UNITY OF MEDICINE AND SCIENCE

But medicine and the other branches of science are inseparable and have been so since the beginning. Simultaneously, the mysterious nature of disease and of the heavens stimulated the mind of primitive man, giving rise to medicine and astronomy, both closely associated with his religion Medicine, like astronomy, was the mother of many of the natural sciences. And it was a physician who first introduced and adopted the scientific process, or method, called in medical research the Hippocratic method.

The progeny of medicine and astronomy—chemistry, physics, and mathematics—have made tremendous contributions to medical research and the cure and prevention of disease, and there can be no doubt that progress in medical science is intimately related to progress in all the natural sciences. In fact, all branches of learning and creative effort move forward together. Moreover, it cannot be repeated too frequently that social and governmental attitudes may deter or facilitate progress. Governments and governmental agencies throughout the world should keep in mind that the doctrines of totalitarianism suppressed the free functioning of the human mind and entombed the gifts of Hippocrates, Aristotle, and Galen for a thousand years during the Dark Ages. This dogmatic authority had to be broken before the natural sciences and the humanities could again go forward together.

Thus, the factors involved in human progress are seen to be so interdependent that every group that has fostered creative and cooperative effort has contributed to the advancement made during the past hundred years.

A CENTURY OF PROGRESS IN MEDICAL RESEARCH

The tremendous reduction of human suffering and in the loss of human lives due to disease,

since 1848, is great cause for rejoicing, but the results of the application of science to the prevention and control of suffering have become so commonplace that many of us have lost our appreciation of them. Since the founding of the AAAS, vaccination, anesthesia, aseptic surgery, investigation of the causes and the methods of control of infections and parasitic diseases, radium, the X-ray, the electrocardiograph, sulfa drugs, aviation medicine, antibiotics, insect control, and the sciences of sanitation and nutrition have been introduced. In addition, phenomenal results have attended experimental studies that have been conducted in every civilized region of the world. Researches in the physiological, biochemical, pathological, bacteriological, and histological fields have rendered it possible to make diagnoses of disease and to apply surgical and medical treatments undreamed-of even as short a time as fifty years ago. These, the greatest humanitarian victories of science, have been won by the zealous efforts of countless investigators in many nations.

These achievements are dramatically demonstrated by the following facts: Life expectancy in 1850 was approximately forty years; in 1900, it was forty-nine years; and in 1945, sixty-six years. Thirty percent of persons who have now attained the age of sixty-five owe their survival to advances made in medicine since they were born. The advances made since 1900 have added five years to the life expectancy of those now forty years of age. Thus, not only have millions of lives in the United States and other countries been saved through reducing the number of deaths in childhood, but years of life have been added to those who pass the age of forty.

Mortality statistics of the United States Army from both world wars illustrate the advances of medical research during the past thirty years. The death rate from disease declined from 14.1 per 1,000 in World War I to 0.6 per thousand in World War II. The percentage of deaths among the wounded decreased from 10 to 3 percent.

There are numerous medical problems yet to be solved, such as mental disease, alcoholism, and the degenerative diseases. But those who recall how yellow fever, smallpox, cholera, plague, dipththeria, and other contagious diseases once killed hundreds of thousands of people, those who work with and for the sick and suffering, those who measure progress by the greatest happiness for the greatest number will agree, I believe, that the prolongation of life and the alleviation of suffering have been the greatest gifts of science to man during the past century.

SOME OLD AND NEW PROBLEMS

These achievements in the field of medical science have magnified some old and created some new problems:

I. The Problem of Degenerative Disease and Old Age.

II. The Problem of Population and Hunger.

III. The Problem of Biological Warfare.

It is at once obvious that these problems have important medical, social, political, economic, and moral aspects. And, on the basis of the history of the success of the natural sciences and the relative failure of the social sciences, or the failure to develop a science of humankind, the medical aspect of these problems appears to be the least difficult.

I. *The Problem of Degenerative Disease and Old Age.* We have reached a turning point in the history of medical research. The emphasis in the past, and properly so, was on the control of acute diseases, which, except for the virus diseases, have now largely been more or less checked. In the future the emphasis must be on the alleviation of chronic degenerative diseases.

The importance of this problem may be illustrated by the following figures. Almost 29 percent of the people in the United States are over forty-five years of age; by 1960, 33 percent will be over forty-five years of age. Almost 11,000,000 persons are now over sixty-five years of age (7.7 percent); by 1960, 9 percent of the population will be over sixty-five years of age. The shift of our population from youth into old age will probably occur more rapidly than is now predictable, because the mortality from pneumonia, tuberculosis, and other infectious diseases is rapidly decreasing. This shift in age will obviously increase the number of people with degenerative diseases and chronic illnesses. Even now degenerative diseases, such as chronic heart disease, hardening of the arteries, chronic nephritis, and cancer, account for about 6 percent of the deaths in the United States. Thirty percent of the patients admitted to mental hospitals are sixty years of age or more, and most of these patients have senile psychosis because of cerebral arteriosclerosis. One and a half million people have the degenerative form of arthritis. Seventy percent of persons now disabled from all causes are forty-five years of age or more.

Unless, through medical research, we discover a way to decrease the disabilities incident to chronic illness and old age, our economy and our social organization as we know them today will have to undergo many changes. One fourth of the population will be working to take care of the other

three fourths. This emphasizes the importance of planning productive work for the aged. When a person is forced to retire because he cannot keep up with the production line, he should not be placed on the unemployable list. Work he can do should be found for him. Observation of the aged indicates that sudden inactivity speeds aging and the time at which custodial care is required.

Thus, medical research has been creating an obligation for future generations to care for proportionately many more old and disabled persons than are being cared for at present. The only way this obligation can be lightened by medical research is to give more attention to the cause and prevention of degenerative disease.

The next important contribution to be made by medical research is "to prolong useful life." Most informed medical scientists feel certain that this phase of the medical problem can be solved reasonably well given sufficient time and financial aid. Medical science, in which I include psychology, can give sound advice to the social sciences regarding the solution of the economic problem of old age by measuring aptitudes and abilities for employment.

II. *The Problem of Population and Hunger.* Today approximately 175,000 infants are born daily. The number has been increasing during the past hundred years, and the percentage of survivals has been steadily increasing as a result of the application of the findings of medical research. In 1830, the population of the world was 800 million; in 1900, 1.6 billion; and in 1939, 2.2 billion. At the present rate of increase, and with the spread of medical science, the population of the world will be 4 billion by the year 2000.

Is this a pleasant prospect? Not when we remember that a surplus of food has never existed in the world, that to provide everyone with a diet of 2,600 calories a day during the next twenty-five years the world's food production will have to be doubled. This does not seem possible when there are only 4 billion acres of land that can be used for food production by present methods, and when 2.5 acres of farm land are required to produce an adequate diet for one person. This serious situation is made worse when we consider that practically nothing is being done in the present food-producing areas of the world to prevent soil erosion and depletion. Soil erosion and depletion caused the transformation of garden spots into deserts in Greece, Syria, Northern Italy, Africa, Mesopotamia, and the uplands of China; today we hear of dust storms in the Volga Valley, in South Africa, Australia, Argentina, and the United States, the "breadbaskets" of the world; and dust storms mean soil erosion and soil depletion. The following question has recently been asked by C. L. Walker (*Harper's Magazine,* Feb., 1948): Shall man's food and population problem continue to be resolved by famine, disease, malnutrition, and wars caused by peoples in need of living room?

This population problem, obviously, has been and will be further magnified by the application of the results of medical science. It must be solved by the social sciences and the humanities. Medical science can provide information regarding the nutritional requirements, and perhaps science can find some nonagricultural way of producing food, but it would seem urgent *now* to check soil erosion and depletion in the still-existing agricultural areas of the world and to discourage population increases. It seems certain that one way to decrease the rate of population growth is to increase living standards; in the past an increase in living standards has been associated with a decline in birth rate, and the same will probably hold true in the future.

III. *The Problem of Biological Warfare.* Knowledge of the infectious diseases that has made it possible to save lives and to protect the production of food may soon be used to destroy human, animal, and vegetable life. Nowadays we read or hear almost daily that biological knowledge—like chemical and physical knowledge—threatens civilization. I have not seen or heard of anyone who seriously doubts it, and it would seem obvious that knowledge may be used for good or for evil purposes. The possibility of the destruction of man by his own inventions was known to the classical Greeks. In the field of medicine, Hippocrates saw the problem and provided a solution. He clearly realized that the scientific and technical philosophy of medicine represented by him and his school could not survive without a sound moral philosophy. The moral code he formulated has been a controlling influence in determining the policies of the medical profession and the conduct of the physician for twenty-two centuries.

SCIENCE AND TECHNOLOGY ARE NOT ENOUGH

We have reached the point in the development of science and civilization where it is clear that they cannot survive without a sound moral philosophy. Science and technology are not enough. In themselves they have no survival value unless used as tools for the attainment of some great

altruistic goal for the good of all mankind. There is no lack of a goal toward which the energies of all humanity may be religiously directed. The conquest of hunger and disease has not yet been accomplished. Pestilence and famine still exist in many parts of the world, and famine promises to increase in the future unless a world-wide effort is begun now to prevent it.

Hunger and disease are common enemies of all mankind. It is high time that the people of the world join in an effort to improve the food-production potentialities of the world and to eradicate pestilence. Men must think *first* of their common enemies in nature and unite to bring these enemies under control. The natural enemies of man will win the conflict unless men cease wasting their energies and intelligence fighting among themselves.

Medicine and medical research are moving in the right direction through the World Medical Association and the World Health Organization. It is the duty of these organizations to eradicate pestilence. All the natural sciences must rally behind the Food and Agriculture Organization, whose duty it is to bring hunger everywhere under control.

Finally, I want to take this opportunity to direct an appeal to the political and military leaders of the nations of the world.

The results of medical research and the efforts of medicine have directly and indirectly saved the lives and increased the happiness of millions of people. People everywhere know that for centuries medicine has worked to promote human welfare. This moral obligation of medicine and its allied professions has been universally recognized. People everywhere have faith in the fact that their physicians, even under conditions of warfare, are sworn to conserve life, and that the function of medical research is to prevent and cure, and not to cause, disease.

Until recently there never has been any question of directing medical research and knowledge toward any objective other than the improvement and preservation of human life. But it has lately been proposed that bacterial warfare might be used *aggressively*. In fact, I have seen Nazi documents in which a committee reported that America could be conquered only by afflicting the people, the food, animals, and plants with disease. It is very important to increase our knowledge regarding methods of preventing all contagious diseases. It is important for all nations to energetically promote research toward the provision of a complete defense against bacterial warfare. But I plead with those political and military leaders who would use bacterial warfare as an aggressive weapon to ponder the ultimate effect on themselves, and on their own people, combatants and noncombatants alike, of crushing the faith that all people have in the humanitarian spirit of medicine. Woe unto the society that would crush and not cherish and cultivate this spirit among its people, medical scientists, and physicians.

Those who have little faith in the effectiveness of such a plea may gain consolation from the fact that the development of a strong defense against bacterial warfare will render its use unlikely. The best way that medical research can make certain that its knowledge will not be used by predatory men against the welfare of man is to prepare a complete defense. Although this may not be possible in chemical and atomic warfare, it is entirely feasible in the case of biological warfare.

Past and present religious leaders, educators, and scientists have made pleas for unity, peace, and concord. Thus Pasteur in 1888 called for an end of war and destruction and urged adherence to "the law of peace, work and health whose only aim is to deliver man from the calamities which beset him." Recently Albert Einstein has stated that "World government can come through agreement or through the force of persuasion alone, hence at low cost. . . . It will not be enough to appeal to reason. . . . Unless the cause of peace based on law gathers behind it the force and zeal of a religion, it hardly can hope to succeed."

In these two statements we have the unifying principle that will cultivate the growth of the peace-producing forces and encourage understanding all over the world. The calamities of disease and hunger stir the emotions of all people. The energies and emotions that governments marshal to win a war against a human enemy should be as readily marshaled against hunger and disease. The creation of harmony between man and his enemies in nature—disease and hunger—will go far toward creating harmony between man and man. The efforts of medical research, the natural sciences, the social sciences, and the humanities—of all who are interested in the spiritual and material welfare of man—will be required for the success of this Operation for Humanity.

AN IMMIGRANT CONQUERS A CONTINENT: THE STORY OF THE WILD GARLIC

HUGO ILTIS

Dr. Iltis (Ph.D., Prague, 1905) has been director of the Gregor Mendel Museum at Mary Washington College of the University of Virginia, Fredericksburg, for the past ten years. In May 1943 The Scientific Monthly *published his article on "Gregor Mendel and his Work," which has since been widely reprinted.*

ONE of the first botanical observations I made after emigrating from my native Czechoslovakia to Virginia was the common occurrence, even prevalence, of European weeds along roads and near human habitations. Whereas in Europe only a few American weeds have become full-fledged naturalized citizens—such as *Erigeron canadense* in waste places, *Erechtites hieracifolia* in clearings, the South American *Galinsoga parviflora* in gardens, *Elodea canadensis* in pools and rivers, etc.—in America scores of different species of European roadside flowers and field weeds almost displace the native ones in some regions. The explanation seems to be simple enough: the European settlers brought to America not only the seeds of most cereals, but also those of different weeds; whereas only a few cultivated plants were taken from America to Europe, and the seeds of those few (corn, tobacco, tomatoes, etc.) were not too likely to be contaminated with many weed seeds.

The strangest encounter, however, was my meeting with one of the most troublesome American weeds, *Allium vineale* L., the wild garlic, or the wild onion, as it is called in other localities. I remembered immediately that years ago I had had to walk several miles from my native Brünn just to get a few specimens of this plant for our herbarium. How had the plant which was of scattered or rare occurrence in Europe become a common weed in America? I first looked in several European floras to confirm my memory. Hegi, in his *Flora of Central Europe,* writes about the distribution of *A. vineale* L.: "Here and there in vineyards, on sandy fields, sunny hills, on ridges, lawns, roadsides. . . ." Potonie's *Flora of North and Central Germany,* 1910: "Scattered, mostly on sandy hills and fields, rare." Wildt's *Botanical Excursions Book for the Surroundings of Brünn* mentions just one single locality where *A. vineale* L. is to be found.

It is, however, somewhat different at the circumference of its native area. In some of the older English floras, *A. vineale* L. is not considered to be a weed. J. D. Hooker, in his *Student Flora of the British Islands,* 1884, for instance, writes: "*A vineale* L.—Crow Garlic—pastures and waste, dry places, from the Clyde and Aberdeen south, not frequent. . . ." But it became a troublesome weed in England and in southern Scandinavia in modern times, as is shown by recent publications of R. H. Scott (Life History of the Wild Onion and its Bearing on Control. *J. Min. Agric.,* London, 1944, 51) and R. H. Scott-Richens (1947). The little map in the second paper shows an area in south-central England where *A. vineale* L. became a serious weed. The earliest record Mrs. Richens could find was in Trow's *Flora of Cardiff,* where it was stated to be serious in the vale of Glamagan, South Wales, at the end of the eighteenth century. It is of little importance in that locality now. In regard to its occurrence as a weed on the European continent, Mrs. Richens writes in a letter to the author: "Sweden is the only country I know of where it is taken seriously. I rather think that a maritime habitat is its natural one and that it has spread from there, becoming a serious weed whenever conditions were favorable." For a taxonomic group "center of the area" refers—after Cain—to the center of its origin, that is, to the territory where the dispersal and the immigration began. Its determination for *A. vineale* would be difficult at the present state of our knowledge.

There are only small, restricted areas in its European home country where *A. vineale* L. is a troublesome weed, but it has become one of the most obnoxious weeds ever introduced into the middle Atlantic and south-central parts of the United States. From Europe it was brought to the United States in the seventeenth or the early part of the eighteenth century. The "*Allium arvense odore gravi, capitulis bulbosis rubentibus,*" as it is referred to in Gronovius' *Flora Virginica* (1739, p. 32), is very probably identical with our species. We know from the investigations of F. J. Pipal that by the middle of the eighteenth century it was in full possession of

many farms in eastern Pennsylvania. In the first volume of the *Transactions* of the American Philosophical Society, Henry Hollingsworth, of Philadelpia, writes: "In April, 1754, I perceived the wheat much colored with garlic; and at harvest found in many places parts of the field almost every tenth head was garlic, which rendered the wheat unfit for use. . . ." It is evident from his introductory remarks that his experience with wild garlic was not uncommon. Dr. Barton, in his *Compendium Florae Philadelphicae,* published in 1818, calls wild garlic "a common and pestiferous weed which has obtained such footing in pastures that it is impossible to extirpate it." Nuttall, in *The Genera of North American Plants* (Philadelphia, 1818), writes (p. 34): *"A. vineale* L.—naturalized, now a prevalent and injurious weed." In Gray's *Manual of Botany* (1848, p. 493) *A. vineale* L. is said to be naturalized and troublesome near the coast. C. C. Deam, in his *Flora of Indiana* (p. 309), writes: "My specimens are all from southern In-

Above, left: forma typicum Beck and *forma capsuliferum* Koch; *right: forma crinitum* Jacob showing mature plant as found in mid-June, aerial stalk carrying awned bulbils and central soft bulb surrounded by several hard-shelled bulbs.

diana where it is one of the most pernicious of all weeds. A pioneer who lived in Point Township, Posey County, told me that when he was a boy (about 1860) both the garlic and wild onion were common in the woodland."

Historical research has brought to life some especially interesting features. It has shown that wild garlic, where it had gained a foothold, was as great a problem in the United States more than a century ago as it is today. But there is also no doubt that wild garlic is slowly taking possession of a constantly increasing area in the middle United States. The present distribution of wild garlic —its "artificial area" (Cain)—ranges from Massachusetts south to northern Georgia, west to Missouri and Mississippi. It is most abundant in the belt of territory extending from New Jersey through Delaware, the Virginias, North Carolina, Kentucky, Tennessee, and along the southern portions of Pennsylvania, Ohio, Indiana, and Illinois.

Wild garlic seems to have little or no preference as to the character of site or soil. It is most widely distributed in the river valleys and creek bottoms, owing to the fact that the bulblets are carried and scattered by the water. It is also frequently found on higher, rolling land. It thrives in clayey as well as in alluvial and loamy soils. It is not characteristic of any particular kind of crop, but may be found in any of them, being most troublesome in meadows and pastures and in the wheat crop.

What are the characteristics that make the wild onion such an efficient combatant in the struggle for life? A description of the plant and its means of reproduction will supply an answer. The mature plant has a 30–70-cm high flower or aerial stalk growing out of a bulb, which is surrounded by a withered leaf, forming a thin, torn membrane. The bulb contains at maturity one large major offset, called the soft-shelled bulb. It is developed near the center and covered by a thin, slightly buff-colored, glossy membrane. It is ovate in its longitudinal section and convex on its outside, with the concave inside clasping the base of the scape. There are in addition several (1–6), mostly small, sharp-edged minor offsets with sharp terminal points enclosed by hard, straw-colored, shell-like coverings; these are called the hard-shelled bulbs. The aerial stalk coming out from the center of the bulb is in its lower part still partly covered by the dry remains of the 5–6 foliage leaves and carries the head, consisting either of flowers, of bulbils, or of both. The head shows at its bottom the remains of the 2 bracts that enclosed it before maturity. From the composition of the head taxonomists have distinguished several varieties of forms (see Britton and Brown, Fernald, etc.). There is, first, *A. vineale* L. *forma typicum* Beck, with a more or less loose umbel consisting of both bulbils and flowers; second, *A. v. forma compactum* (Thuill.), with a compact head consisting only of bulbils of a whitish or greenish color; third, *A. v. forma fuscescens*, Ascherson and Graebner, with a head consisting of bulbils of a reddish color; fourth, *A. v. forma crinitum* Jacob, bulbils forming the head tipped with long, green, capillary appendages; and, finally, *A. v. forma capsuliferum* Koch, with an umbel consisting of flowers only. The flowers are pedicellate, pale rose or violet, the perianth segments tinged with pink, or sometimes with green, along the keel, this being most marked in those of the outer whorl. The capsule is elongate and trilocular, the seeds black, triqetrous, two in each loculus, but rarely all developing. *A. vineale* has (after Darlington and Janaki) 32 chromosomes. Since 16 is the chromosome number of the majority of the *Allium* species, it is apparently tetraploid. Apogamy is common in *A. vineale* (after Hunger and other authors).

The species *A. vineale* would be, after the definition given by Camp and Gilly, a parageneon, containing aberrant but interfertile genotypes, or formae. The different formae are possibly the result of mutations. The term " forma" is applied since it should be restricted to those variant individuals that occur intermixed with those of the species.

The five varieties or, better, forms described above are not dependent upon the locality or the environment but often occur together. They all occur in Virginia, the *formae fuscescens* and *crinitum* being most common, the *formae compactum* and *typicum* coming next, and the *forma capsuliferum* being the least frequent. There are, however, often transitions between these forms. To all appearances these forms seem to be dependent upon Mendelian allelic genes, and their occurrence at the same places to be like a segregation within a hybrid generation.

The wild onion has, therefore, as means of reproduction, first, the major offsets, or soft-shelled bulbs; second, the minor offsets, or hard-shelled bulbs, both underground; third, the red aerial bulbils with long green appendages; fourth, the red bulbils without appendages; and fifth, the whitish or greenish bulbils without appendages. Finally, there are the seeds resulting from sexual reproduction. Each of these six ways of reproduction is very effective. Together they give to the wild garlic all the advantages necessary to survive in the struggle for life and they make it a trouble-

Varieties of the wild garlic (*Allium vineale* L.): *Left to right: A. v forma crinitum* Jacob, not ripe; *A. v forma crinitum,* ripe (dry); *A v forma compactum* Thuill; *A. v. forma typicum* Beck (two forms); and *A. v. forma capsuliferum* Koch.

some weed, one extremely difficult to eradicate.

Propagation of wild garlic by seeds takes place mostly near the southern limits of its range. There are no seeds formed on wild garlic in England (Scott) or in Indiana (Pipal). In Tennessee (Watts) and in Virginia, seeds are abundantly produced. Nearly 100 percent of the seeds are viable. From about a hundred seeds I planted at the end of August, the great majority (95) had soon formed a grayish-green terete leaf surrounded at its base by a yellowish or purple sheath. It was growing out from a small bulblet the size of a large wheat grain, which was fixed to the ground by 3–5 slender roots, each 1–3 inches long within two weeks. The sheath at the ground proved to be purple if the seeds had been taken from a plant with purple bulbils (*forma fuscescens*), but yellowish if taken from a plant with whitish bulbils (*forma compactum*). Of course the growth of the young plants was stopped during the winter and was resumed the next spring. In the summer following this spring (June), the bulblets had reached the size of a large pea or a small hazelnut. But only one bulblet had developed from the plants grown from seeds at the end of the first year.

The central, or soft-shelled, bulb starts its growth early in the fall and often continues to grow during the fall and the early winter months. From the soft bulb usually originates the strongest plant, which may again produce an aerial stalk. The terete gray-green leaves produced by that bulb are 8–10 inches long before the frost begins. Growth is then stopped and is resumed in the spring when the formation of new adventive underground bulbs begins. These bulbs, the new soft bulb and several hard-shelled bulbs, originate within the axils of the fleshy, scalelike leaves of the maternal soft bulb. During April and May the coverings of these new bulbs grow tough, and they become separated from the parent bulb. Each bulb develops an increasing number of roots arranged in a semicircle around the attachment scar.

The sharp-edged, hard-shelled, or lateral, bulbs do not start to germinate the same season as do the soft-shelled bulbs, the aerial bulbils, and the seeds. They sprout the next summer at the earliest. Most of them, however, exhibit a marked dormancy and do not germinate for at least two years. Tinney deduces from his experience that the maximum period of dormancy of the hard-shelled bulbs is about six years. Thus, these hard-shelled bulbs become the means by which the wild garlic may survive through unfavorable seasons, through drought and frost, and through fungus infection.

The aerial bulbils are the most effective means of reproduction of the wild garlic. Almost all mature wild garlic plants, with the exception of the rarer *forma capsuliferum,* which has umbels consisting of flowers only, produce heads or clusters of aerial bulbils. Each plant normally produces a single head, but it is not uncommon to find stalks with 2, 3, or even 4 heads. The aerial stalk bearing the heads starts to elongate in early April and emerges in May from the sheath of the innermost foliage leaf as a green, terete scape. Its tip carries the head, which is first small and completely enclosed within a membranous spathe. The spathe bursts in June, exposing one or more heads consisting of bulbils or of bulbils and flowers or of flowers only. The bulbils need about four to six weeks to ripen.

There are three well-distinguished kinds of bulbils, each kind arranged separately in different heads. The first form consists of whitish or yellowish bulbils with pointed greenish or purple tips, which are slightly bent to one side. These whitish heads do not at all—or very seldom—show the long green appendages but often contain in addition to the bulbils a small or large number of flowers. The

number of bulbils in these, as in the other forms, varies a great deal. There can be as few as 20 and as many as 150. If there are few, they are usually large (5–8 mm long); if there are many, they are small (3–4 mm). The bulbils look very much like grains of wheat or rye. Each bulbil consists of one cone-shaped, fleshy scale, which contains a growing point and which may include near its base just above the scar the first young root. Each bulbil is surrounded by a kind of coat consisting of two or three cell layers. The color of the bulbil depends on the color of the second cell layer under the epidermis of the bulbil itself. Its vacuoles are colored by anthocyanin in the purple bulbils; the vacuoles are colorless in the whitish ones. The second, more common, form consists of dark-purple bulbils similar in shape to the whitish bulbils, also without awns. The difference between these two forms is clear-cut, and there are generally no transitions present. The two characters purple and white seem to behave like a pair of Mendelian characters. The white color may depend upon a recessive gene inhibiting the formation of anthocyanin. The heads consisting of purple bulbils may also contain some flowers, although the formation of flowers occurs more commonly among the whitish bulbils.

The third kind of heads is characterized by long green appendages, sometimes 2–3 inches long. The bulbils carrying these appendages are almost always red or purple in color because of the anthocyanin in the cell layer beneath the epidermis. The appendages, which are already fully developed within the spathe before its bursting, look very much like the young leaves of the wild garlic. Some authors mistook the appendages for the result of germination of the bulbils within the head (Hegi, *Flora von Mittel Europa*, **II**, 217, *"Brutzwiebeln . . . wachsen gelegentlich bereits am Stengel aus"*). Such a head of bulbils, each one ending in a long, green, terete leaf, really resembles an inflorescence of *Poa vivipara* or of *Juncus bufonius* with their viviparous bulblets— ". . . the outer leaf shows on its tip a distinct blade thus imitating a bulbil already germinating" (Kirchner-Loew-Schroeter). But a more exact investigation instantly shows the difference. The appendage is nothing but the continuation of the coat, or cover, of the bulbil. The coat of the bulbil is purple, thin, and more or less transparent, however, and the appendages are green parenchyma rich with chlorophyll. The epidermis of these appendages is characterized by the large number of stomata. The epidermis of the green aerial stalk carrying the head of bulbils showed an average of about 10 stomata in the field of view of my microscope, whereas the epidermis of the appendages showed an average of 14–15 stomata.

This fact may help us to understand the function of these appendages. It may be similar to the function of the awn of the grasses (Zoebel, 1893) or of the elongated style of the fruits of *Geum* (Iltis, 1913). It is an organ of transpiration helping to accelerate the metabolism and the ripening of the bulbils. A preliminary experiment seems to support my hypothesis. On a waste field with several hundreds of mature specimens of the wild onion, the awns of 50 heads were cut off with scissors and 50 similarly awned heads in the neighborhood were left to ripen undisturbed. The experiment was started on June 1, and the heads were harvested three weeks later. There was no difference in the shape or size of the bulbils visible whether the awns were cut or not. There was, however, some difference in regard to the tempo of ripening. Those heads with the awn still present seemed to have

Section through aerial bulbil with green appendage (awn). The bulbil is enclosed by a coat, the continuation of which forms the awn. The bulbil is formed by a fleshy scale, which encloses the growing point. A young root is formed near the scar.

ripened more quickly. The awn became dry and brown. The drying started at the tip of the awn and progressed down to the bulbil. Finally, the bulbils dropped off, some of them singly, some in clusters. The clusters were kept together by threadlike, wrinkled, dry awns. The aerial bulbils, whether single or in whole clusters, could be carried by the wind for a short distance. But they were spread especially by rain and floods and probably by manure, since a percentage of the bulbils went undigested through the digestive tracts of birds and mammals.

The main function of both the awn and the coloring by anthocyanin seems to be to accelerate the metabolism and thus to quicken ripening. This turns out to be an advantage to the wild garlic where it is a weed in wheat fields. The bulbils ripen at the same time as the wheat, and, since they look very much like the wheat grain, they are harvested with the wheat and may be sown together. It is, however, a disadvantage to the farmer since the wheat by the admixture of garlic bulbils becomes "garlicky," decreases in value, and may even become unfit for consumption.

Almost all aerial bulbils are viable. Of a hundred bulbils planted on September 24, the great majority (about 90) started to germinate around October 1, forming in about a week the first gray-green, terete leaf coming out from a purple or whitish sheath. Two weeks later the second leaf was formed. At that time the bulbils themselves had increased in size and become about as large as pea seeds. Some weeks later, around the middle of November, secondary bulbils started to form at the base of the primary ones. The secondary bulbils developed between, and at the base of, the scaly layers of the primary one and grew during the winter (December) into separate plants. When the young plants were taken from the ground the following June, 2–6 bulblets had been produced by each aerial bulbil. Each one had grown into a separate independent small plant with its own root system. Each of these secondary bulblets may itself form new bulblets next year. Thus the number of young plants may be very much increased.

This method of reproduction by formation of an increasing number of secondary and tertiary bulblets from one primary aerial bulbil is especially characteristic for the very troublesome lawn variety of the wild garlic. Everyone in Virginia, and probably in all southeastern states north of Georgia, knows the brushlike bunches of green garlic leaves, 5–15 inches long, which appear everywhere on the lawns in early spring before the grass starts its growth. They disappear during the hot summer months, only to reappear again in the late fall. If we pull out such a bunch of garlic leaves from the ground after a rain we will be surprised at the large number of bulbils at its base. There are very often more than a hundred: in some instances I counted almost two hundred.

The lawn variety and the similar variety appearing in pastures are characterized by two distinct periods of growth, one in the spring and one in the fall. At some places, especially where the winter is very hard, there are also two distinct resting periods, one during the hot summer months (July, August) and one during the cold winter months (January, February). At other places, where the winter is milder, the leaves stay green all winter, although their growth may become retarded.

The wild garlic in pastures, where the grass is cut by grazing instead of by mowing, is prevented from forming aerial stalks and, therefore, from reproducing either by seeds or by aerial bulbils. Instead, the reproduction goes on scarcely less effectively underground. The rapid reproduction of wild garlic in the lawn is a mere nuisance impairing its smooth appearance. The garlic does real damage in pastures, however, since the milk from cows grazing there acquires a garlicky flavor. The cutting of

a, young plant grown from aerial bulbil, two weeks after planting (November); *b*, plant grown from aerial bulbil, six months later (June), secondary bulbils developed; *c*, small part of a bunch of young plants of the lawn variety taken from the ground in November.

The lawn variety of the wild garlic. Bunch of plants, with soil, taken in November and, *right,* the same bunch cleaned by running water, showing bulblets and roots.

the grass at regular intervals—and similarly the regular grazing of the pastures—keeps the wild garlic from reaching the mature stage, but, on the other hand, stimulates the formation of secondary and tertiary bulblets.

"There are few weeds, if any, which cause greater loss to the farmer within the region where it thrives than wild garlic," writes Pipal in his excellent investigation (Wild Garlic and its Eradication. *Agric. Exper. Sta. Purdue Univ. Bull. 176,* 1914). Wheat suffers the greatest injury, for the garlic bulbils are harvested with the wheat seeds.

Badly infested wheat cannot usually be sold and is fed to the stock. In the milling of garlicky wheat the garlic bulblets gum the rollers and the buhrstones and interfere seriously with the grinding of the flour . . . the garlic flavor is imparted to the flour as well as to the food made from it. . . . In pastures where garlic is a veritable pest to the dairy farmer, milk of cows fed on garlic has an unpleasant flavor, also the butter made from it (Pipal).

Many attempts have of course been made to find methods of eradication. The high reproductive capacity of the wild garlic, the high resistance against chemical agents of the thin cylindrical leaves with their waxy surface, the ability of the hard-shelled bulbs to stay dormant for several years and to endure periods of drought or cold, the formation of secondary bulblets by each aerial bulbil—all make the problem of eradication a most difficult one. Where the infection is slight, as in lawns, pulling by hand may be effective. It has been recommended (Tinney) that spring crops be rotated over a period of six years, thus destroying the plants in the middle of the growing season each year. It is also claimed that late fall plowing will delay the production of hard-shelled bulbs, which can finally be prevented from forming by the spring cultivation. This method has already met with great success (Scott, 1944). Since the reproduction by seeds or by aerial bulbils is the most efficient method, any way by which the immature heads could be removed would be of the greatest importance. Unfortunately, no controlled large-scale experiments have been made in this direction until recently. Thus, the wild garlic will remain an obnoxious weed in the eastern and the middle United States for years to come, as it has been for more than two centuries.

There are still several unsolved problems in regard to the wild garlic that are worth investigating It would be interesting to find out whether there are cytogenetic differences corresponding to the morphological and anatomical characters of the different forms. Although it is clear how, by its excellent equipment for the struggle for life, the wild garlic conquered the country into which it came as an immigrant, it is not known why the same equipment did not have the same effect in its old home, And, of course, the discovery of an efficient method of eradicating the wild garlic as a weed would be of great value.

THE BASIC CONSERVATION PROBLEM

LAURA THOMPSON

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VIEWED as a whole, the basic conservation problem in any community involves all of life, human groups and their cultures as well as the fauna and flora, in the total environmental setting Local conservation programs, if they are to be practical and economical, must not ignore the active and potent human factor. Indeed, to be effective, from the long-range viewpoint, conservation programs should aim at fostering the balanced organization and health of the total community, the human population as well as the plants and lower animals, in relation to the natural resources.

ISLAND COMMUNITIES

It is well known that isolated natural communities tend in time to develop a delicately balanced ecological organization wherein, through a process of symbiosis and accommodation, the various species of flora and fauna attain a mutually advantageous and self-regulating adjustment within the total environment. Ordinarily, we think of the life-web process mainly in terms of plant and animal species in relation to the soil, microorganisms, water supply, and other natural resources. But in remote, isolated areas, such as many island communities, where natural processes have proceeded for centuries undisturbed, it is clear that all of life is involved in the self-regulating adjustment. Seen whole, the total island population of plants, animals, and human beings is a complex, interdependent community, each factor related to the other and to the land base, fishing grounds, soil, fresh water, minerals, and other resources.

Moreover, if we study traditional island cultures from the conservation viewpoint, we find a tendency to organize and integrate the habits of feeling, thought, and behavior of the social group with the world of nature in a way that, in the long run, fosters rather than destroys the web of life. Native conservation measures are often so diffused throughout the culture that their total, cumulative, life-preserving, and life-promoting functions are overlooked by outsiders, even anthropologists. Nevertheless, these measures are present and are often highly effective.

In functioning South Seas cultures, for example, first-fruits taboos, whereby the entire crop of breadfruit, mangoes, or other important native foods is protected until it matures, prevent vital means of subsistence from being consumed before they are properly ripened. This custom not only increases the size of the total crop and provides a mechanism whereby a surplus may be set aside against time of scarcity, but it also improves the native diet by making available the full food value of the mature crop. Indigenous fishing regulations organize offshore and deep-sea fishing activities to take advantage of the weather, the seasons, and the habits of various edible species, as well as to protect local fishing grounds from overfishing and undue disturbance. The development of the men's house complex, correlated with certain taboos on sexual intercourse (as, for example, during the nursing period after childbirth), functions as a means of birth-spacing, population control, and the protection of infant and maternal health. And, as a final illustration, the use of fresh water in functioning island cultures ordinarily does not exceed the annual supply. Indeed, on limestone islands, such as Lau in Fiji, where the natives have access to practically no fresh water except the rain water they are able to catch, a culture has developed in which fresh water plays a negligible part. The islanders drink coconut milk, boil their food in coconut cream or steam it in the earth oven, and bathe in the sea. Fresh-water problems arose when the missionaries introduced a new concept of modesty requiring the wearing of lava-lavas made of cloth. Today, on some islands, almost all the available water, both fresh and brackish, has to be used for laundering.

Space limitations forbid further development of this point, but the evidence points to the conclusion that isolated human groups, through their cultures, tend in the long run to cooperate with natural processes and to foster rather than destroy the web of life. In other words, the culture process tends through time to complement and reinforce the basic nature process.

This conclusion is of utmost significance to the total conservation problem and to human welfare

in general, for it means that ecological communities are composed of human beings, as well as other animals and plants, within the total environmental setting, with human beings traditionally playing an *active and positive* part in the multidimensional attaining and maintaining of a balanced, healthy adjustment. Man, through his culture, tends to cooperate with nature in a mutually dependent, life-promoting endeavor. This means that the basic conservation problem is not primarily one of how this topsoil may be saved, or that species of bird, tree, or animal may be protected from extinction, or of which forests, watersheds, or fisheries should be made into reserves, or of how a unique biotic equation or archeological monument may be preserved for posterity. Rather, the basic conservation problem is that of using and adapting indigenous attitudes, roles, and institutions, supplemented where necessary by appropriate new ones, to the end that the resident groups of each area, with the advice and help of scientists and administrators, may foster the emergence of balanced, healthy total communities.

It is necessary to stress the human factor in the conservation problem because there is a strong tendency among scientists and laymen alike to think of conservation mainly in terms of birds and forests, soil, and water supplies, rather than in terms of a balanced relationship between local populations and their natural environments. Man is not only a major factor in the life web; he is the *only* agent whereby a conservation program for a local area may be actively implemented.

There is a very real danger that a one-sided or limited conservation blueprint, drawn up by highly specialized technicians outside the area under consideration, may inflict a great hardship on the indigenous populations to the extent that it is made effective. If, for example, in order to preserve a near-extinct species of bird or a certain biotic arrangement, we forbid the people access to relatively large areas of their privately or communally owned lands (as is implicit in recommendations of the Pacific Science Board), we may be denying them vital resources from forests, uplands, streams, fisheries, or garden lands on which the subtle balance of their economy has long depended. If, furthermore, we appropriate such resources without valid compensation, as is often done (for example, with the entire output of phosphates on Angaur Island, the natives' only nonrenewable cash asset, now being shipped to Japan), we violate every tenet of just and economic government administration.

Manifestly, the only practical and economical approach to a coordinated, long-range conservation program is to enlist the interests and efforts of the local groups themselves. If there is to be an effective conservation program, it is primarily the people who will have to organize and carry it out. The indigenous populations are, as a rule, much more vitally and personally concerned than scientists or administrators in conserving local values—whether they be birds, fish, trees, or garden lands. From the practical standpoint, they are the ones fundamentally involved in a local conservation program, for their welfare, their very existence, depend on it They have proved in the past, and can now and in the future prove, their capacity to devise practical and effective means to implement such programs.

But they need help. Modern scientific discoveries and technologies, hand in hand with alien, exploitative ideologies, purposes, and habits, have to a large extent created the conservation problem. The destructive processes thus initiated have now proceeded so far that they are seriously interfering with the recuperative powers of nature. Unless the resources of modern science are applied to the problem, the development or restoration of community balances may be indefinitely delayed. It is too late for *laissez faire*. To ensure the emergence of new, healthy adjustments between nature and man, we must foster the curative processes of nature through the methods of the social, biological, and physical sciences.

MICRONESIA, A TYPICAL ISLAND CULTURE

The problem of conservation in Micronesia, currently under discussion by the Pacific Science Board of the National Research Council, is of utmost concern to all those interested in the welfare of such island cultures. Our Trust Territory is composed of hundreds of small islands, each with a unique web of life, which developed its obscure and subtle interrelationships through the centuries when these islands were relatively undisturbed. During World War II and the postwar period the sudden impact of the outside world seriously disturbed the insular ecological balances. Destructive faunal, floral, bacteriological, and chemical agents were introduced; irreplaceable topsoil was bulldozed off and dumped into the sea to make room for military installations; ground-water levels have been dangerously lowered; and coconut, mango, and other fruit trees upon which the natives depended for subsistence, as well as many game birds and animals, have been killed. In addition, resources indispensable to the native economy—such as garden land, fisheries, fresh-water sources, and phosphate deposits—have been withdrawn indefinitely from native use.

On Guam alone, for example, almost one third of the island acreage (which totals only about 225 square miles), including much of the best farm land, has been requisitioned from native ownership since 1941. Inasmuch as another third of the island had already been appropriated by the United States government, this means that today only about one third of the island remains in native hands, although land traditionally has been and still is the basic and most valued natural resource.

These disturbances are the more devastating since, compared with continental areas, the life webs of island communities are highly vulnerable. In oceanic islands ecological organizations consist of unusually small populations confined to limited areas, and they are delicately balanced, highly endemic, easily disturbed, and susceptible to quick extermination. Indeed, the loss or addition of a single species may seriously jeopardize the whole organization. For example, the introduction by the Japanese of a giant snail as a table delicacy upset the balance on several islands.

Since the island life webs evolved in the absence of large land animals, they have little or no resistance to the ravages of cattle, sheep, goats, and deer. On the island of Tinian, for instance, a species of deer brought in by the Spaniards soon ran wild, increased manyfold and destroyed much of the native vegetation. The introduction of cattle and deer, which the Chamorros hunted by the brush-burning method, is largely responsible for the fact that about one third of Guam, the south-central region, is heavily eroded. Most of this area is now not only unfit for agriculture, but also is poor grazing range.

This brings us to an important point. In each of the examples cited (and in hundreds of others that might be mentioned), it is man who has been responsible, either directly or indirectly, for the change in island biotics. In remote, seabound areas it is inescapably clear that all of life is involved in the ecology of the island. Hence, if we are to understand the total conservation problem in Micronesia, human societies may not be left out of the picture any more than any other relevant factor, such as phosphates or fishing, may be omitted.

Whereas plant and animal species tend to adjust to the island web of life by obscure processes of symbiosis and accommodation, man's role is more active. Indeed, it is clear that in such situations man's part has been not only active but also irresponsible and destructive. It is important to note, however, that in each case mentioned we are witnessing the aftereffects of relatively recent activities on the part of intrusive, nonindigenous peoples. When we examine the evidence regarding the traditional role of the indigenous populations in island ecological processes, we get quite a different picture.

Manifestly, the only practical and economical approach to a coordinated, long-range conservation program in Micronesia is the enlisting of native initiative and effort. The natives are not interested in the preservation of "living museums" or "laboratories for the study of evolution." They are interested in rehabilitating themselves and their islands from the ravages and consequences of total war. If there is to be an effective conservation program, it is primarily the natives who will have to organize and carry it out, for on many of the islands all the residents are natives, the only contact with the administering authority being short official visits by naval administrators, often months apart.

If the Pacific Science Board and the Island Government do not meet this issue, they will be missing a golden opportunity to lay the firm foundations for a model trusteeship administration in Micronesia, one that is practical and economical as well. On account of the vastness and remoteness of the region, and its historical and cultural diversity, Micronesia presents one of the most difficult problems in dependency administration in the world. Without sound long-range goals, native-oriented and native-implemented, and without the help of technicians experienced in the methods of democratic, integrative leadership, we may pour tens of millions of dollars into the area with little positive result either in terms of improved native welfare or of conservation of resources. But a scientifically based and coordinated conservation program, organized and developed by the native groups themselves and integrated with their own attitudes, mores, and institutions could, with wise, democratic guidance, effectively supplement the curative processes of nature and foster the re-emergence of balanced, healthy island communities. The administrative organization needed for this island trusteeship task—an organization scientifically informed and motivated and democratically oriented—will be difficult to attain under whatever auspices. Under the present naval government or any other setup dominated by the military, it cannot be attained. The facts suggest, therefore, that from the viewpoint of island health and welfare and administrative efficiency, the announced policy of President Truman to transfer jurisdiction of the Pacific islands to a civilian agency under carefully framed organic acts urgently needs to be implemented.

THE EXPERIMENTAL METHOD IN THE STUDY OF HUMAN RELATIONS

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IN 1916 I wrote an article on this subject which was published in THE SCIENTIFIC MONTHLY of February–March 1917. It may be of interest, after thirty years, to review briefly the development of experimental designs applied to the study of human relations.

The experimental study of human relations in the free community can hardly be reduced to laboratory conditions of control. Hence the present article will exclude all experimental studies conducted in an artificial or classroom situation and confine attention to six illustrative studies that were made in the free community situation. This limitation has the advantages of distinguishing sharply, first, the fundamental principle that controlled observation relies on the matching of measurements rather than on physical manipulation; and, second, the kinds of practical obstacles encountered in all attempts at controlled observation in the free community situation. The six studies to be described are all published and show how systematic efforts were made to control variable factors by matching on measurements between an experimental group that received some social program and a control group denied this program.

In the interest of clear thinking about this problem it is helpful to distinguish, first, the trial-and-error "experiments" of social legislation as a means to achieve some desired end (public housing as a means to improve the adjustment of low-income families); second, the operations of natural social forces that produce an effect; and, third, the use of experimental designs as a method of study of the first two, in order to determine the degree of success in the attainment of a desired social end, or to measure the effect of some social force. Since a social reform program is a social force that operates according to some plan of action, our description will be confined to illustrations of this, the first of the foregoing distinctions, rather than illustrations of the use of experimental designs in the study of unplanned consequences of combinations of independently planned social actions, which is the second type distinguished above. This second type would take us too far afield, since it concerns the phenomena of conflict among the means-ends scheme of special interest groups, a resulting composition of social forces, and the emergence of unplanned consequences which plague the authors and the leaders of the component but independently planned programs. Business cycles, inflation, mass movements, etc. are examples of this second type. Such mass behavior can hardly be studied experimentally until we have solved some of the problems connected with experimental study of their simpler component means-ends relations. Hence the limitation noted above.

Five methods of observation and analysis are essential in studies of human relations by experimental designs: social measurements by use of standardized psychometric or sociometric scales; random samples or a stratified random sample; control of observations by matching on social measurements; use of the null hypothesis; and qualified application of the principle of probability.

Leaders of social reform have often found that the ends they desired to achieve by means of legislation are seldom fully attained, usually because there are unexpected results which flow from the unplanned combinations of other independently planned social actions. In the free community situation of a democratic social order, it is customary to protect the rights of minority groups (political, religious, and social) in freedom of assemblage, freedom of speech, freedom of religious worship, etc. The consequences of these democratic freedoms are that many special-interest groups in society set up their own means-ends schema and work at cross-purposes to other minorities to achieve their contradictory goals. These phenomena take the familiar forms of social tensions, group conflicts, achievement of one at the expense of the frustration of the other, etc., all of which appear to the foreign observer as evidences of internal confusion and mislead such observers about

the existence of a deep current of agreement on essentials. This web of human relations fluctuates within certain limits of social elasticity, now seemingly stretched to the breaking point, and then contracting again to a more stable social structure. How can one ever untangle the separate strands of this complex social fabric? Although adequate answers to this question await the study by experimental designs, or by other methods of analysis, of the involved chain of causes and effects now hidden in the unplanned consequences that arise out of combinations of independently planned social actions, it can be shown that a real beginning has been made at a simpler level in the study of particular cause-and-effect sequences which are called the means-ends schema of social reform. The beginning is real because it is based upon concrete studies of social facts and not on armchair social philosophy; it is promising because the method of experimental design has now been used with moderate success in tests of such problems as direct relief versus work relief, public housing, and juvenile delinquency; it is objective because the method is susceptible of repetition by equally competent observers who use quantitative descriptions of human relations.

Despite some modest development in experimental designs, my present concern is with the still-unsolved problems of the method, so that the difficulties to be overcome may not be lost sight of in an account of some undoubted achievements.

There are three general patterns of experimental design in the study of human relations in the free community situation: first, a cross-sectional design in which comparison is made for a given date between an experimental group which receives a social program, and a matched control group denied this program; second, a projected design in which before and after measurements are made upon an experimental group which received a program over an interval of time, and a matched control group denied this program; and, third, what may be called the ex post facto design, in which a present situation is taken as an effect of some assumed and previously operating causal complex of factors, and, depending upon the adequacy of accessible records, an experimental group and a matched control group are traced back to an earlier date when the force to be measured began functioning upon the experimental group but not upon the control group.

Cross-sectional experimental design. Nathan Mandel used this method in a study of the Boy Scout program and the measured personal adjustment of boys. It is at once evident that some measures of personal adjustment that are reliable and valid will be required, and also some measures of other factors to be controlled, in order that the real association between the Scout program (the means, or causal variable) and the personal adjustment (the end, or effect variable) may be described. The three measures of adjustment used were the Bell *Adjustment Inventory,* Rundquist and Sletto's *Morale* and *General Adjustment* scales (all measures of how the individual feels), and the Chapin *Social Participation Scale* (a measure of overt activity in organized groups).

Choosing every tenth case in the file of 2,050 Boy Scouts in the 1934 "drop-out" file of the Minneapolis area yielded a total of 205 boys. Only 102 of these were found and interviewed in 1938. Since 103 had moved away, could not be located, would not cooperate, or were deceased, the residual sample ceased to be random. The remaining 102 cases were divided into two groups, an experimental group whose training had lasted 4 years on the average, and a control group with an average tenure of 1.3 years. When these were matched on four available traits in the record (birthplace, urban or rural, father's occupation, health rating, and school age-grade ratio), there was a further shrinkage of 22 cases, so that the final totals were 40 in each group.

It was found that the 4-year Scouts were slightly better adjusted on both the Bell and the Rundquist-Sletto scales than the control group of drop-outs. Interpretation of these results as indicative of mere conformity and conventionality rather than of somewhat better integration of personality would then have to explain why it was that the drop-outs showed higher scores on the Chapin *Social Participation Scale* than did the 4-year Scouts, since a higher social participation score is evidence against the rejection from membership of the drop-outs by the organized groups in which they were active. All in all, this study was somewhat inconclusive, since the advantage evidenced by higher adjustment scores of the 4-year Scouts was too slight to provide local proof of the hypothesis upon which the Scout program is based; but the results were at least consistent with the expectations implicit in the Boy Scout program.

During the great depression of the 1930s it became evident that material relief alone was a mere stop-gap, and that self-respecting assistance to the unemployed should take the form of a work-relief program. Hence the Works Progress Administration was undertaken. It was claimed that the WPA would develop morale and maintain it

more effectively than a direct-relief program of material assistance.

To test the validity of this claim an experimental design study was made in St. Paul, Minnesota, in 1939. A total of 465 cases were on direct relief in March of that year, and 8,074 persons were on WPA. A 5 percent random sample of these WPA cases, or 412 persons, was taken as an experimental group to be compared with the 465 relief clients. To make these groups sufficiently homogeneous for valid comparison, seven conditions were set: living in St. Paul; working in Ramsey County; not previously on WPA; and not single, widowed, separated, or divorced. Meeting these conditions reduced the WPA experimental group to 324 cases, and the control group of relief clients to 198. These 522 cases were interviewed in April–May and the total still further reduced by 320 cases because of refusals, could not be located, had moved away, deceased, sickness, gainfully employed, changed status, etc. As a result there remained 130 cases in the experimental group of WPA and only 72 direct-relief clients. These two groups were then matched on seven factors: age, sex, race, nativity, years of formal education, usual occupation, and size of family. Had this matching on traits not been done the obtained variations in measured morale and adjustment might have been due to one or more of these factors, rather than associated with being on WPA or on direct relief, the association we set out to measure. Again, both groups lost cases because unable to match, so that the terminal groups consisted of an experimental group of 80 WPA matched on seven factors against a control group of 42 on direct relief.

During the interviews every individual was measured for his morale and his general adjustment score on the Rundquist-Sletto scales, and for social participation and social status on the Chapin scales. On each of these scales the WPA group showed better average measures of adjustment than did the relief group. Differences between these average group scores were, however, not statistically significant, so that chance could account for the differences. But the differences were in the expected direction, and a measure of all four in combination as a pattern of response to differential treatment by the two contrasting programs showed that these differences in pattern could occur by chance somewhere between 1 in 10 and 1 in 50. Again, our experimental design study failed to yield positive proof of a hypothesis of high association, although it did yield results not contrary to this hypothesis.

To summarize the most significant findings from these two examples of cross-sectional experimental design, it may be noted:

1. That no test of cause and effect or of concomitant variation is possible by this method, the only evidence obtained is that of an *association* at a given date between two factors (different programs of assistance on the one hand, and measured adjustment on the other), but this association was in the expected direction, if it is assumed that WPA is superior to direct relief in attaining better adjustment.
2. The enormous losses of cases owing to selective conditions in the natural community, plus losses from matching, destroyed any randomness of the original samples, and increased the magnitude of the standard errors, thus reducing the statistical significance of any differences obtained.
3. Since the residual groups departed from randomness, no generalization can be made from such studies to any larger universe or universes from which the groups were taken, although it remains true that conclusions for the limited groups studied do have a valid basis in fact.

Projected experimental design. Unless social cause and effect can be subsumed in the area of human relations, there is little hope of a rational explanation of the vexing problems of the social order. We have just shown that the cross-sectional experimental design, because it is limited to a controlled comparison of two groups at a given time, fails to disclose cause-and-effect relations, although it may be suggestive of hypotheses for study by more elaborate methods. Does the projected experimental design offer a more hopeful promise of discovery of social cause and effect? Two published studies will now be analyzed in an effort to answer this question.

The first study was an attempt to measure the effects of a public low-cost housing project upon the social adjustment of slum families in Minneapolis from 1939 to 1940. One year is admittedly too short an interval for any real test, but practical considerations set this time limit, and so we are obliged to make the best of it.

This study began in the spring of 1939 when 108 former slum families having low incomes had taken up residence in Sumner Field Homes, a PWA housing project planned in 1935. The control group consisted of 131 other families residing in the same slum dwelling during the period. Although these 131 families were initially so much like those eligible and admitted to residence in the project as to be borderline or deferred cases who might be admitted later, an attempt was made to make the experimental group of residents and the control group of slum dwellers still more alike by matching on ten control factors: race or cultural class of husband, of wife; occupational class of

husband, of wife; employment status of husband, of wife; number of persons in the family; income of the family; and years of formal education of wife. As in the studies previously reported, many cases were lost by inability to match. In all, 59 cases dropped out for this reason—47 from the experimental group and 12 from the control group. Because of the lapse of time between initial and terminal measurements, various events intruded and 50 more cases dropped out for these reasons—12 from the experimental group, and 38 from the control group. Forty-eight more cases were lost before the initial interviews were completed, owing to mobility, refusals, and other reasons, of which 5 were from the experimental group and 43 from the control. Thus the final matched groups (frequency distributions equated) consisted of 82 persons, 44 in the experimental group and 38 in the control group.

This test of low-rent public housing as a means to achieve the end of improved adjustment of slum families, or as a measure of a program of social reform as a cause, to produce improved adjustment as the effect, as the case may be regarded, relied upon changes in measured social adjustment before (1939) and after (1940) the program had operated for one year, as evidence of proof. The measures of social adjustment used were the same as those applied in the WPA–relief study described above. Since no changes in measured morale or in general adjustment were found statistically significant in a comparison of average scores of each group before and after, and also because each family had resided in the *same* dwelling unit for the period, there were in fact thus added three additional controls to the ten noted above, making in all, control on thirteen factors.

When, however, attention was directed to the changes that occurred in social activities (social participation scores), in the percentage use-crowded, and in the condition of the living room of each home (measured by the Chapin social status scale), it was found that a pattern of response consisting of these three factors in combination showed a difference between change in the experimental group and change in the control group that was of high statistical significance. Whereas the pattern of change on these three factors in the control group was not statistically significant (multiple critical ratio, 1.82), the corresponding change for the experimental group was highly significant (multiple critical ratio, 6.01); and the difference between the changes of the two groups on this measure was also highly significant (multiple critical ratio, 4.97). Such high critical ratios as these are so unlikely to occur in chance that a cause-and-effect relation may be inferred. In a certain sense this is a gratuitous conclusion because the public-housing program was intended to interfere with chance and to create for the project residents improved housing, so that it might be more surprising if it did not effect improved adjustment. This desired effect cannot be established by mere wishful thinking, however, but only by the evidence of measurements on scales that had been standardized to measure adjustment prior to the existence of the change they were used to test. Furthermore, although science often merely confirms the results of practices based on experience, it is by no means certain that a scientific test will not disprove the effectiveness of popular practices; hence the justification for the present study.

The real question that faces us in making inferences from the facts obtained by this experimental design is: Did housing per se cause these changes in pattern of response? Only a positive answer to this question, phrased in terms of a low probability of chance as an explanation, would prove the effectiveness of the public-housing program in this example, and then only if all unknown factors had been controlled. Although thirteen factors were controlled in rough degree, the remaining known but unmeasured factors (health, for example), and an undetermined number of unknown factors, were not controlled. Thus the results of this test by experimental design offer no final conclusion. Furthermore, since the groups compared were nonrandom samples, no reliance upon probability tests is permissible as a basis of generalization to any larger universe of similar "experiments" by public-housing authorities.

Where does this leave us? On the negative side we may offer the opinion that the results are sufficiently suggestive to urge repetition of this type of study on similar cases using like methods of research in the hope that replication will yield corroboration of the results. For only by replication in numerous similar studies may we escape from the dilemma of whether the obtained significant differences were due to the nonrandomness of the samples, or to the fact that they were drawn from different universes (i.e., universes that were made different by virtue of the public-housing program, the objective that was to be tested by the experimental design). Should the same results be found on many trials, then generalization from even nonrandom samples to a universe might be valid and justified. On the positive side we may say that the differences found were of an absolute magnitude which would be regarded as highly significant if

they were found between two random samples.

Of the five principles enumerated as essential to study by experimental designs, three have been illustrated: social measurements by use of psychometric or sociometric scales; control by matching on measurements rather than by physical manipulation (the conventional and mistaken idea about control of variable factors); and qualified application of the principle of probability. But the housing study illustrates also the advantages to the research student that follow from the use of a null hypothesis in sociological research wherein the purpose is to test the results of a social reform program.

The conventional working hypothesis is a positive assertion: "Public housing improves the social adjustment of individuals and families living in a slum." There are at least two difficulties to be overcome in efforts to test such a hypothesis: first, the statement uses the normative term "improves," which implies the question "What is improvement?" Whose standards of what is improvement are to be taken? Shall we rely on the judgment of the housing manager, or on the opinions of those who promoted the project, or on the critics of all public housing? Obviously, any such definitions are open to subjective considerations which stem from different desires. This problem is simplified when standardized sociometric scales are used, since there was incorporated into the initial construction and testing of such scales elements of objectivity not present in individual opinions. Moreover, the norms of such scales were discovered in previous studies, so that the application of these scales in any present study is not affected by any desire to vindicate or to disprove the program being investigated. A second difficulty is more serious; it consists in the fact that "improvement" is an open-ended concept. How much change in the desired direction is improvement? When the positive hypothesis is replaced by a null hypothesis, this difficulty is avoided.

Three null hypotheses susceptible of proof or disproof by facts may be set up as follows:

1. When measures of adjustment are made upon an experimental group which receives a social program and an experimental group denied this program during an interval, and the two groups are matched on a number of factors, there are no changes in measured adjustment during the interval. Since the facts obtained in the housing study show the existence of changes, this null hypothesis is proved false.

2. If changes in measured adjustment are found in these groups, the changes are not statistically significant. Again this null hypothesis is proved false by the results found.

3. Although changes may be found, and these changes are statistically significant, the difference between changes of the two groups is not statistically significant. This null hypothesis is also proved false by the factual evidence.

It is evident that it is a much simpler task to prove that differences do exist, that these differences are statistically significant, and that the difference in changes is statistically significant as departures from zero than it is to prove that the changes are in the nature of improvement; and this is particularly the case in studies of problems of human relations, wherein bias is hard to avoid and the subject matter of study is emotionally disturbing to the observer because it involves his personal value systems.

A second study using the projected experimental design to test the effectiveness of a social program of differential treatment is that reported by Harry Shulman in New York City. This is a study of the effects of treatment to prevent juvenile delinquency by a controlled-activity program. The program consisted of workshop and game-room activities, classes in creative art, woodwork, leather, and metalworking, which met three sessions a week for two hours a session over a period of three successive school semesters. Groups of 50 problem boys and 80 normals were mingled naturally in these activities. The problem boys included chronic truants, incorrigibles, serious personality problem cases, and some charged with arson and theft. The normals were nonproblem cases obtained by serial selection from class-roll books of children who had never dropped below a B grade on their studies. The experimental group to receive the program consisted of 65 boys, including 25 of the problem cases and 40 normals. The control group was similarly constituted but denied the program. At the beginning there were 310 boys, 155 in each group, who were ten and one-half to fourteen and one-half years of age and in Grades 4-A to 8-A from four public schools in socially substandard areas of New York City.

The measure of community adjustment used consisted of 13 of the 66 Baker-Traphagen items which measured behavior status. Comparison of the results for the problem boys of the experimental group with the problem boys of the control showed that 72 percent of the problem boys in the experimental group which received the program improved, and only 33 percent of the problem boys in the control group gained. Meanwhile 28 percent of the former had lost, and 66 percent of the latter. In tests before and after in the classroom situation, all differences among the mild-to-medium conduct-disorder cases were statistically significant on scores of the Haggerty-Wickman-Olson behavior rating scale for the two problem children groups.

Since these favorable results might have accrued from differential changes in home environment during the experimental period, a case study was made of factors of family disorganization (broken home, marital disharmony, public assistance, economic maladjustment, children's illness, mental deficiency and disease, unethical or antisocial example, etc.), of defective social relationships (between parents, parent-child, and community), and also of improper discipline. The results of this analysis showed no appreciable differences in changes for the problem children of the two contrasting groups. Thus the intrusion of environmental factors of the surrounding community had not operated to confuse the relationship between the treatment program as a cause and the diminution of behavior problems as an effect.

Although the two studies in projected experimental design just described illustrate some advance toward the scientific goal of discovery of social cause-and-effect relationships, each study stands by itself as a closed system, and no scientific generalization may be made from either to the larger universe of housing or juvenile delinquency. Again, repetition by similar studies on like subjects, using the same methods of research, is the only avenue of approach to a reliable basis of generalization.

Ex post facto experimental design. For many years students of human relations have sought a valid method to discover cause-and-effect relations which it is believed may be hidden in the records of past social events. The ex post facto experimental design was developed as one attempt to clarify this methodological problem.

A study that illustrates the use of this ex post facto method was made by Helen Christiansen under my direction in 1935–38. The positive hypothesis to be tested was the longer the period of high-school education before leaving school, the better the subsequent economic adjustment in the community. Here the formal high-school course of instruction is taken as the program variable, or the causal factor, and measured economic adjustment is taken as the effect variable. To test this hypothesis the records of 2,127 high-school students who left school nine years earlier (1926) were obtained and analyzed. In that year 1,130 had graduated from the 4-year course in all high schools of St. Paul, Minnesota, and 997 had dropped out of school, having been in 1926 at the end of their first, second, or third year of study. The graduates were taken as the experimental group and the drop-outs as the control group. The measure of economic adjustment in the community situation chosen as the dependent variable was the percentage of shifts on jobs from 1926 to 1935 which involved increase in salary, no change in salary, and decrease in salary. This is admittedly a crude measure, but the data could be obtained by interviews of all persons of the original 2,127 who could still be located in 1935.

Considering the relatively long experimental period of nine years, substantial losses of cases were to be expected. As a matter of fact, 933 individuals were lost for analysis, 459 of the graduates and 474 of the drop-outs, for reasons of death, moving away, not located, or incomplete records. After matching on six control factors, age, sex, father's occupation, parental nativity, neighborhood of residence, and average high-school marks (no I.Q. data were available in 1926), the two groups were reduced to 145 individuals each.

Analysis of economic adjustment to the community in 1935 showed a regular decline in percentage with decrease in salary or no change in salary, with each additional year of high-school education for drop-outs of 1, 2, and 3 years of study, and a regular increase in percentage with salary increases. The 4-year graduates attained the highest percentage with salary increase for 1926–35 of any group, and their percentages of cases with decline in salary or no change were approximately the same as those of the 3-year drop-outs. When the matching technique was made more precise—that is, by identical individual matching, instead of equating frequency distributions on each control factor—the two groups were reduced to 23 cases each. The trend in percentages on the salary criterion became even more pronounced, and the advantage of the graduates over the drop-outs was much greater. All in all, the positive hypothesis seems to have been established for this study, provided the results can be explained as not due to health factors uncontrolled, the factor of persistence in school work, the existence in the group of 933 lost from study of trends contrary to those found in the groups that were analyzed, or unknown factors. Again no decisive proof has been achieved; only the probability of a cause-and-effect relation between length of school attendance and subsequent economic adjustment in the community.

But this matter of probability deserves further exploration. To test the statistical significance of the percentage differences found between the experimental and control groups, two random samples of 23 cases each were chosen from the larger experimental and control groups, but the individuals in these random samples were not matched (as were the individuals in the 23-case terminal

experimental and control groups which showed the largest differentials). Analysis of the random samples showed indicia of differences in community adjustment occurring by chance as 1 in 14, as compared with 1 in 200 for the 23-case matched samples. Since the original 2,127 cases constituted the universe of all who left high school in 1926, and the remaining 1,194 cases composed the remainder after losses of 933 cases, both the random samples and the experimental groups were from a subuniverse, so that comparison of real random samples with nonrandom groups introduces in this experiment a more adequate basis for the use of probability tests than was the case in any of the other studies described in this article.

In summary, it may be said that the differentials in favor of the graduates seem too large to be explained as mere chance fluctuations, and of absolute magnitudes that cast some doubt upon the validity of the four alternative explanations, which would set up the claim that it was not the length of high-school education that caused better economic adjustment in the community of graduates. The comparison with the random samples yields results also consistent with the foregoing inference.

But, again, we must caution against generalization to all high-school programs of all areas of the United States. Perhaps the place and time studied were unique. On the other hand, it may be pointed out that the 1926–35 period included the depth of the great depression of the 1930s, so that the hypothesis was substantiated for an interval of sharp testing by unemployment, and hence that our conclusions have a safety factor that makes them conservative.

A variation in the ex post facto design was made in a study of public housing and juvenile delinquency in New Haven by Naomi Barer from records of 1944 on 317 families traced back to 1924. She made a self-comparison of a group of 649 children seven to seventeen years of age in these families, for the period 1940–44 to the period 1924–40, when the same families and their children lived elsewhere. From 1924 to 1940 the rate of juvenile delinquency per 100 children per year was 3.18; when the same subjects were residents of the housing project the rate had declined to 1.64. This is a statistically significant decline. During the years 1940–41 there was an increase of 9.1 percent in total juvenile delinquency in the city of New Haven over that of the period 1927–40, so that no general decline in juvenile delinquency occurred in the community at large, which, if it had happened, might explain the decline of the experimental group of children in the housing project. No generalization is justified from this study alone about housing and juvenile delinquency at large. Only corroboration from repetition of the experiment will furnish proof of a hypothesis that good housing operates to reduce juvenile delinquency.

Two types of critics deplore the kind of studies herein described. There are the "practical" reformers who have expressed the opinion that this kind of detailed analysis is superfluous since it is mere common sense to expect to find improvement following upon well-matured plans for social betterment. But are desire and wishful thinking the sort of support upon which to base and to justify expensive and elaborate social programs? Then there are sincere critics who express the judgment that the time and expense of detailed study by experimental designs might better have been applied to the investigation of problems wherein the expectation of significant results had a firmer basis in technical and precise research methods. No adequate answer can be made to this criticism. The reader will have to decide for himself whether exploration by these methods of experimental study, admittedly crude and lacking in precision, is worth the time and effort put forth. In any event, I have found these experimental designs interesting, and sometimes exciting, ventures. They are offered for what they may be worth, and their limitations have been stated and reiterated throughout this article.

Three closing comments may be made. First, objective evidence has been offered to show that experimental designs may be applied to the measurement of problems of human relations with some expectation of a partial clarification of social cause and effect in society. Second, the chief obstacle to the experimental method in the study of problems of human relations is removed, since it stemmed from a false analogy between "social experiments" that shove people around and laboratory experimentation that manipulates physical matter, whereas the fundamental principle involved is observation of social relations under conditions of control, which conditions are attained by matching on social measurements, and do not require interference with personal freedoms. Third, the methods of experimental design offer the promise of an objective procedure for evaluating the effects of some types of social reform programs. Such programs consist of the approved means chosen to attain some desired or valued end. This being the case, it seems possible that further extension of the methods described herein will lead to an experi-

mental test of values, phenomena hitherto circumscribed by desire and wishful thinking and suffused with emotional attitudes.

If the first expectation is realized, we may have in it the beginnings of a rational basis for the amelioration and control of some of the problems of human relations. If the third is realized, we shall have a rational substitute for mere subjective opinions and sanctimonious or self-righteous judgments about what may be done by social action to achieve collective desires. Furthermore, through the replication of experimental design studies, which attempt to measure the effectiveness of specific means-ends schema planned to attain specific goals, it may be possible to develop a systematic mosaic of nonrandom samples that will possess a degree of representativeness to compensate for lack of randomization, and thus to supply a basic representativeness upon which reliable scientific generalization may rest. Finally, if the foregoing development takes place, an approach will have been made to the solution of the most difficult methodological problem of all, to describe objectively the tortuous chain of social cause and effect that now lies hidden in the unplanned social consequences that seem to flow from the combinations of innumerable and independently planned social actions within a democratic society.

SONNET

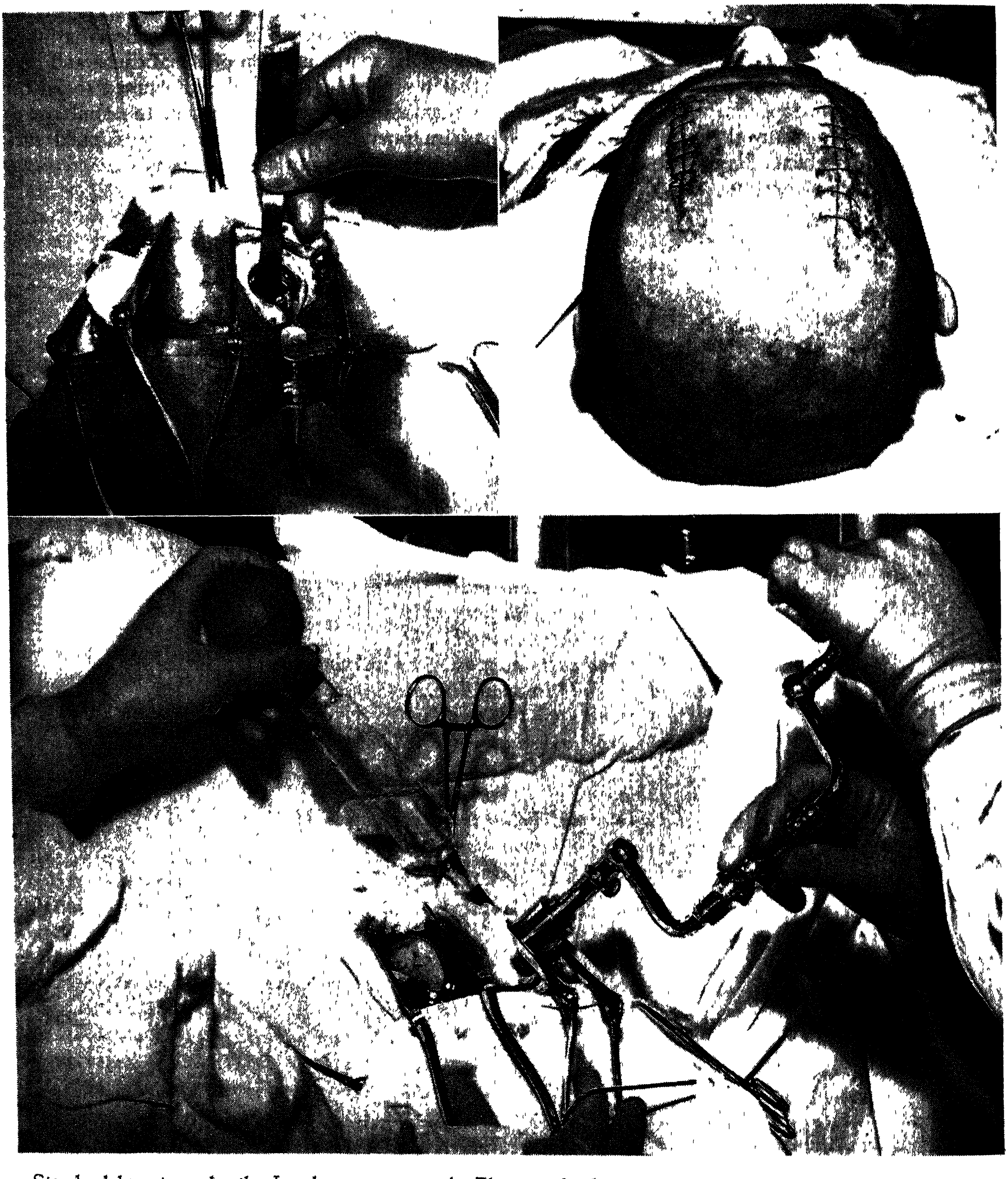
(ON THE GIANT METEOR CRATER IN ARIZONA)

Was it by chance it fell upon this place,
Or was it by design some meteor sped
Roaring, an incandescent mass from outer space,
Coloring all the sky with fiery red?
O miracle! O comet glowing bright,
Touched off and cast down by some unknown hand,
When did you fall? In what dim, awful night
Did you descend upon this arid land?
What conqueror looked up—and fell before
Your dooming blow, O conqueror mightier still?
Jarring the atoms of the earth's deep core,
Why did you come? What word did you fulfill?
Not even when, within your tomb self-sealed,
Man finds you, shall these riddles lie revealed.

NOEL RODERICK PEATTIE

SCIENCE ON THE MARCH

PSYCHOSURGERY—NEW HELP FOR THE MENTALLY ILL



Standard leucotomy by the Lyerly open approach. Photographs by courtesy of Dr. William Beecher Scoville, Hartford, Connecticut.

IN Lisbon, in 1935, a professor of psychiatry by the name of Egas Moniz, together with a neurosurgeon, Almeida Lima, opened up a spectacular new field in the treatment of mental diseases. They operated on the brain of a mental patient.

Brain surgery in itself was not entirely new. By that time brain abscesses and tumors were already being removed successfully, but Moniz and Lima's venture was unprecedented in that they operated on apparently healthy brain tissue in the frontal lobes. Out of their efforts have come the brain operations known today as *prefrontal lobotomy, topectomy, transorbital lobotomy, lobectomy, thalamotomy,* and so on, each aimed at alleviating various types of mental disturbance. These operations have already brought relief to a large number of patients who otherwise would have been doomed to a lifetime of the hopelessness and suffering that have been contained for centuries in chronic mental illnesses.

Because apparently healthy brain tissue is sacrificed in the process, the prefrontal lobotomy and related surgical techniques have been slow of acceptance in many quarters. With the accumulation of experience and the refinement of techniques, however, brain surgery (or *psychosurgery,* as this particular aspect of surgery is called) now has a considerable body of evidence in its favor. Last August 3–7, more than 200 neurosurgeons, neurologists, and psychiatrists, representing twenty-seven nations, gathered in Lisbon for the First International Congress on Psychosurgery, which was held in honor of Dr. Moniz, and was organized primarily by Dr. Walter Freeman, American pioneer in the field. Reports were made on 5,000 cases of psychosurgery—a world-wide scrutiny of the original operations and the techniques that have been evolved since then.

The present-day surgical intervention in the frontal lobe region is based on the prevailing concept of the function of the frontal lobes, of which there are two, and which are very much more prominent in man than in other species. Of all parts of the brain, and probably of the whole body, the frontal lobes have been the most difficult to understand. The frontal lobes have resisted scientific probing, although nearly every other region of the brain had its functions fairly well delineated.

There are the areas of the brain that govern speech. This we know through laboratory experiments, and because experience has shown injury to those areas to render a person speechless, although no harm had come to his speech organs. Certain areas have been shown to govern sight; others, hearing, writing, vision, motor ability, and so on. The frontal lobe areas, long the greatest mystery, and studied by anatomists, neurologists, psychiatrists, physiologists, and psychologists, have now been marked, largely on an empirical basis, as the areas of association or integration, governing such high mental functions as judgment, imagination, and initiative.

In the beginning, Moniz theorized from the mounting indications that great cerebral activity is centered in the frontal lobes. Convinced that certain neuropsychiatric conditions reflect a morbid fixation of various frontal association pathways, he reasoned that surgical interruption of cell connections in these areas might relieve the disturbing condition. Second, he reasoned that the interruption would have to be bilateral, since in other dual organs of the body, the activity of one can be taken over by the other in the event of loss.

As so often happens with speculations that lead to important practical advances in medicine, the original theory upon which Moniz based his approach has not found wide confirmation. A great deal of progress has since been made in broadening and increasing the knowledge of frontal lobe functions, but today, with more understanding, scientists are actually less set in their ideas of the frontal lobes than they were ten years ago. The bilateral emphasis by Moniz was in effect more important than his concept of function; it has been demonstrated that the loss of only one frontal lobe has no significant effect on personality performance.

Many observers are now inclined to ascribe the benefits derived from frontal lobe surgery to a reduction in the emotional charge attached to abnormal ideas. They believe the success of the operation lies in the interruption of the pathways between the frontal lobes and the hypothetical center of emotion located elsewhere in the brain, presumably in the thalamus. With the reduction of the emotional energy from the thalamus, delusions and hallucinations tend to lose their importance, bizarre mannerisms and motor manifestations to recede, and preoccupation with physical complaints to fade into the background.

Thus, although the fundamental disease process seems not to be directly altered by psychosurgical procedure, the patient is relieved of abnormal emotion attached to morbid ideas, and his attention can be diverted to more realistic concepts and more constructive activities. In the opinion of most authorities, some type of unpleasant and disturbing emotion in the reaction pattern of the patient is an important criterion for selection of patients.

The technique originated by Moniz, and since improved by him and others, has come to be called, in this country, the prefrontal lobotomy. There is still a long way to go in refining operative procedures, and in improving methods of selecting patients, but the field has advanced far beyond the point, in the early days, when patients were chosen on the basis of what might be called negative selectivity—the operation performed as a last resort when the patient was so far gone that he had nothing to lose anyway. Certainly, if too much of the personality has deteriorated in the course of the disease process, little can be expected from psychosurgery; it is not a restorative.

Today most observers see the best outlook for prefrontal lobotomy in long-standing depressive illnesses, particularly the involutional type, and in incapacitating obsessive-compulsive neuroses. Also, certain schizophrenic patients, especially the catatonic subgroup, have benefited from the operation. Contraindications for lobotomy are present when the emotional tone has become chronically flattened (the operation would only "flatten" it all the more); and the advisability of operation is also questionable in those cases where antisocial traits were evident in the previous personality.

The surgical technique employed varies with different practitioners. Moniz adapted an instrument called the leucotome (whence his use of the word "leucotomy" for the operation) with which cores could be cut in the white matter of the brain and left *in situ.*

Freeman and Watts, who first introduced the operation into this country, have used a blunt knife to section the white matter at the posterior end of the prefrontal lobes, making their incision through a trephine opening in the skull in such a way as to sever the main portion of fibers between the thalamus and the frontal lobes.

Following the original lobotomy, a number of new techniques have been developed in the effort to learn more about what is actually cut in the brain, and to control more precisely the site of the incisions and the amount of tissue sacrificed, the latter logically being the minimum necessary to produce the desired clinical result.

After the original "closed" technique of lobotomy was introduced, Lyerly, an American, developed the "open" technique, in which the skull is opened by turning down a bone flap. Many of the larger clinics in this country are now using the open technique.

The topectomy was originated by Pool, who felt that to obtain beneficial results in psychotic patients, it might not be necessary to disconnect the frontal lobes as extensively as in the "standard" prefrontal lobotomy. The topectomy is also a bilateral procedure, but instead of the cores being left *in situ,* certain areas of the cortex in the frontal lobes are actually removed. So far, the therapeutic effects of topectomy are similar to those of lobotomy —that is, relief from emotional pressure, tension, and anxiety. Further, there seems to be a minimum impairment of social, moral, and intellectual performance.

Among other techniques being advanced, Freeman is pioneering in what is called transorbital lobotomy, a less drastic method that he recommends for early cases of mental illness; Wycis, Freed, and Spiegel have developed the thalamotomy, which involves partial electrical destruction of the main connecting station from the prefrontal lobes; and Scoville is doing preliminary work in selective cortical undercutting, in which certain areas of the cortex are separated, but the cortex remains in place, a technique that is based on experimental work in Fulton's laboratory at Yale. Also, a group in England is undertaking to outline areas for neurosurgical attack according to symptomatology.

All this investigation is in search of techniques to reduce time of treatment and hospitalization, at the same time achieving maximum relief from mental suffering, with minimum social and personality disturbance.

After prefrontal lobotomy, a change in the personality of the patient is evident, even on the operating table itself when the operation is preformed under local anesthesia. The voice shows a change in emphasis and inflection, and the patient appears oblivious of the emotional troubles that had been harassing him so severely a short time before. At this time, immediately after the operation, when there is a reduction in emotional tone, when the behavioral display is superficial and improper habits are not yet ingrained, it is essential to bring into play the right kind of influences in a favorable interpersonal and material environment. Prompt rechannelization of the haphazard reduced emotional energy at the disposal of the personality after the operation determines to a great degree the eventual social adjustment of the patient. So it is that psychosurgery cannot stand alone; it must be applied in close correlation with other psychiatric treatments, including, especially, reeducational therapy.

Postoperative patients may present a special problem. Because their new personality components are carelessly assembled during the period immediately following operation, they shc

be exposed to old psychotic patterns, nor is it desirable that they mingle immediately with convalescent patients. They should be assigned to special postoperative groups, and participate in a program of activities shaped by life outside the institution and especially designed to provide the stimulation, encouragement, and re-education necessary at this time.

Although some psychiatrists recommend early removal of the patient from the hospital back into his home environment, others, including the writer, are convinced that a premature return to the family, to the environment in which the illness originated, at a time when the patient presents so many behavioral and nursing problems, may easily compromise chances for an optimal result. The well-controlled hospital environment, with especially trained personnel, is best suited to the task of retraining.

After a number of years of careful investigation and work in psychosurgery and postoperative re-education, the Institute of Living has found it advisable to construct a special psychosurgery unit where operating facilities, postoperative training facilities, and residential quarters are all housed under one roof. Within seventy-two hours after the operation, and sometimes the following day, the patients are up and engaged in activities. This postoperative retraining program, like other work in this field, is still in its pioneer stages, so modifications and refinements must be constantly envisaged. Current results are nevertheless highly encouraging.

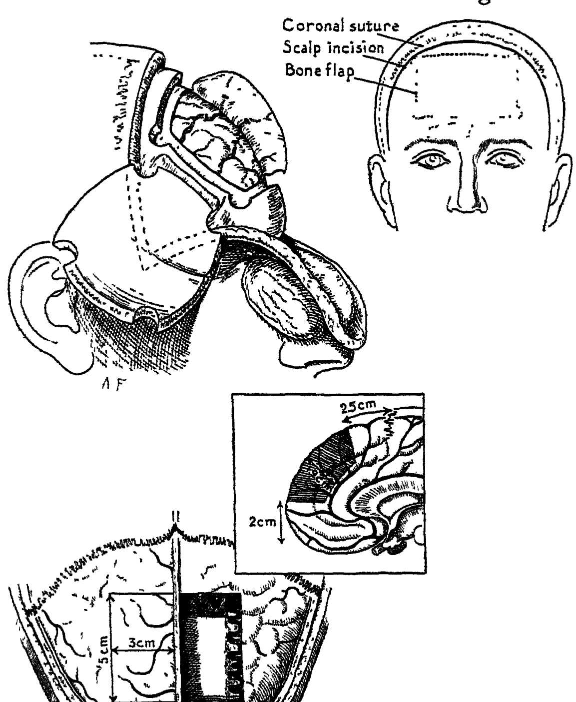
Broadly speaking, the program embodies adequate encouragement and personal attention, stimulation and motivation, and consistent social pressure. The postlobotomy patient, even more urgently and conspicuously than any other psychiatric patient, needs a consistently active routine throughout the day, and it is of paramount importance that the re-educational program for this group be geared at all times to the characteristics and capacities of the patient, with provision for gradual expansion from relatively simple individual activities to increasingly complex projects and group activities.

The post-lobotomy patient tends to think in concrete terms. Most studies indicate that intelligence is not measurably impaired by the operation; the difficulty lies more in a reduced capacity for prolonged attention, a lack of initiative, and inferior planning ability, all of which have been rapidly improved with proper training.

In the early postoperative stages, the patient is extremely suggestible and susceptible to immediate external stimuli, but his span of attention is short. He must be reminded to continue the task which confronts him, even where meals are concerned. His foresight and his conception of time suffer, and he may idle away the day if permitted to do so. Once he gets busy, though, he may do very well at a variety of more or less complicated tasks. To accomplish this, most patients need prodding and will take it in their stride. Guidance must be firm and consistent. The patient must be kept occupied in constructive individual activities or in group situations favorable to the development of initiative and socialization.

The re-educational program extends throughout the day, with individual and group activities variegated and flexible. Great emphasis on grooming is important in the early postoperative period: the men have regular barber appointments, and weekly appointments are made for the women with a beautician for hair-styling and instruction in the art of facial make-up.

With improvement in individual performance, the patient is encouraged to participate in group activities and to assume responsibility for such participation. For women there are special classes in home economics and in other activities germane



Operative approach for new topectomy operation (Cortical Ablation of Superior Convexity of Prefrontal Lobes). Drawings by courtesy of Dr. J. Lawrence Pool, New York.

to their domestic pursuits. Men have special activities in the field of physical education and sports. A coed social hour is provided daily in the form of informal dancing and games, and coed classes are given in calisthenics, rhythm games, and group singing. Each patient frequently has a two-hour motor ride to get him in touch with the community, and to vitiate any institutionalized pattern that may have eventuated from his past hospitalization.

In favorable weather, picnics are arranged for those patients who will benefit from them. By appointment, patients may purchase articles in the campus shop, where they calculate prices and count out their money for the transaction. Their interest in reading is encouraged at the campus library, where current periodicals and best sellers, fiction and nonfiction, are featured. Classes in vocational subjects, avocational pursuits, and physical education stimulate interest in personal performance.

Activities, as cross-sectioned above, complement the psychotherapeutic work carried on by the psychiatrist with the individual patient, and thus are directed primarily toward the goals emphasized by the Institute for all patients in the re-educational phase of its psychiatric program—goals of vocational orientation, avocational outlets, satisfactory social and recreational relationships, and good physical educational habits.

Within the psychosurgery unit, groups are set up on the basis of general behavior and ability to cooperate with others, and the individual patients are promoted as they reach a higher level of achievement. The next step for the patient is to take his place in the regular convalescent group in the hospital. Finally, with the abatement of symptoms of the original illness, the control of the psychosurgical phenomena, and the achievement of a sense of responsibility consistent with everyday life outside the hospital, arrangements are made with the family for taking the patient home and getting him to work immediately in a job suitable to his resources.

The training period may be as short as three months; for those who have been seriously ill before the operation, it may be nine or twelve months. During that time, behavioral improvement usually accrues in consistent fashion, though in some cases considerable fluctuations occur for an extended period. Some patients make significant progress for a year, and others continue to improve for several years.

In the tabulation of results from psychosurgery, it is seen that of a group of patients who had been previously regarded as hopeless and destined to spend their lives in a mental hospital, between 30–50 percent have been re-established outside the hospital on a self-sustaining basis; a percentage of the remainder have been established outside the hospital on a semi-independent basis; and, with a few exceptions, the rest have been materially improved over what would have been their destiny without the operation. It is evident that in skilled hands, the danger to life and of aggravating conditions is negligible.

As with all new treatments that prove at all successful, psychosurgery brings with it the danger of overenthusiasm for its effectiveness. Unsettling to unsophisticated doctors and laymen uninitiated in the field are the near miracles, in which psychosurgery has helped to relieve patients who have been desperately ill for long periods of time and who have not benefited materially from other forms of treatment; but the pioneers in the field, who hope perhaps the most conscientiously for its bright future, are the ones first to advocate the prudent viewpoint toward this powerful tool that has been placed in the hands of the profession. Occupying a somewhat spectacular place in psychiatry, psychosurgery is now advancing through increasingly precise criteria of selectivity, more refined operative procedures, and improving techniques of retraining and rehabilitation, all of which have seen progress in the past year.

These problems are receiving the attention of some of the most astute, most brilliant scientists in the world. In 1935 Dr. Egas Moniz cast a ray of light that is today brightening the hopes of the mentally ill, who are still the greatest public-health problem facing the country. Through the studied development and careful application of psychosurgery in conjunction with psychotherapy, tomorrow promises to dawn on an ever-brighter future in the treatment of mental diseases.

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BOOK REVIEWS

THE GIFT OF TONGUES

The Miraculous Birth of Language. Richard A. Wilson. 256 pp. $3.75. Philosophical Library. New York.

BEFORE being certified as educated or eligible for the franchise or for any scientific, religious, legal, or civil employment," says Bernard Shaw in his preface to *The Miraculous Birth of Language,* "I should like everyone to be examined in this book."

Such an ardent endorsement is perhaps prompted particularly by Professor Wilson's Foreword, emphasizing an anti-Darwinian organic hypothesis which assumes that purposive mind is a basic and permanent element in the world and thereby expostulating what we already know to be a pet viewpoint of Shaw's.

Actually this organic hypothesis, which Wilson finds more synthesizing than others, has little to do with his treatment of language. It would therefore be unfortunate if his acceptance of such a view prejudiced consideration of his other ideas, ideas set forth by himself as follows:

> I have treated language as one step or cycle in the general evolution of the world: the emergence of conscious mind in the world and the new problem that emerged with it; the birth of language in answer to this problem; the materials from which language was made; the metamorphoses it underwent in reaching its final form; its structure in relation to space and time; and its unique character among other phenomena of the world.

Professor Wilson approaches his objective by a twofold method. First, he critically evaluates various theories concerning the origin of language, beginning with the Hebrew and continuing through those of Herder, Kant and Darwin, giving a rather detailed summary of the last and presenting arguments for its inadequacy. Second, he offers his own view emphasizing the emergence of consciousness in human beings which freed man intellectually, via the process of language, from space and time and set him apart from other animals.

An animal below man—the dog, for example—has no sense of time prior to his own birth or subsequent to his own death; therefore, he has no consciousness of any other dog's life at any other time. The same applies to space, which for the dog consists merely of local space. Man, on the other hand, has no difficulty visualizing limitless stretches of space and time beyond the range of his senses. This ability is due to the difference between the mind of man and that of other animals, a difference reflected in man's speech—in fact, creating speech. As Wilson puts it,

> If the animal does not conventionalize sound so as to differentiate one sound explicitly from another, and to multiply their number as man has done, it would seem to follow that he does not *in his mind* differentiate one object from another in space, or one event from another in time. Man's power of explicit mental differentiation was what brought human language into existence.

Ability to differentiate prompted man to fashion a tool permitting the mental representation of the things differentiated. Thus, through the definite shaping of natural sounds, the conventionalization of their meaning, and their multiplication as needed, man has elaborated a language structure by means of which he can translate the "actualized space-time world of nature into the new-born space-time world of mind."

The analysis of this process for both oral and written language provides not only stimulating reading but also a better understanding of the tool used by so many and understood by so few. For the scientist, especially, it is well to consider that language—written language—is less susceptible to the never-ceasing detrition of time than any other product he may fashion. Science, as the refined and accumulating awareness of the universe, must have written language as a bulwark against the evanescence of time.

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OUR IMPERILED RESOURCES

Road to Survival. William Vogt. xvi + 355 pp. $4.00. Sloane Associates. New York.

LEARNING on almost every front has been a war as much against the unknowns as against the trees obstructing our view of the forest. In medicine, we have learned and moved from the diseased organ, to the disease, to the patient; eventually we must move to the patient in his total environment. In education we have moved from compartmentalism, to the individual disciplines, to a synthesis of disciplines; eventually we must weave the whole cloth of integration. In statecraft we have moved from the city-state, to the nation, to the United Nations; eventually we must move to world government.

In recent years we have given much lip service but have yet to learn the imperative in the interrelatedness of man and man and of man and his physical environment. Writing readable, sound, and persuasive science, Vogt warns that unless we learn these facts and do something about them, our chance of survival is about as good as the chance of a procession of saints' images halting the plague.

Drawing upon the best of the technical studies and his own observations and experience in traveling from country to country, Vogt arrays incontestable evidence on the plunder of our all-too-limited natural resources,

on the mismanagement of our food production, on the maldistribution of populations, and on many other questions. The adductions and outlook are dire—unless, as Vogt suggests, we—and he means all of us, you and me, and not only industries, farm combines, and governments—do something now, not next week, next year, or ten years hence, to restore our natural resources and control our rampant population growth.

You may not agree with all of Vogt's thesis or with the means of achieving his recommendations, but you will come away from this book impressed with the importance and urgency of its message.

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FISH STORY

A Study of Fish. Chapman Pincher. 343 pp. Illus. $4.00. Duell, Sloan & Pearce. New York.

THE author, in his preface, states that this book is intended to present facts about the structure and behavior of fish in a way understandable to the angler, naturalist, and general reader. Technical terms have been replaced wherever possible by simple words.

The subject matter treated is supposed to cover the field rather fully. There are 22 chapters, some of which are: Smell, Taste and Touch; Hearing, Balance, Current Perception; Blood, Breathing and Excretion; Fins and Locomotion; Hormone Glands; Breeding; Fish Migration; Scale and Scale Reading; and Pollution. The book covers practically the same ground as Norman's *History of Fishes*; but, whereas Norman thoroughly knew his subject and recorded it accurately, this book predominates in gross errors in statements of facts, indicating that the manuscript could not have been carefully checked by a professional ichthyologist. Much of the terminology was probably coined by the author and is not used in the fields of zoology or ichthyology.

By way of illustration of a few of the most numerous and most unpardonable errors, I cite the following: Page 5, Figure 3, shows behind the head the designation "spine," and on page 6 it is defined: "Embedded in the muscles of the dorsal half of the fish is the bony spine, which peters out behind but in front is joined to the skull." This "spine" probably refers to the vertebral column, or backbone. Page 8, Table 1, gives a primitive type of classification for fishes. One section is headed "Ventral fins set well back," under which are listed "eels, morays, conger;" these of course lack "ventral fins." We are told, page 12, that "The goldfish has taste cells scattered through the surface layer of its skin." It would be interesting to know the source of the statement that "Brown's killifish was reported from water at 128° F." We note in Figure 52 (of an eye of a fish) that the blind spot is labeled "special sensitive spot." Figure 73, of a gurnard, shows the free pectoral soft rays as "spines." Page 94, Figure 92, has the spiracle of a skate designated as "breathing hole." Page 105, Figure 99, and page 150, Figure 114, show the intestine as "food tube." Page 210: "A few fish, like the hagfish, and the sea perches of the genus *Serranus* are said to be truly hermaphrodite. . . ." On page 218, in regard to fertilization of eggs, the author says: "In most cases the revolving sperm bores a hole through the egg wall." There are dozens of other errors.

The majority of the drawings are most elementary, crudely done, and highly inaccurate. Legends in too many instances substitute absurd names for already well-established terms. The list of common names followed by scientific names is a fairly good one, as far as it goes, but I wonder what the "South American mud minnow," page 259, might be!

Publishers certainly have a responsibility to the public and should ask competent advice before publishing such books. I could not recommend it for schools nor for the angler, naturalist, or general reader.

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Smithsonian Institution

A NATURALIST'S LIFE

Ant Hill Odyssey. William M. Mann. 388 pp. Illus. $3.50. Atlantic-Little, Brown. Boston.

DR. WILLIAM M. MANN, Director of the National Zoo, writes, as he talks, in a refreshing and colorful manner of his childhood and student days up to the time he accepted his first permanent position as an entomologist in the U. S. Department of Agriculture. From early childhood "Bill" has been interested in collecting insects and all sorts of animals. Warm-hearted and generous, he has liked and been liked by people all over the world and in all walks of life.

Although he is a world authority on ants and the insect guests that live with them, his interests have covered the whole field of natural history. As a young naturalist Mann found that he could not properly study his specimens without a formal education. While a student at Washington State College and at Stanford and Harvard Universities, he made many friends among distinguished scientists.

A collecting trip in Arizona for beetles—financed by a wealthy expert—led to more important trips to Brazil—with the Stanford Expedition—to Haiti, Mexico, the desert of Arabia, and finally to the Pacific Isles. All sorts of odd insects and animals were collected on these trips; many, being new to science, have since been described by Dr. Mann and other scientists.

In Haiti, Mann had a Presidential passport, but it was his own personality and good sense that enabled him safely to penetrate inaccessible regions of this land of voodoo. Among the cannibals of Fiji, where "human meat is sweeter," and in the Solomon Islands, he traveled in safety through the interior and made friends with the natives. Later some of his former

companions were killed by villagers. Exposed to poisonous snakes, wild animals, and disease-carrying insects, Mann's only casualty was a severe case of malaria.

Full of human interest anecdotes, descriptions of wild countries, people, and natural history, this book is an inspiration to both layman and scientist. Bill's friends will look forward hopefully to additional books that will describe later expeditions to South America, Africa, and Malaya, as well as his experiences as a circus fan and as Director of the National Zoological Park.

THOMAS E. SNYDER

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STANDARDS OF MEASUREMENT

The Metric System of Weights and Measures. The National Council of Teachers of Mathematics: Twentieth Yearbook. Compiled by the Committee on the Metric System, J. T. Johnson, Chairman. xiv + 303 pp. Illus. $3.00. Bureau of Publications, Teachers College, Columbia University. New York.

THIS "Yearbook of the National Council of Teachers of Mathematics" gives a survey of the history, nature, and advantages possessed by the metric system, as supplemented by reports on the use of metric units in this country by some sixty individuals in different occupational fields. The general contention is made that the substitution of the metric system for the common one would give greater speed and accuracy in computation, make possible a freer interchange of knowledge and goods between the peoples of the world, and benefit education by the substitution of decimals for common fractions.

The work of the Committee has been, quite largely, one of compilation, with the contributions accepted for inclusion coming from those who are convinced that the best interests of America would be served by a more universal adoption of the metric system in this country. The writers are, however, not in agreement as to the methods that would be effective in bringing about the change. Thus we have such comments as the following:

> Another course is to finance and carry on propaganda for influencing the people and Congress. This might result in radical action (page 76).
>
> Any attempt to force the adoption of the metric system by mandatory legislation . . . would end in fruitless controversy (page 218).
>
> This should be approached by starting out with our public school system (page 94).
>
> Another basic approach would be for each one of the major industries to make a thoroughgoing study of its particular problems (page 220).

The compiled material is presented in four sections. The first two emphasize the history of the metric system and the present usage of that system in this country. The third is in the nature of a scrapbook, which has gathered together such items as scripts for school radio programs, newspaper accounts of teachers' meetings, reports on talks before clubs, and resolutions passed by clubs or other groups—all urging the adoption of the metric system in this country and the elimination of the common units. As might be surmised, these contributions are not entirely free from overstatement and ballyhoo. The final section carries as its subheading, "Methods of making the change to the metric system, both in general use and in education." The articles included are, however, more diverse than the heading would indicate. Three are by members of the Yearbook Committee and deal with schoolroom situations; all of these are excellent. The first of this group follows a line at variance with the rest of the book in that it scarcely mentions the metric system, giving its attention, instead, to methods of presenting decimals in the arithmetic classes through the decimalization of common units.

For the book as a whole one is apt to note adversely the brief and ineffective Foreword and the blithe disregard of significant digits in such problems as those of pages 54–56. A more drastic criticism would be made of such material as that on pages 81 and 82, which purports to give the history of the common units. The entire discussion is historically unsound. That it ignores the standards of length and weight placed in the Exchequer by Edward III and used officially by all England for two centuries before the time of Elizabeth is certainly inexcusable.

KEITH GORDON IRWIN

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ZOOLOGICAL SUPERSTITIONS

Animal Facts and Fallacies. Osmond P. Breland. xvii + 268 pp. Illus. $3.00. Harper. New York.

ANY teacher of biology or zoology, as well as natural-history museum workers, will testify to the appalling amount of misunderstanding and superstition that clutter up the average person's mind in respect to animal life. Professor Osmond P. Breland, of the University of Texas, has taken time out to record and correct some common misconceptions about animals. He has likewise included numerous items about special habits and idiosyncrasies of certain animals, such as the possible danger of shock from electric eels, the speed of birds, chewing of the cud by cows, and the delicate question of how porcupines mate.

Breland has arranged his sections on mammals, birds, fish, reptiles, and amphibians in question-and-answer form for the convenience of the reader. The work, however, cannot in any sense be considered seriously as a reference text; in the words of the publisher, "This is not a guide book."

Even though much worth-while information may be found between the covers of *Animal Facts and Fallacies,* one takes away the impression that the author has not come fully to grips with his undertaking. For one thing, the absence of scientific names will surely

prove disconcerting to readers who intend to seek additional data on the life habits of the animals they read about. Although the so-called jointed snake is admittedly not a snake, it cannot be dismissed simply as a "glass-snake lizard." The unfortunate and overall result of Dr. Breland's efforts to avoid being technical has been to swing to the opposite extreme, that of oversimplification. Chapters such as A Fish Said to Raid Hen Houses, Frogs that Fly, and Some Frogs that Eat Alligators have relatively little value in a book of his type, and they should not have been included.

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WORLD PROBLEM

Plenty of People. (Rev. ed.) Warren S. Thompson, xiv + 281 pp. Illus. $3.50. Ronald Press. New York.

JUST 150 years ago Malthus published an essay on population in which it was shown mathematically that human population, increasing as it does by geometric proportion, will sooner or later outstrip the possible increase of food production and present a prospect of starvation or the alternative of restricting the birth rate. For nearly a century this crisis was looked upon as lying in the indefinite future. There were still millions of acres of fertile land to be brought under the plow. Especially was this true of the United States and other Western countries where a rapid increase of population was taking place, yet the teeming populace lived better than had their ancestors.

Thus the evil day, when the pinch of hunger might be felt on a grand scale, was postponed, or lost to view. But as the reserve of unused land dwindled toward the vanishing point and the world's population continued to grow by 20,000,000 or more a year, the ghost of Malthus walked abroad again, and to many students of human affairs the menace of overpopulation seemed not far off.

For years Thompson has taken a leading part in the study of this problem. The voluminous census figures and other statistical data from all parts of the world have been thoroughly combed for information on the actual facts of birth rates, death rates, and resulting population growth or decline. Taking into consideration racial differences, conditions of sanitation, climate, disease, economic and social standards and customs, religious taboos, family traditions, and "opinion climate," or attitude toward childbearing, he finds that

even the most sanguine hopes of science and industry for increasing the means of living will be inadequate to supply more than a meager existence to our increasing numbers for more than a few years unless birth control becomes the rule in all the world. There can be no rational hope of a decent life for all mankind if birth rates remain at more than about one-third of the physiological maximum in any considerable part of the world's population.

The available mathematical data used to buttress these conclusions have, I think, been marshaled wih care and fairness. In the analysis and interpretation of the statistics there is some difference of opinion among demographers on certain moot points, such as the effect of immigration on the growth of population in the country of adoption. Among the numerous factors involved in his discussion of population, perhaps special mention should be made of the author's treatment of the immediate and aftereffect of war upon the military and civilian populations, of internal migration, of minorities, of epidemics, industrialization, prosperity, depression, changes in age distribution, use of contraceptives, and various means adopted by different nations for increasing the birth rate, including marriage loans, bonuses for large families, remission of taxes, police drives to reduce abortion, and birth control.

Thompson stresses the importance of the psychological factor in determining the size of the family. The desire for ease and luxury and the unwillingness to make personal sacrifice for children may be more decisive than considerations of economy and security. If the desire for a biological stake in the future through the continuation of the family is weak, perhaps it will be difficult to bring pressure to bear either for more or fewer children. At any rate, the author makes a good case for the proposition that the citizens of every country should more keenly appreciate the importance of maintaining a population of the right size and quality and of recognizing that a reasonable adjustment to the environment, though neglected at present, may soon become imperative, and that the matter cannot be put off indefinitely.

E. V. WILCOX

Washington, D. C.

THE SCIENTIFIC MONTHLY

MARCH 1949

THE SUGAR BEET: PRODUCT OF SCIENCE

GEORGE H. COONS

Dr. Coons (Ph.D., Michigan, 1915) is principal pathologist in charge of research on sugar beets, Division of Sugar Plant Investigations, Bureau of Plant Industry, Soils and Agricultural Engineering, ARA, USDA, Beltsville, Maryland.

THE creation of the sugar beet out of the lowly mangel-wurzel is a triumph of plant breeding. Through the miracles of modern science, sugar, once the luxury of the very rich, has been transformed into one of the cheapest and purest of foods, available to all.

Nor did the demands on science end with the launching of beet-sugar enterprise, for crisis after crisis has confronted the new industry. From the beginning, sugar from the sugar beet was in competition with tropical and subtropical sugar. Even around the factory itself, the sugar beet has had to maintain itself against other crop plants competing for the farm acreage. To cope with such situations, plant breeders have constantly increased the productivity of the sugar beet; agronomists have discovered efficient methods of growing the crop; chemical engineers have improved the processes of sugar manufacture; and against epidemic diseases resistant varieties have been bred. Only through these contributions of science has it been possible for the sugar beet to survive.

New problems face the sugar industries. The wartime sugar situation is fresh in our minds. Highly important in our economy in time of peace, sugar becomes, in time of war, of prime strategic importance. It is a necessity as food for our armies, our people, and our allies. In the manufacture of munitions and strategic materials, sugar and its by-products are essential. Sugar was among the first commodities rationed and the last off the list. In wartime, domestic production of sugar takes on especial significance. Grown within our borders, this sugar is safe from enemy submarines, and every ton produced at home, be it from mainland sugar cane or from the sugar beet, reduces by just that much the demands on maritime shipping. In a troubled world, there are potent reasons why our domestic sugar industries must be maintained.

In the few years since the close of the war, scarcity of sugar has almost turned to excess, a situation brought about in part because nations lack dollars to buy the very foodstuff they crave. World surpluses of sugar may bring economic problems of great magnitude to the industry as a whole, and these threaten the sugar-beet industry of this country. What has been so painstakingly built up appears now to be threatened unless production costs, already low, can be further reduced. This is the road to survival for the sugar beet. The industry turns to agricultural science for ways and means of accomplishing this. By intensified research better varieties and strains must be discovered, and these must be grown and processed in the most efficient ways. The chance of success in meeting this new appeal for help may, in some degree, be gauged by review of what sugar-beet research has accomplished and by appraisal of its armamentarium of scientific methods, techniques, and plant materials.

SUGAR AND THE SUGAR BEET

Sugar of commerce and the kitchen is sucrose. It is purchased as practically a chemically pure product. Obviously, *sucrose*, the chemical, is the same irrespective of the plant from which it is obtained. Impurities are so negligible in amount that for all ordinary uses beet and cane sugar are interchangeable. The United States is the world's largest user of sugar, consuming annually about 100 pounds per capita, which amounts to a little over 7,000,000 tons. Sugar is used directly as a food and in baked goods, confectionery, canned goods, preserves, soft drinks, etc. It has countless uses and applications in pharmacology, industry, and the arts. Nearly 25 percent of the sugar used in the United States is supplied by our sugar-beet industry.

The sugar beet is a Temperate Zone crop. It is grown in Europe, Asia, Australia, and in North and South America. In Europe, beet sugar is an article of international trade and is grown from Gibraltar to the Arctic Circle. It is grown in the USSR, both in Europe and in Asia; in Turkey, Afghanistan, and Iran; in Korea, Manchuria, and Japan. Culture in north-central China is a possibility. There is one factory in Australia. In the Western Hemisphere, culture of the sugar beet is chiefly in the United States and Canada, although Argentina once grew sugar beets on a small scale in the Rio Negro Valley. Trials to determine whether the crop will be an economical source of sugar are under way in Chile. Beet sugar production is established in Uruguay.

Acreage planted to sugar beets in the United States has in some years exceeded 1,000,000 acres. In 1948 the area for harvest was estimated at 700,000 acres. The crop is grown in 22 states, of which California, Colorado, Idaho, Michigan, Montana, Nebraska, and Utah are the leading producers.

The average acre yield of the sugar beet in the United States is about 12.5 tons. Since about 15.5 percent of the root weight is sucrose, the average acre yield of sugar amounts to nearly 2 tons. As a producer of calories, the sugar beet stands high among all crop plants, outranking corn and the small grains in digestible nutrients produced. From the roots, sugar is obtained as the main product, with beet molasses and beet pulp as by-products. In addition, the leaves and crowns, called sugar-beet tops, are left on the farm and constitute a valuable livestock feed. An average crop will supply the farmer about 4 or 5 tons of tops for forage. Yields of sugar-beet roots run far above the national average in those states having especially favorable conditions of soil and climate; for example, California crops often average 18 tons per acre, and Colorado, 16 tons. A yield of roots of 65 tons per acre has been reported for California, but the sucrose quality was low. An outstanding production record came from a 150-acre tract near Salinas, California, that averaged 37.57 tons of roots per acre with a 19.91 percent richness in sucrose, corresponding to a yield of nearly 7.5 tons of pure sugar per acre.

Sugar-beet seed is planted in a carefully prepared seedbed in early spring. The rows are usually 20–24 inches apart, and a fairly dense seeding is made. The initial close stand of seedlings is

Sugar-beet root of good type.

Beet-sugar factory. Roots in the storage piles represent only a portion of the total to be processed. (From the E. E. Patton collection, courtesy of Mrs. Emily Patton Colmus.)

thinned to leave single plants standing 10–12 inches apart in the row. The sugar-beet farmer seeks to retain a population of about 25,000 plants evenly distributed over an acre. By August, the leaves cover the ground and the rows can hardly be distinguished. A vast array of gray-green, glossy leaves presenting a million living surfaces to trap sunshine, a field of healthy sugar beets is an inspiring sight. By mid-October the crop is made, and the long, cone-shaped roots are ready for harvest. These run from 1 to 2 pounds up to 5 or more pounds in weight—root size being dependent on stand, soil fertility, soil moisture, length of season, freedom from wasting disease, and other factors. Among all plant forms, the sugar beet is unique because it stores in its roots such high amounts of pure sucrose. Anywhere from 12 to 22 percent of the fresh weight of the root may be sucrose. Other sugars, such as dextrose, are negligible in amount. As with root size, the quality of the root is a function of the conditions of growth.

A strictly biennial type of sugar beet is desired for culture. In the first year of growth, foliage and root are formed; if the roots are held over winter, then in the second year the plant sends up stalks on which the seed balls are borne. These are glomerules formed by the fusion of florets. A seed ball may contain one, two, or many true seeds. Botanically, the sugar beet is *Beta vulgaris* L., as are also the red garden beet, chard, mangel-wurzel, and the sea beet, commonly called *B. maritima,* found growing wild along the Mediterranean and Atlantic shores of Europe. Thus we have within this species the red beet, the garden vegetable with its purple-red, globose root; the mangel-wurzel, with its tankard or cowhorn-shaped root, red-, pink-, or yellow-fleshed; Swiss chard, the salad plant, whose small, sprangled root seems only to serve as a base for the green or red-colored, engorged leaves; and *Beta maritima,* the presumptive wild progenitor of the species, beetlike in character, but with thickened leaves and small, sprangled root. Allied to these forms, but entirely distinct in form and function, is the sugar beet. Its leaves are green or only tinted or veined with red. The root is long, obconical, or parsnip-shaped. Its flesh is white, never colored, and its skin is white or creamy. The sugar-beet root does not extend out of the ground as does that of the mangel-wurzel, but seems to nestle in the soil. All varieties of *B. vulgaris* readily intercross.

CREATION OF THE SUGAR BEET

Origins of most of our important crop plants are lost in antiquity. Over countless centuries, primitive man preserved his food plants, intuitively selecting them for improved form and function. They come to us as a legacy of the past, and we can only speculate as to the primitive types. In contrast to this, the origin of the sugar beet is known, and the research that gave it to us is a matter of record.

The discovery that sucrose, until then known only as palm or cane sugar, exists in plants endemic to Europe was made in 1747 by Andreas Siegmund Marggraf, director of the Mathematical-Physical Class of the Royal Prussian Academy of Science. This great chemist, protégé of Frederick the Great, found sucrose in *Beta alba* (presumably chard), in *Beta rubra,* the red beet, and in *Sium sisarum,* the skirret. This was not a mere recording of sweetness, as had been done a century before, but actual recovery of sugar from the juice by crystallization.

Nowadays, we take the trade in sugar as a matter of course, but for centuries sugar and other exotic products were the prized commodities of world trade. Nations contrived, struggled, plotted, and fought for trade supremacy. Trade with the

Indies through Asia Minor before the fall of Constantinople made the greatness of Venice, Pisa, and Genoa; the search for a new route to the Indies motivated the voyages of Columbus; "spices of the Orient," to use the intriguing phrase of our schoolbooks, were widely sought. But these were more sugar than spice. Columbus himself on his second voyage brought sugar cane from the Canary Islands to the New World. A century or two later America was to wrest from the Indies the trade in sugar.

When Marggraf made his discovery, France and England were engaged in a great struggle for mastery of colonial trade in which sugar, rum, and tobacco—the products of plantations worked by African slaves—were exchanged for the manufactures of Europe. Frederick the Great was building in Prussia the intense spirit of nationalism. Against this background Marggraf made his discovery, and he boldly announced that "This sweet salt, sugar, may be made as well from our plants as from sugar cane." It remained for Marggraf's pupil and successor in the Prussian Academy of Science, Franz Carl Achard, to bring to fruition the dream of the old chemist.

After the death of Marggraf in 1782, Achard became leader of the Physical Class of the Academy of Science (1786). He investigated to the high satisfaction of Frederick the Great and his successor, Frederick William II, various problems of chemistry, physics, and agriculture. The first experiments with beets as a source of sugar began in 1784, two years before the death of Frederick the Great. In the next decade, the supply of raw sugar to Prussian refineries became more and more uncertain and the price went higher and higher. In 1799 Achard received a grant from Frederick William III, then King of Prussia, that permitted him to devote his full time to improving the beet as a source of sugar and to finding processing methods. We may safely assume that Achard was fortified by his faith in Marggraf's discovery, by his own experiments, and by his knowledge that the peasants of the Magdeburg region had for generations made a sweet syrup from beet roots.

It was Achard's task to start with the beet complex as it existed on farms and gardens around Magdeburg and from this motley of types find those plants that contained a high amount of sucrose and only low amounts of compounds that interfere with recovery of sugar. When the usable sugar plant was found, proper methods of culture to give both root size and quality had to be discovered, and then practical methods of extraction, purification, and crystallization of sugar had to be devised. As a starting point Achard chose from some 26 varieties he had collected the *Runkelrüben* of the Magdeburg peasants. Probably its nearest relative today is the mangel-wurzel. The stocks available were a mixture of red-, white-, and yellow-fleshed plants extremely variable in shapes and sizes. By a masterly job of plant breeding Achard found the types richest in sugar and purified them. He compared and tested methods of culture, and wrote a practical guide on beet growing. To agriculture he contributed the White Silesian beet denominated by von Lippman, the historian, "as ancestress of all the sugar beets of the world."

It would be a pleasant excursion to explore fully the historical background of this development of the sugar beet as a Temperate Zone source of sugar. Here was a potential rival of sugar cane. The royal support given Achard's research shows the Prussian hope of becoming independent of imports from the plantations of French, Spanish, and English colonies. But our story must move more swiftly, and we may only set down that this great pioneer bred a fairly high-yielding and moderately sweet beet that for the first time was called sugar beet, and that he discovered in his little sugar factory at Cunern, Silesia, the methods for making sugar from it. These methods were forerunners of those used today in the great sugar factories that take advantage of every advance in chemical engineering. The record clearly shows that this brilliant scientist learned to grow the crop, developed processes for sugar extraction, purification, and crystallization, established breeding and seed production programs, taught students his methods and techniques, and by his publications made his discoveries the property of the

Contemporary cartoon satirizing Napoleon's attempt to establish a sugar-beet industry.

Sugar-beet seed field in Oregon. (Photo by Geo. T. Scott.)

world. Achard is universally recognized as the father of the beet-sugar industry.

That Achard's factory burned to the ground in 1807; that in the troublous period of the Napoleonic Wars he could not get funds to rebuild; that continuance of the industry in Prussia and the saving of the White Silesian beet for agriculture depended on Freiherr von Koppy, friend and follower; that Achard died in 1821, poor, unknown, and unrecognized for his contributions, make his life conform to the pattern the world so often contrives for its benefactors.

Because of far-reaching effects upon the launching of the beet-sugar industry, we need to relate the story to Napoleon's continental system which forbade any trade with England. Historians have dwelt on the paralyzing effects on the life of Europe that cessation of maritime trade brought about. Sugar refineries of continental Europe that had worked raw sugars of the tropics were idle. Everywhere there was a dearth of sugar. In the emergency, Napoleon seized on Achard's discovery and ordered beet sugar to be made in France. By his decree of March 25, 1811, he subsidized the establishment of beet-sugar factories. Hundreds sprang up, and the sugar beet was launched in France as a new crop. The discoveries of Achard, almost without notice at home, were the foundation of the French industry. A cartoon appearing at about this time shows derisive political comment on the project.

EARLY WORK OF BREEDING SUGAR BEETS

In the first quarter of the nineteenth century, when the sugar beet began to be utilized as a sugar source, the root yields of the White Silesian beet were probably not more than a few tons per acre, and sucrose percentages were probably not over 7 or 8 percent. Philippe-André de Vilmorin, of the famous breeding establishment of France, began selection work based on morphological characters about 1820. The record is obscure, but some improvements were made. In the late twenties and thirties the sugar-beet industry entered into very hard times. The industry failed in France and disappeared in Germany, only to be revived about 1840 with the introduction of the Imperial beet. This sugar-beet variety was a stronger grower and higher in sugar than the White Silesian from which it undoubtedly was derived. It gave new impetus to home production of sugar because it strengthened the weak part of the program, namely, agricultural production. Beet-sugar factories sprang up everywhere in central Europe. Sugar-beet culture was a boon to the run-down fields where a long succession of grain crops had made the soil foul with weeds, destroyed tilth, and created an unbalanced condition in soil nutrients. As a cultivated row crop it broke the retrograde sequence, and its by-products encouraged livestock farming. Agricultural economists attribute to the sugar beet the revival of agriculture.

A heavy yield of sugar-beet seed curing in shocks before threshing, St. George, Utah. (Photo by Bion Tolman.)

A great contribution to the science of plant breeding was made in France by Louis de Vilmorin, son of Philippe-André. In his researches to improve sugar beets, Louis discovered the significance of the progeny test as a means of judging the breeding potential of plants. He was the first to give continued attention to individual selection. This discovery, which now seems so very simple and, when viewed by hindsight, almost axiomatic, is fundamental in plant genetics. Vilmorin, in his improvement of the sugar beet, did not judge a selected plant by its size or its analysis, but by its potency in contributing these factors to its progeny. He developed ingenious tests to determine ease of lifting the beet—since apparently sugar beets with long, slender roots were sweeter—and gravimetric methods whereby sugar beets could be appraised for sweetness. Saillard is authority for the statement that he employed the polariscope as early as 1853 to determine richness in sucrose. Running through his researches was the cardinal principle of judging the breeding value of a plant by its progeny.

Within a decade, improvement of the sugar beet by application of Vilmorin's new technique was given further impetus by the rather general employment of the polariscope to determine the sucrose percentage of individual roots as a basis for discriminating selection. Here was a precision instrument that immediately showed the quality of a root, a determination by other means of analysis requiring hours. The way was open for highly efficient selection for sucrose content. Accordingly, by 1875, or even earlier, sugar beets were obtained by mass selection from the old White Silesian stock, either by way of the Vilmorin strains or the Imperial, that in sucrose percentage and perhaps in productiveness approached those we grow today.

MASS SELECTION

There sprang up on the continent of Europe a number of sugar-beet breeding establishments that employed the mass-selection system. Progeny tests constituted the guiding principle in the breeding program. The method of breeding used has been called mother-line breeding. In principle it does not differ from the "ear-to-row" system that at one time was the mainstay of corn breeding. A mother beet was selected and brought to seed along with other selectees. Although some isolation was attempted, for the most part selected individuals pollinated *inter se*. Then a portion of the seed from each mother plant was used in tests to determine the progenies giving the best performances in sucrose percentage, in root size, or in sugar production. For example, seed lots that were produced on the mother plants whose progenies were found highest in sugar were then individually increased in small parcels more or less isolated. The selection process and lining went on year after year, with selections being made from pedigreed stocks—pedigreed so far as the mother was concerned. Breeders spoke of the method as family breeding, and certain beets, always the mother beet, were denominated "heads of families."

Each establishment produced its own brand of sugar beet. It is to be expected that any outstanding advance by one establishment led to subsequent appropriation of a superior stock, either for direct use under a new name or for amalgamation into a general pool. Various claims were made by the breeding establishments as to what their meth-

ods were accomplishing. The only identifiable thread in the breeding history was the mother beet, whose selection was based on the qualifications of her progeny. This mother beet had been subjected to pollination by a great number of other roots similarly under test; the progeny was therefore not a selfed one, but a congerie of hybrids. Furthermore, a mother beet must have produced a heavy set of seed or it was dropped. Heavy seed production in the sugar beet almost always implies cross-fertilization. It seems clear that progenies tested were hybrids; hence, the only distinctive characteristic of a progeny was the fact that the plants had the mother in common. Any selection from the material at a breeding establishment would essentially be selection of F_1s, and these subsequently were allowed to interpollinate. The postulated continuance, generation after generation, of hereditary characters from a given mother that once was head of a family obviously did not take place. What did take place was a slow but probably continued mass selection toward a target of either high sucrose percentage or high productivity, since it is entirely probable that these strong physiological differences could, by the methods employed, guide selection. As these types were established and the so-called high-sugar families began to be kept distinct from high-yield families, a selection would produce more decisive winnowing out of aberrant, or nonconforming, types. As a result, sugar-beet breeding establishments of continental Europe, all following the same methods of selection, offered in the period 1890 to date relatively fixed physiological types—high-sugar (*Zucker*, or "Z"), high-yield (*Ernte*, or "E"), and a compromise type (*Normal*, or "N")—the last-named being the catchall group into which stocks not falling within other types were bulked.

No disparagement of the European methods that gave us the sugar-beet stocks used for over seventy-five years, and on which the sugar-beet industry of the world was founded, is intended by the above critique. By slow process, the "varieties," or brands, of sugar beet were produced that, under normal conditions of growth, were high-yielding, extremely sweet, and a distinct contribution to agriculture. The textbooks on plant breeding at the end of the nineteenth century cite the meticulous efforts of the sugar-beet breeder as exemplifying proper applications of the breeder's art.

Schribaux' graph showed that in the period 1838–68, with morphological selection, the average richness (sucrose percentage) progressed from 8.8 to 10.1; in the period 1868–88, to 13.7 percent; and in the period 1888–1912, to 18.5 percent. Schneider cites similar comparisons, by periods, drawn from the agricultural and factory statistics of Germany, but he calls attention to low recovery of sugar in the earlier periods of the industry—factory inefficiency being interpreted as the result of low-quality sugar beets. Bonne cites records from a sugar-beet breeding establishment that show a gradual climb in sucrose percentage from an average 16 percent in 1883–87 to about 23 percent in 1930.

There is evidence from tests run in the United States in the period 1880–1900 that the various European varieties of sugar beet commonly produced 10–20 tons of beets per acre, depending on agricultural conditions, and 12–18 percent sugar. The results of the various tests, many of which were conducted under the leadership of Dr. Harvey W. Wiley, famed chemist of the Department of Agriculture, read very much as do tests undertaken with European varieties today. There have been improvements. They are not the decade-by-decade increases commonly postulated; however, the data do prove that increased factory efficiency and an improved crop plant operated jointly to bring very definite advances in sugar production.

THE SUGAR BEET IN AMERICA

Early history of the industry in the United States is a record of one failure after another, as enthusiasts attempted to attain in America the success that the sugar beet was winning in Europe. The first factory was started in Northampton, Massachusetts, in 1837. After three years the venture was abandoned. Other attempts in a number of states likewise were failures. Of great interest was the attempt made by the Mormons in 1853 to establish the industry in Utah. This dramatic story, finally to be crowned by success nearly forty years later, is told by Taylor in his *Saga of Sugar*. The first successful factory in the United States was that at Alvarado, California, established in 1870, rebuilt in 1879, and finally modernized in 1936.

The entire history of the sugar beet in the United States is replete with instances of factories being established only to fail for one reason or another. By 1890, 16 factories had been erected and 13 removed, leaving 3: at Alvarado and Watsonville, California, and Grand Island, Nebraska. By 1900 the score stood: factories built, 50; removed, 16; existing, 34. By 1920, 146 factories in all had been erected, 42 had been removed, and 105 were left. By 1940, a total of 164 had been built, 67 removed, and 97 standing. In 1948 there were, in all, only 85 factories, located in 16 states, and

owned by 22 beet-sugar processing companies. Of these, 11 were scheduled as not operating in the 1947–48 campaign. The history of factories built and factories left is illustrative of the difficulties that this industry has faced as it has won its way in America.

Until a relatively recent period, the sugar beets grown were the product of European breeding establishments. In spite of early recommendations of Dr. Wiley and other scientists of the Department of Agriculture, and the clear showing in the period 1890–1900 of advantages from homebred varieties and home-grown seed, the industry, dominated by the European technologists brought over to run the sugar-making equipment, insisted on importing seed. This seed was capable of giving good crops under conditions comparable to those of Europe, but in the districts where the industry was struggling to become established the conditions were often decidedly different from those of Europe. As we shall see, the use of imported seed was at the bottom of much of the trouble of American factories.

In 1914, when war with Germany broke out, continental European ports were blockaded by the British fleet. This created an emergency for our beet-sugar factories because seed to plant the crop had to come from European breeding establishments. Arduous diplomatic representations finally obtained permission for German seed to come through the cordon—the seed to be consigned to the Chief of the Bureau of Plant Industry, who could release seed only when bond was given that the seed would not be transshipped. A prerequisite on the German side of the transaction was that the payment be in gold and that bond be given that the gunny sacks would be returned. In wartime, jute may be more important than money. In 1918 America, cut off from German seed sources, began to grow its own stocks as straight increases of European varieties. Eventually, the sugar-beet industry was able to produce almost enough seed to plant its acreage. Production followed the conventional European methods of growing roots in one season, storing them over winter in pits or silos, then transplanting the roots in the spring to produce seed. The job was expensive, and seed yields were uncertain. An estimated average yield of 500 pounds of seed per acre is probably liberal. As soon as trade was resumed after the war, the factories went back to importing sugar-beet seed.

Thus there was continued the anomalous situation of a great agricultural industry dependent on foreign sources for the seed from which to grow its crop. The World War I experience should have taught the disastrous effects of such dependence, but the lesson was soon forgotten because of the greater convenience and lower cost of foreign-grown seed.

EPIDEMIC DISEASES OF SUGAR BEET

In this period, it was the common experience for factories established in high hope to run a few years and then fail because recurrent outbreaks of disease meant either no sugar-beet roots to process or that the roots were of such low quality as to be unprofitable to work. Farmers would not grow a crop that was not dependable, and the factory would be forced to close. As the record shows, many beet-sugar factories had become almost gypsies, being moved from one place to another always in the hope of finding a favorable district where production would be stable.

Curly top is an especially serious disease of sugar beet and other crops. By 1926, it had caused repeated failures of the sugar-beet crop in the states west of the Rocky Mountains. Abandonment of factories in the West was entirely attributable to the ravages of curly top.

The disease is caused by a virus carried to beet fields by the beet leaf hopper (*Circulifer tenellus*), an insect that breeds on mustards, Russian thistle, and other weeds. Overgrazed rangelands were invaded by these weeds, so that vast tracts of the semiarid West became breeding grounds for the leaf hopper. Even more serious in furnishing breeding grounds were the abortive attempts during World War I at grain farming on plowed rangelands near irrigated districts. The native grass cover was destroyed, and, when the fields were abandoned, they became covered with almost solid stands of the weeds that were host plants for the insect. Enormous populations of the beet leaf hopper were bred on these weedy tracts. When the vegetation began to dry in the spring the leaf hoppers moved to the young beet plants in the fields in the irrigated valleys. Many of the insects carried curly-top virus, and the plants on which viruliferous hoppers fed became infected with curly top. Other leaf hoppers picked up the virus from these plants and carried it about so that soon no plant in the field escaped the disease! European varieties of sugar beet succumbed almost completely. The leaves curled, and growth of top and root almost stopped. In years of epidemic the havoc from the disease was clearly evident in late spring. Around factories, thousands of acres of sugar beets were plowed up for replanting to other crops or simply were abandoned.

As counterpart to epidemics of curly top in

sugar-beet districts west of the Rocky Mountains the more eastern producing areas were subject to sporadic epidemics of leaf spot. This disease, caused by a fungus, *Cercospora beticola,* blights the tops and causes dwarfing of root growth and depression of sucrose percentage. Its effects are less dramatic than curly top but no less damaging to the sugar-beet industry. The disease is a wasting one that reduces tonnage and sucrose enough to make the beet crop unprofitable both to farmers and factory. Years of outbreak are those in which in the early part of the season rainy periods are frequent and total precipitation abundant. Paradoxically, the very conditions that should give a bumper crop bring only disappointment as wave after wave of blight occurs. In epidemic years leaf spot may kill back the entire foliage bouquet several times in the growing season. New growth is pushed out, only to be blighted in two or three weeks. Replacement of blighted foliage by the new growth is at the expense of the root growth and sugar storage; hence a blighted crop is lacking in both weight and quality. The farmer has a short crop to harvest, and the factory has low-grade roots to process that cannot yield a profit. In the period 1915–30 blight years recurred frequently. Factories in the humid area and in other districts subject to leaf spot were in financial distress.

BREEDING FOR DISEASE RESISTANCE

In 1925 direct attack against curly top and leaf spot by disease-resistant breeding was started in

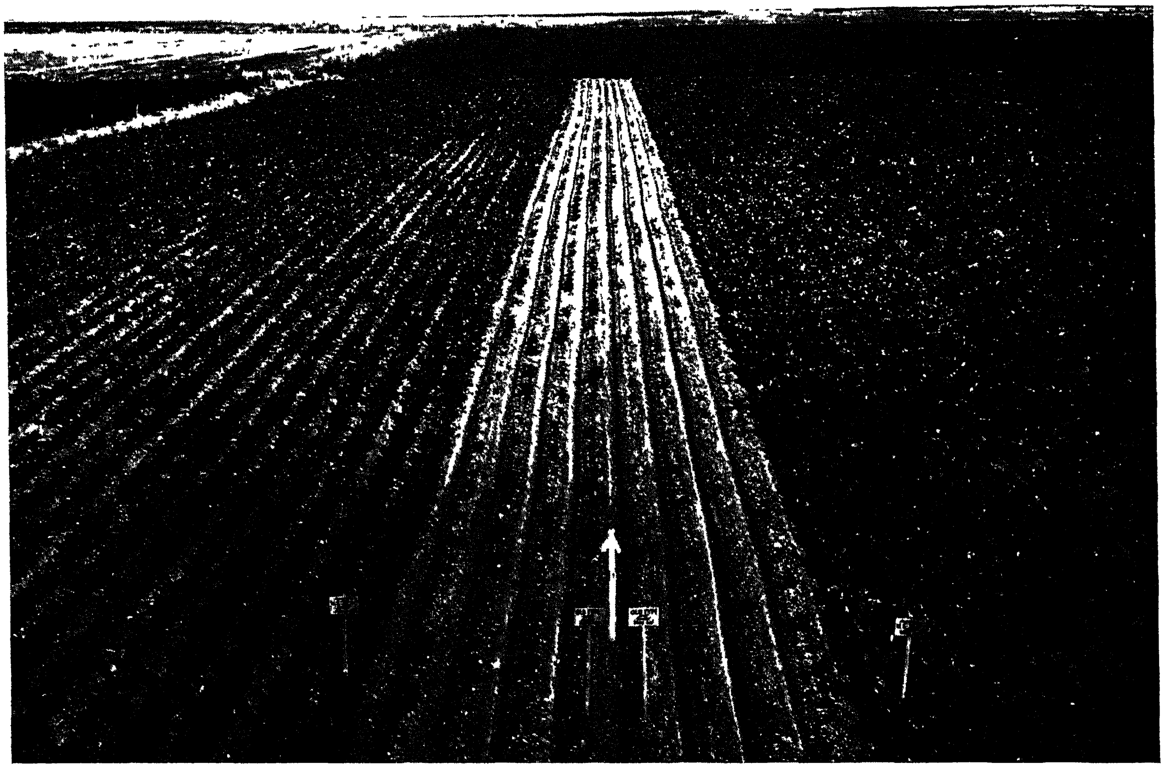
Curly-top-resistance tests in agronomic plots at Castleford, Ida., photographed Sept. 19, 1930, by Eubanks Carsner. *Left foreground* (plot 1703): four rows of U. S. No. 1 variety; *right* (plot 1704): four rows of the nonresistant European brand (Pioneer), which are continuous through the field. Photograph shows also replicated plots of the U. S. No. 1 variety and those of three other resistant strains being tested.

the Bureau of Plant Industry. The program involved search for new genes by making collections of the wild progenitors of the sugar beet, selection of resistant types within the existing commercial varieties and strains of sugar beet, and the setting up of field stations at locations where severe exposures could be expected each year, to permit discriminating reselections.

In sugar-beet fields where curly top is severe, no plant escapes infection. Susceptible plants suffer very drastic effects, but a few plants—possibly 1 or 2 per 10,000—are outstanding because they show less severe reactions. Selection of these individuals from fields that had been severely attacked gave progenies that were more resistant than the general run. A combination of the best of such selections, plus roots from other sources having more or less resistance, gave rise to U.S. No. 1, the first curly-top-resistant variety of sugar beet, bred by Carsner, Pack, and their associates. *Achievement Sheet 78 P* of the Agricultural Research Administration tells how U.S. No. 1 held the line and gave new hope to the industry in the grave period when sugar-beet culture in the West was about to be given up.

U.S. No. 1, the curly-top-resistant variety, first became available for commercial use in 1933, and it was followed in 1935 by U.S. 34 and U.S. 33, reselections from it. The latter was selected for increased curly-top resistance and for high sucrose as well. For many years it was the standard variety for western United States. In 1938, U.S. 12, a reselection, and in 1940, U.S. 22, both varieties showing improvement over the earlier ones, were released. The resistant sugar beets have brought about most dramatic changes in the Western agricultural situation. The sugar beet became a dependable crop. Districts once abandoned for its culture now returned to full-scale production. The factory at Toppenish, Washington, was rebuilt on the foundations of the factory that had been given up and torn down because of curly top. Other factories have entered areas previously considered unsafe because of curly top. By 1938 the Western beet fields were planted almost exclusively to U.S. curly-top-resistant varieties, and a seed enterprise to supply domestically grown sugar-beet seed had sprung up. This was the first fruit of sugar-beet breeding projects.

Control of sugar-beet leaf spot could not be accomplished by mass selection. Nowhere in blighted fields did individual plants show themselves as outstanding in resistance. Furthermore, beet leaves as they become mature and moribund are subject to attack by leaf spot. This made recog-



Curly-top-resistance breeding field near Twin Falls, Ida., showing contrast between resistant and susceptible types. Portion of field left of arrow was planted April 11; right of arrow, May 1. Susceptible European variety (Old Type) is in center, flanked by the highly resistant variety U. S. 22.

nition of potentially valuable individuals almost impossible. When disease-resistance breeding began in 1925, there was available at Fort Collins, Colorado, as a result of many years of endeavor of the veteran breeder W. W. Tracy, a large array of sugar-beet strains, separated out from various sugar-beet accessions. Tracy's assignment was to break the sugar-beet complex into its components on the basis of morphological characters.

When about 200 Tracy strains were grown in 1925 at Rocky Ford, Colorado, under conditions of severe leaf-spot exposure and judged for leaf-spot resistance, 14 strains were noted as outstanding. All others had blighted severely, but these remained relatively green. Leaf spot that season also made a relatively severe attack at Fort Collins, where Tracy had the same strains under test. The strains that were outstanding in leaf-spot resistance at Rocky Ford were also outstanding at Fort Collins.

It was clear, therefore, that to breed leaf-spot-resistant varieties selection and continuous inbreeding would be required until the factors for resistance were stabilized. Such work was begun by Coons and his colleagues using the Tracy strains and other selections as the basis. It was soon found that, whereas inbreeding stabilized the characters governing resistance, there was definite tendency for the strains to lose vigor. It was therefore very heartening in 1932, and later, to find that hybrids between two relatively nonvigorous types showed strong heterosis response. Inbreeding technique could thus be utilized to increase and stabilize leaf-spot resistance. The job of breeding for leaf-spot resistance resolved itself into production of as many distinctive leaf-spot-resistant inbreds as possible, and then making hybrids among them to find the pairs that would give greatest heterosis response.

The first leaf-spot-resistant variety introduced in 1937 (U.S. 217) was a synthetic made from 5 inbreds, all with better-than-average leaf-spot resistance. Seed stocks of the 5 were pooled, and plants were grown in the seed field from the seed mixture so that all types could intercross. The variety proved to be very resistant to leaf spot, high in sucrose, but somewhat low in root yield. It was soon replaced by U.S. 200 × 215. This variety was obtained by pooling the seed of Inbred U.S. 200 and Inbred U.S. 215 and planting the mixture in the seed field, thereby allowing the two components to flower together and intercross. Since the sugar beet has perfect flowers, and among inbreds there is more or less tendency toward self-pollination, a variety produced from a planting stock made by pooling of seed consists not only of the hybrid but of sibs of parent varieties as well. The proportions of these classes cannot be predicted and may vary from field to field. It was hoped that in U.S. 200 × 215 about 40 percent of the progeny would be hybrid. Heterosis shown by a hybrid portion is more or less nullified by the lower yields of inbreds in a progeny. One of the inbreds, how-

ever—U.S. 215—was noteworthy among all available strains, because it was highly productive, about equaling European commercial varieties in root size. Its sibs probably did not bring about depression of yields.

U.S. 200 x 215 was ready for introduction in 1939 when the European war cut off supplies of foreign-grown sugar-beet seed and forced the American industry to move 100 percent into domestic seed production. Seed supplies of the inbreds, U.S. 200 and U.S. 215, were available from Federal sources to furnish the planting stock from which commercial seed for the affected districts of the humid area could be produced, thereby averting a seed crisis such as that of 1914–18. It is noteworthy that the variety supplied was not a mere stopgap but an improved, disease-resistant variety at least 5 percent more productive than the European varieties it replaced.

By continued breeding research, new leaf-spot-resistant varieties have been introduced, notably U.S. 215 x 216, which, in its current phase, will be almost exclusively grown in many districts subject to leaf spot. This variety without leaf spot is equivalent in sugar production to the European varieties previously grown. Under conditions of leaf-spot exposure, it is greatly superior, exceeding the susceptible European types by 10–15 percent or more, depending on the severity of leaf-spot attack.

NEW PROBLEMS FOR THE PLANT BREEDER

It is a common experience among plant breeders that when certain primary requirements in plant improvement are met then other plant characteristics are revealed as of great importance. Thus, when growers attempted to utilize the new curly-top-resistant sugar-beet varieties in winter plantings in central California and in the Imperial Valley, these varieties showed such strong tendency to bolt—that is, to go to seed in the first, or vegetative, year's growth—that fields became a tangled mass of seedstalks. These had to be cut and disposed of before the roots could be harvested. One field was reported as yielding 16 tons of roots and 9 tons of tops. To meet this the plant breeders came forward with U.S. 15, a sugar-beet variety that, although only moderate in curly-top resistance, has strong resistance to bolting. The variety is also resistant to downy mildew and to rust, two other diseases serious in California coastal districts; hence, it is very well adapted to California conditions.

This variety was the product of breeding research conducted at the New Mexico Agricultural Experiment Station by Coons, Stewart, and Elcock and was obtained by selection from a high-sugar European variety. The variety was about to be dropped because of its extreme susceptibility to leaf spot when its tendency toward low bolting was discovered in tests conducted near Davis, California. Introduced into commercial use in 1938, the variety has become the stand-by for all winter plantings of sugar beet.

The most noteworthy achievement of U.S. 15 is the bringing about of successful sugar-beet culture in the Imperial Valley of California, a matter signalized by the opening in 1948 of the new $5,000,000-factory near Brawley, California. In its sugar-beet production, the vast, fertile Imperial Valley, watered by impounded waters from the Colorado, takes advantage of climate by reversing the order of plant growing seasons. Seed of U.S. 15 is planted in October, the plants of this cold-tolerant variety grow during winter, and the roots are ready for harvest in May and June. With such a planting schedule, and with the onset of cold weather soon after planting, ordinary varieties of sugar beet would bolt so much as to be almost unusable. There is sometimes a light exposure to curly top; hence, the combination of resistance to both bolting and curly top makes this variety of especial value. Over a number of years the Valley has given average yields of better than 18 tons per acre, with average sucrose percentages of not less than 18 percent. Such a record indicates that the area is one of the great sugar-producing regions of the world.

Another serious disease problem has been brought to the sugar-beet breeder. In the humid area, stands of sugar beets are often decimated by a seedling and root disease called black root. Research has determined that the primary cause of black root is a phycomycete, *Aphanomyces cochlioides*. Other damping-off organisms associated with seedling diseases are either less important or more readily controlled. The sugar beet will not attain a position of stabilized production in the United States, nor will full mechanization be possible, until a control is found for this disease, which strikes at the stand of plants in the field. Here again breeding of resistant plants represents the solution. Since the area is also subject to leaf spot, control of black root requires superimposing of resistance to the black-root fungus upon resistance to leaf spot. Fortunately, U.S. 216, the most leaf-spot-resistant inbred available, is outstanding in its resistance to black root, so that a start toward the desired combination has been made. Active breeding research has been under way three years, and

Comparison of leaf-spot-resistant variety (A) with nonresistant variety (B). *Cercospora* leaf-spot blights leaves of susceptible plants.

promising results have already been achieved by mass selections within the leaf-spot-resistant sorts.

NEW TECHNIQUES IN SUGAR-BEET BREEDING

Other developments in sugar-beet genetics may have important applications in meeting the situations that confront the domestic sugar industry. Tetraploid sugar beets have been produced by use of colchicine and other chemicals, and these may eventually be found to contribute something of value. So far as the research has gone it is evident that tetraploidy of itself does not automatically confer increased sugar production. The tests indicate that tetraploidy leads to increased vegetative growth, thereby giving greater root size, but the tendency to remain in growing condition results in a lower sucrose percentage. Thus the increased root size is compensated by the lower sucrose percentage, and sugar yield is not enhanced. An interesting feature of the tetraploids is the tendency of the seed balls to be one- or two-germ, indicating that by tetraploidy a seed ball could be obtained that would give one, or at most two, plants in a place instead of the plant clumps such as arise from an ordinary seed ball. Triploids have been produced and are under test to determine performance. It is significant that curly-top or leaf-spot resistance shown by a parent diploid variety continues in the respective polyploid form at about the same level.

One of the greatest advances in breeding techniques has come from the discoveries of F. V.

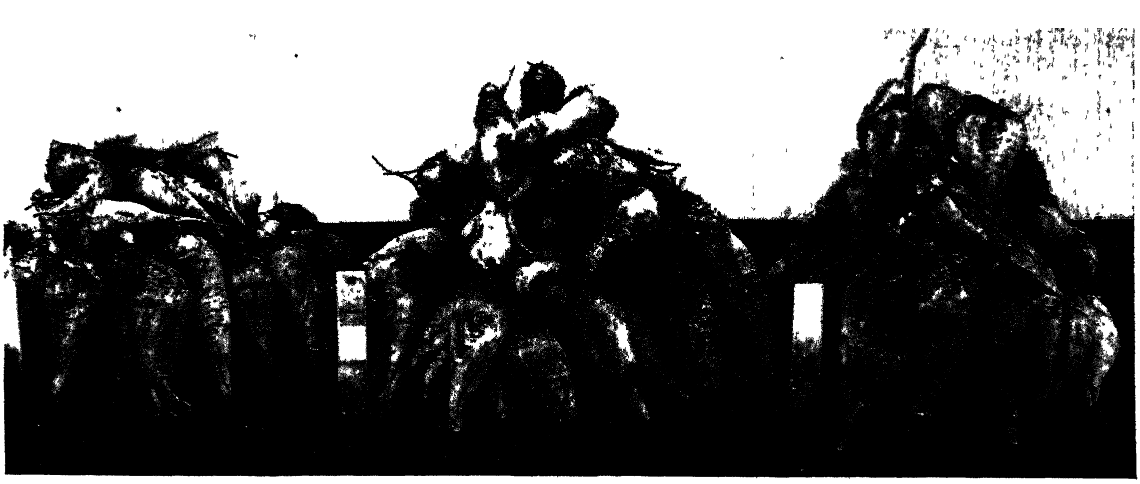
Owen on male sterility in sugar beets. His work has shown that one form of male sterility in sugar beets is cytoplasmically inherited. Flowers borne on plants whose cytoplasm is of the S type, if fertilized by pollen from plants bearing the proper complementary Mendelian characters, give rise to a progeny that is 100 percent male sterile. The situation with sugar beets is akin to that reported for the onion by Jones and his associates and for some other plants. The discovery is especially significant because it opens the way to production of seed that is 100 percent hybrid, a thing previously not possible.

Practical application of the male sterile character in sugar-beet breeding may be illustrated by work now in progress in production of a leaf-spot-resistant single-cross U.S. 216 x 225. As indicated, it is necessary to discover resistant lines and stabilize them by breeding. To restore vigor and take advantage of heterosis, single-cross hybrids or synthetic varieties are produced from the appropriate inbreds. The percentage of hybridization obtainable in the seed field is a matter of chance so long as the only practical method of obtaining some intercrossing is the pooling of seed stocks used to plant the seed field.

Tests have indicated that the hybrid between Inbred U.S. 216 and Inbred U.S. 225 is leaf-spot-resistant, high-yielding, and high in quality. By repeated backcrossings the male sterile character, originally from Dr. Owen's material, has been incorporated and a male sterile equivalent of U.S. 216 produced. If the pollen in future backcrossings is from hermaphroditic U.S. 216 plants chosen for the proper Mendelian characters, then the plants produced from seed grown on U.S. 216 male sterile plants should be 100 percent male sterile. Once the hermaphrodite is purified so that it carries only the proper complementary factors, it is possible to continue the male sterile phase of U.S. 216 indefinitely. U.S. 216 MS is being grown in Oregon seed fields bordered by rows of U.S. 225 to serve as pollinizer to produce U.S. 216 x 225. The stock of U.S. 216 does not as yet show complete male sterility, but it is expected to produce about 75 percent male sterile plants, the remainder being relatively weak pollen producers. In the seed produced, the percentage of sibs should not be important. In future productions of hybrid seed it may be possible, through manipulation of the genetic material, to obtain seed that is 100 percent hybrid.

The new discoveries give promise of permitting the sugar-beet breeder to obtain with his material what the corn breeder accomplishes by his detasseling technique. It will be remembered that in the production of hybrid corn all pollen-producing tassels of inbred "A" are removed in order that the ears on the detasseled plants may be pollinated solely by inbred "B" grown in adjacent rows.

Applications of the male sterility technique in the production of hybrids have gone forward so rapidly that is has not been possible to appraise fully what may be expected. There are results from Dr. Owen's laboratory with curly-top-resistant varieties that indicate that hybrids of curly-top-resistant lines made by utilization of male sterility reach a new plateau of sugar-beet productivity. With the leaf-spot-resistant varieties, advantages have come from the earlier introductions of U.S. 200 x 215, U.S. 215 x 216, and U.S. 216 x 225, in spite of the



Hybrid vigor test, Arlington, Va. Representative roots of the hybrid, U. S. 215×216, are shown at center, with a similar number of roots of the mother and pollen parents at right and left.

Variety test at State College, N. M., under conditions of severe curly-top exposure. U. S. 15, leading variety for fall plantings in the Imperial Valley and for winter plantings in Southern California originated from the plants growing in Row 277 A. (Photo by H. A. Elcock, October 2, 1930.)

fact that intercrossing was a matter of chance and hybridity of the seed was only partial. These give abundant promise that if the seed is 100 percent hybrid there will be superior performance. There is reason to expect, also, that disease resistance can be enhanced, since eventually all plants in the field will be the progeny of mother plants high in leaf-spot resistance.

Taking stock of our progress, research is well on the way toward gaining and holding for the sugar-beet industry a considerable degree of control of the major diseases, curly top, leaf spot, and soon of black root. In addition, the heritable characteristics that control bolting, downy-mildew resistance, and rust resistance have been combined with moderate curly-top resistance. In varieties about to be introduced these factors have been combined with high curly-top resistance and other desirable characters. The research program must take cognizance of the fact that viruses and fungus diseases are not static entities, but are constantly changing. It is almost axiomatic in plant pathology that the strains and biological forms that cause serious outbreaks of disease so change over a number of years that the resistant varieties once capable of meeting a situation became ineffective and must be replaced. The program for control that is based on resistant varieties must be a continuous one if new virulent forms of virus or fungus are not to nullify the gains that have been made.

The work I have outlined has stressed the research of the Department of Agriculture with which I am familiar. Plant breeders of beet-sugar companies and of the State Experiment Stations by their research, in part utilizing strains from the government program, have made and are making important contributions to varietal improvement. They have produced varieties that in addition to affording disease control open the way to greater productivity. Now that disease problems are more nearly met, there are opportunities for plant breeders to attain even greater local adaptation of varieties, to utilize polyploidy, and especially to capitalize on vigor of hybridity.

These normal developments of the breeding program are being elbowed to one side by new requirements forced to attention by the economic situation confronting the sugar-beet industry. A surplus of sugar on the world market may further depress the already low prices of sugar. In such an eventu-

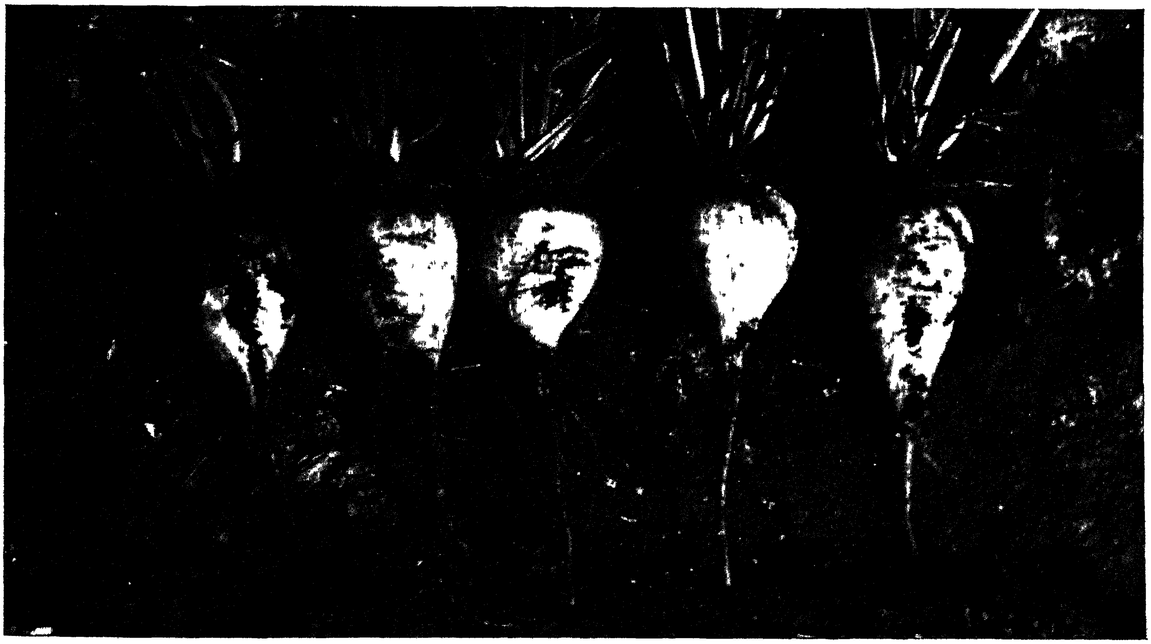
ality, only those segments of the industry capable of producing sugar with the greatest degree of efficiency can hope to survive.

It is generally recognized that the greatest costs in production of beet sugar, and those offering the greatest chance of economies, are farm costs in producing the sugar beet. The price paid for the roots to be processed amounts to nearly half the cost of the sugar produced. Reduction of costs on the farm must come from greater production per unit of area worked and from reduction of the large amount of hand labor now used in growing the crop.

The breeding programs already under way are making every effort to increase production. If combined with improved agricultural practices they will do much to raise farm productivity. The greatest demand is further to reduce labor costs. In the operations described earlier, hand labor has been employed to block and thin the crop and to hand hoe it two or more times. What the industry must have if it is to reduce costs of sugar-beet production in the first half of the season is a full mechanization of these operations. The fact that the sugar-beet "seed" is really a seed cluster means that seedlings emerge as clumps. An attempt has been made to counteract this by milling the seed to one- or two-germ units, but this has its disadvantages. So long as the seed unit as planted cannot be depended upon to give a single plant exactly where it is dropped in the drill row, then precision planting comparable to that attained with other crops is not possible. If the morphological character for single germness of seed could be introduced into the sugar beet, and if this character were combined with other essential characters such as disease resistance, high yield, and high quality, then the first step in full mechanization could be taken. A sugar beet whose seed has the character of single germness may be precision planted. If resistant to black root, stand would be maintained, and the rows could be thinned mechanically to an exact pattern. In many districts mechanized weed control would be entirely applicable. Thus the employment of hand labor to start the crop would be done away with.

Already the sugar beet is being harvested with considerable success by machines. Nearly 75 percent of the California crop and 50 percent of the Colorado crop in 1948 were machine harvested. Machines top, lift, and frequently elevate the roots to the truck, the beet tops being windrowed at one side. Sometimes the harvesting job is done as two operations and sometimes by a combine. One type of machine loosens and lifts the root by plowshares, then clasps the leaves and elevates the entire plant to the topping mechanism. Leaf-spot-resistant varieties that retain the beet tops in spite of blight conditions that otherwise would destroy them, have made possible the successful operation of machines of this type. This is a contribution plant breeding has made to mechanical harvesting of sugar beets.

Other contributions are on the way. One plant



The new hybrid variety developed by G. W. Deming at Fort Collins, Colo., from cross of globose red garden beet and the sugar beet, is streamlined for easier mechanical harvesting. (Photo by P. B. Smith, Beet Sugar Development Foundation.)

breeder has made definite advance in combining the globe shape of the red garden beet with desirable characters of root size and quality to produce an easy-lifting beet. In actual trials these beets, prevailingly peg-top in shape, lifted more easily and with minimum breakage—a desirable contribution to both efficient and economical handling.

At present the work of harvesting machines, coming at a time of labor scarcity, is not viewed too critically so long as the beets come out of the ground quickly and with not too serious loss from skips, breakage, or tare. It is to be expected that better and better performances will be demanded of harvesters, especially with respect to topping. If breeding research achieves greater uniformity of tops and crowns, then this may simplify the problem of the agricultural engineer. If breeding research improves keeping quality of roots in factory storage piles, then the present heavy losses from deterioration can be reduced.

THE FUTURE OF THE SUGAR BEET

Thus we have in the sugar beet a creation of science that has given great benefits to America. A vegetable mediocre in productivity has been transformed into one of the greatest producers of foodstuffs of all our agricultural crops. In Europe, its culture broke up a cropping sequence that was retrograde in its effects on the soil. It became the keystone of European agriculture. In America the sugar beet has functioned similarly. Allied to the dairy industry, it has built an agriculture that has maintained soil fertility and made farming in many districts profitable. It is the keystone of irrigation agriculture.

To achieve these results in Europe it was necessary to breed a highly productive sugar beet. For the crop to succeed in the United States it has been necessary to breed strains of sugar beet to meet disease hazards unknown to Europe.

The sugar-beet industry is now established in 22 states. A capital of nearly $250,000,000 is invested in factories and facilities for processing the sugar-beet crop. In their farm lands, improvements, and equipment, and their irrigation systems, farmers have an investment equaling that of the factories. Much of this development is in the West. The significance of the sugar beet to Western agriculture cannot very well be overemphasized. It is a cash crop that the farmer grows for an assured price. Its resistance to hail damage and to hazards of weather make for a dependable harvest. Most important, the sugar-beet crop is marketed in chemically pure form, and its by-products are fed to livestock and are marketed as meat. High freight costs preclude the growing of bulky, low-value crops on farms of the West located far from markets. For these farms, the sugar beet is almost an irreplaceable crop.

The sugar-beet industry is now faced with economic problems that threaten its survival. The contributions science has made give promise that research again can meet the challenge. If productivity can be increased, if disease ravages can be lessened, then the crop has a chance. If the machine can be made not only to turn the soil and to plant the crop but also to do the job of weed control and to harvest the crop, then the sugar beet can continue its benefaction to American agriculture. The plant breeder is asked to play a significant part.

REFERENCES

ACHARD, F. C. Anleitung zum Anbau der zur Zuckerfabrication anwendbaren Runkelrüben und zur vortheilhaften Gewinnung des Zuckers aus denselben. 1803. (Reprinted in Ostwald's *Klassiker der exakten Wissenschaften No. 159*. Leipzig: 1907, 72 pp.)

BRANDES, E. W., and COONS, G. H Climatic Relations of Sugarcane and Sugar Beet. *U. S. Dept. of Agr. Yearbook 1941*. 421–38.

CARSNER, E., *et al.* Curly-Top Resistance in Sugar Beets and Tests of the Resistant Variety U. S No. 1. *U. S. Dept. Agr. Tech. Bul. 360*. 1933, 68 pp.

CARSNER, E. Development of Curly-Top-Resistant Sugar Beet Varieties. U. S. Dept. Agr. ARA, *Research Achievement Sheet 78 (P)*. July 9, 1947, 2 pp.

COONS, G. H. Improvement of the Sugar Beet. *U. S. Dept. of Agr. Yearbook 1936*. 625–56.

COONS, G. H. The Development of Domestic Sugar-Beet Seed Production. U. S. Dept. Agr., ARA, *Research Achievement Sheet 47 (P)*. Nov. 9, 1945, 2 pp.

LIPPMAN, E. O. VON. *Geschichte der Rube (Beta) als Kulturpflanze*. Berlin: 1925, 184 pp.

MARGGRAF, A. S. Chymische Versuche, einen wahren Zucker aus verschiedenen Pflanzen, die in unseren Landern wachsen, zu ziehen. In his *Chymischen Schriften*. Theil 2. Berlin. (Reprinted in Ostwald's *Klassiker der exakten Wissenschaften No. 159*. Leipzig: 1907, 72 pp.)

OWEN, F. V. Cytoplasmically Inherited Male Sterility in Sugar Beets. *J. Agr. Res.* 1943, 71, 423–40.

STEWART, D., GASKILL, J. O., and COONS, G. H. Heterosis in Sugar Beet Single Crosses. *Proc. Amer. Soc. Sugar Beet Tech. 1946*. 1947, 210–22.

STEWART, D., LAVIS, C. A., and COONS, G. H. Hybrid Vigor in Sugar Beets. *J. Agr. Res.* 1940, 60, 715–38

TAYLOR, F. G. *A Saga of Sugar*. Salt Lake City: Utah-Idaho Sugar Co., 1944, 234 pp.

THE GENETIC APPROACH TO HUMAN INDIVIDUALITY

LAURENCE H. SNYDER

Dean of the Graduate College, University of Oklahoma, Dr. Snyder (D Sc., Harvard, 1926) is well known for his work in genetics. He has taught at Harvard, North Carolina State College, Ohio State, and at Duke, and has participated in research studies of the Carnegie Institution and the National Research Council. His article is based on the paper he presented in the AAAS Centennial symposium on "Human Individuality" on September 14, 1948, Washington, D. C.

NO TWO human individuals are exactly alike. Variability is as much of a characteristic of human beings today as it is of all other species, and has been of living things throughout evolutionary history. Although no two persons are exactly alike, all human beings possess certain traits in common, and show certain similarities. The similarities and differences observable and measurable among people are the results of the interaction of genetic and environmental influences. It is becoming apparent that only modern genetic methods can successfully distinguish between the relative contributions of these interacting influences in bringing about human traits, and that the proportionate extent of these contributions will differ from trait to trait.

Human beings are most similar in those characters they share with the greatest variety of other animals, and differ from each other more and more as they get farther from these basic deep-seated traits and into the more superficial characters. We are chordates and we all have at an early stage in development a chorda, dorsal to the alimentary tract. We must assume that this is a hereditary trait, but we cannot specify the genes that result in the presence of a chorda, since we have never been able successfully to cross a chordate with a nonchordate and thereby to analyze the genes involved.

The fundamental chordate pattern is a remarkably constant characteristic of human beings. This may be due to the fact that whatever genes are involved in the production of this pattern are exceedingly stable and develop in relatively constant environmental surroundings, or it may very well be that any mutation interfering with this fundamental pattern is lethal. Living organisms, as a result of long ages of mutation and selection, have become highly adapted and delicately adjusted mechanisms, and fortuitous mutations, except those involving superficial characters, are apt to be, to say the least, harmful. Your watch is also a delicately adjusted mechanism. A fortuitous mutation might be likened to the result of jabbing an ice pick into a watch. If you jabbed enough ice picks into enough watches, you might come up with a revolutionary improvement in watchmaking, but the usual result would be to interfere with the normal working, or to stop the watch altogether.

Similarly, we are vertebrates and possess an ossified skeleton, much of which was preformed in cartilage. Mutations that prevented the appearance or development of the skeleton would most likely be lethal, but minor viable variations can and do occur, and these variations interfere more or less with normal functioning. More than one hundred mutations involving the skeleton are known in man, variously affecting the vertebral column, sternum and ribs, the skull, the pelvic and pectoral girdles, the long bones, and the bones of the wrist, hand, ankle, and foot.

Our mammalian characters of hair, mammary glands, and control of body temperature are somewhat less constant. Various mutations result in complete absence of hair, in excessive growth of hair, in persistence of lanugo hair, in complete absence of sweat glands, and in variations in the number, position, and functioning of the mammary glands. Here a mutational change can often be compensated for by man's ingenuity in controlling his environment. Thus we provide ourselves with varying degrees of clothing, with various hormone preparations, and we successfully feed and rear infants in the absence of human milk.

As we consider those traits that distinguish man taxonomically, we find such characters as the upright position, binocular vision, the opposable thumb, the development of the speech areas and the neopallium, and the loss of the external tail and body hair. Although mankind as a whole could probably not have developed to its present position without these traits, individual human beings can exist, with the aid of fellow-men, without one or more of these characters. Hence we find in any

human population today considerable variability in these attributes.

Within the species *Homo sapiens* there are many variations known to have a genetic basis. Through geographic or social isolation, and the concomitant effects of inbreeding, genetic drift, and selection, a number of populations have become more or less differentiated one from the other in respect to one or several of the more readily recognizable of these variations, such as skin color, hair form, or stature. Such physically differentiable populations have been variously referred to as races, strains, peoples, or ethnic groups. It should be noted, however, that from a genetic standpoint, these groupings have little resemblance to the races or stocks of the laboratory geneticist. Through systematic and intense inbreeding, the usual laboratory race has been made very nearly homogeneous in respect to its entire genotype. Whatever homogeneous human races may once have existed have been largely obliterated as such by migration, with consequent interbreeding and genetic segregation.

Within any existing population of human beings there are genetic and environmental variations of so many and diverse sorts that each human individual is unique. These variations have been the subject of study by many groups of investigators: philosophers, physicians, anthropologists, psychologists, biochemists, and geneticists. With the development of modern scientific methods fruitful results are rapidly accumulating regarding human individuality.

The twentieth century has witnessed the remarkable development of the understanding of the principles of heredity. This development includes not only the classical principles of the transmission of chromosomes and genes, but their physiological and biochemical activities, their evolutionary history, their distribution and behavior in populations, and the social, medical, and legal implications of many of the traits resulting from them. The understanding of these principles has given us the first clear insight into human individuality.

Based on the studies of salivary-gland chromosomes in *Drosophila*, a reasonable estimate of the minimum number of pairs of genes on a pair of chromosomes would be 500. With 24 pairs of chromosomes, we could reasonably assume a minimum of 12,000 pairs of genes in man. Given a mutation at each of only 200 of these loci, the number of possible phenotypically different combinations would be 2^{200}, or approximately 1 followed by 60 zeros, even if dominance were complete in all instances. If dominance should not be complete, so that the heterozygote were phenotypically recognizable, or if more than one mutation has occurred at a locus, resulting in multiple alleles, the number of distinct phenotypes that could result from combinations of genes at 200 loci might well exceed the staggering total of 3^{200}, or approximately 1 followed by 143 zeros. Either of these numbers far exceeds the number of human individuals who have ever lived on the earth. Evidence is accumulating that many human genes long supposed to show dominance are actually recognizable in the heterozygous state.

Mutations are now known in man at far more than 200 loci. True, the mutant gene of the pair is often rare in comparison with the original gene, so that certain combinations of mutant genes will be exceedingly rare, but through the workings of Mendelian heredity all the various combinations are potentially possible, and many of them will occur with reasonable frequency.

I

Let us examine some of the better-known mutations and assay their contributions in producing human individuality. One of the first pieces of research undertaken in my laboratory more than twenty-five years ago was the analysis of the inheritance and distribution in populations of the blood groups. At that time we knew only four blood groups, and we were able to confirm the hypothesis that their inheritance was on the basis of a set of three multiple alleles. Today, because of the researches of workers in many laboratories all over the world, we know more than two million blood groups, dependent upon ten or more sets of alleles. The inheritance of the groups is rather well known. The blood groups have been described in detail on many other occasions, and I shall do no more than list them here (Table 1).

TABLE 1

THE KNOWN HUMAN BLOOD GROUPS

BLOOD GROUP SYSTEMS	NUMBER OF PHENOTYPES
O, A^1, A^2, A^3, B, A^1B, A^2B, A^3B	8
Secretor, non-secretor	2
M, MS, N^1, N^2, N^1S, N^2S, MN^1, MN^2, MN^1S, MN^2S	10
P^1, P^2, P^1P^2, P–	4
C, C^u, C^w, c^v, c, CC^u, CC^w, Cc^v, Cc, C^uC^w, C^uc^v, C^uc, C^wc^v, C^wc, c^vc	15
D, D^u, d, DD^u, Dd, D^ud	6
E, Ee, e	3
Kell +, Kell –	2
Lewis +, Lewis –	2
Lutheran +, Lutheran –	2
Levay +, Levay –	2

$$8 \times 2 \times 10 \times 4 \times 15 \times 6 \times 3 \times 2 \times 2 \times 2 \times 2 = 2{,}764{,}800$$

Some of the groups are common in the populations, others rare. Moreover, the proportions of the various groups differ from population to population. The antiserums for determining the groups are not all equally readily available, and probably no one laboratory has ever had all the antiserums available at any one time. Nevertheless, it would be possible for me to take a drop of blood from each of hundreds of individuals and, with the proper antiserums, to classify each person in one or another of these many blood groups. It would be surprising if any two of them fell into the same groups. Then, ten years from now we could all meet, say, in a reunion, and I could again take a drop of blood from each person. Without labeling the samples at all, and merely by referring to my previous list, I could identify the blood of each person, except for such duplications of type as might occur.

Another source of human individuality lies in the enzymes and enzyme systems that characterize us. Modern biochemical research indicates that metabolism proceeds by a series of chemical steps, each activated by a specific enzyme. All these enzymes are, as a rule, produced by each individual. Now and then an individual is found who fails to produce a particular enzyme, and as a result some metabolic process is interfered with at that point. The outcome may be the excretion of some unusual metabolite in the urine, or it may be evidenced by lack of pigmentation in the hair or skin, or by physical or mental peculiarity. In such instances the failure to produce the enzyme has been shown to be the result of a single gene substitution. Ancillary investigations on *Neurospora* are providing evidence of a one-to-one correspondence between gene and enzyme; and, in fact, this one-to-one correspondence may very well apply equally to gene and antigen and to gene and hormone.

II

Let us consider four human traits that at first glance seem to have no close connections with one another, but which turn out to be very closely related chemically. The four traits are albinism, in which there is an absence of pigment in skin and hair; phenylketonuria, in which there is severe mental defect accompanied by the daily excretion of about a gram of phenylpyruvic acid in the urine; tyrosinosis, in which tyrosine is excreted in the urine; and alcaptonuria, in which the urine turns black on exposure, because of the excretion of homogentisic acid, and pigment deposits are sometimes made in the cartilages and joints.

Each of these conditions appears to be due to a separate recessive gene. A long series of biochemical investigations shows that they are all intimately related to the metabolism of phenylalanine. Each recessive gene results in the failure to produce a specific enzyme which is produced by its normal allele, and when the mutant gene is present in homozygous form the metabolic process is stopped at that point, often with the excretion of the blocked product into the urine (Table 2). In the table, *c* represents the recessive gene for albinism, which, when present in double dose, fails to produce the enzyme formed by its normal allele and necessary to build melanin from its precursor, "dopa;" *k* represents the mutant gene for phenylketonuria, which, in homozygous form, fails to produce the enzyme formed by its normal allele and necessary for the degradation of phenylpyruvic acid; similarly, *t* represents the gene for tyrosinosis, and *a* the gene for alcaptonuria.

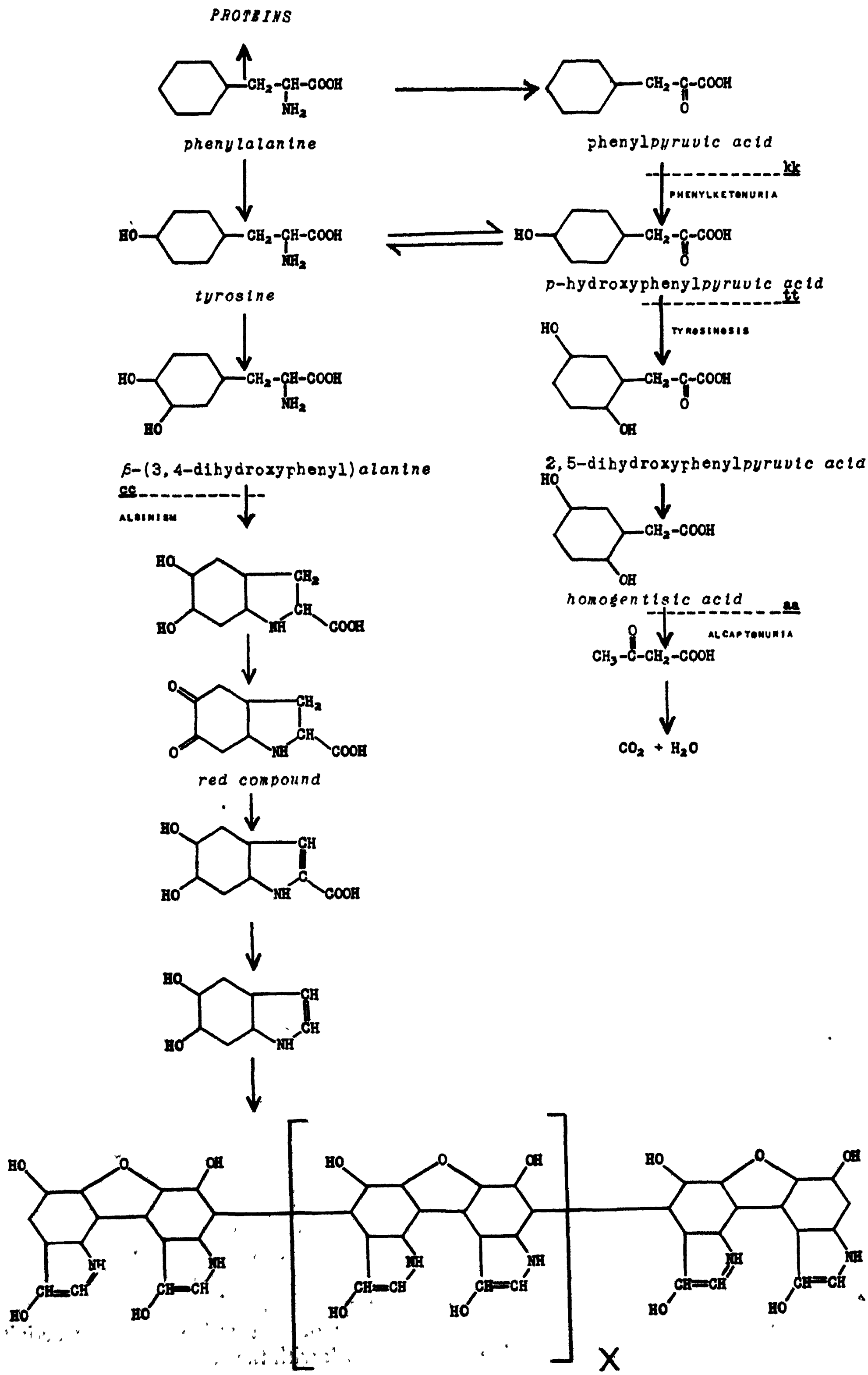
Thus these four metabolic derangements are seen to be chemically related and intimately bound up with the metabolic processes of the body. In a similar way glycogen disease, also due to a recessive gene, appears to be brought about as a result of the failure of this gene to produce the normal enzyme phosphorylase necessary to degrade glycogen into glucose-1-phosphate and so to glucose.

Human individuals show many other genetic variations in normal metabolic processes, many of them resulting in severe diseases. The detailed chemistry has not been worked out for all of them. Such inherited metabolic derangements as amaurotic idiocy, gargoylism, Gaucher's anemia, Niemann Pick's disease, xanthoma tuberosum, gout, and diabetes offer great possibilities for discovering the detailed chemical activities of genes. It is not too much to hope that someday the genetic basis for the complete metabolic processes of man may be built into a single mosaic. Indeed, the workers in at least one laboratory are already actively searching for variations in the normal metabolic activity of man, with gratifying results. Even at this early stage of the game it seems certain that each individual inherits a unique metabolic pattern, which can be measured and demonstrated.

Since several of these single-gene metabolic derangements, such as phenylketonuria and amaurotic idiocy, involve severe mental defects, it is well within the range of possibility that, through the intensive study of metabolic processes associated with mutant genes, the entire nervous and mental make-up of an individual may someday be specified in terms of measurable metabolic activities. Recent work with the electroencephalograph in meas-

TABLE 2

METABOLISM OF PHENYLALANINE



Melanin

uring cerebral dysrhythmia indicates that this dysfunction is associated with the tendency to convulsive disease and is apparently inherited as a dominant factor. It remains for the detailed metabolism to be worked out.

The recognition of the genetic basis for many of the mental derangements has had to await the development of specific genetic methods. Indeed, the whole modern study of human individuality has required the elaboration of new and special methods. In so far as the analysis concerns genetic variability, the new methods center around two major approaches: gene-frequency analyses and twin methods. The gene-frequency procedures, which I have had occasion to formulate and elaborate on many other occasions, are used in the analysis of those traits in which genetic diversity provides the effective variable, and environmental diversity produces relatively little effect. The twin methods, which are becoming more and more essential in studies of human individuality, are used primarily in the investigation of those traits in the production of which there is reason to suppose that environment acts as an effective variable.

It has long been realized that there are two types of human twins: monozygotic, arising from a single fertilized ovum, which early in development has split into two parts; and dizygotic, resulting from two separate ova, which have matured and have been fertilized at the same time. The members of a pair of monozygotic twins are always of the same sex, and carry identical complements of genes. Differences between them must be attributed to environmental influences, including prenatal. Dizygotic twins, on the other hand, may be like-sexed or opposite-sexed, and the gene complements of the members of a pair will be in general of the same degree of similarity as those of ordinary brothers and sisters.

Many investigators have used the comparison of the two types of twins as a method of evaluating the relative contributions of heredity and environment in the production of various human traits, both normal and pathological. Recent refinements of the procedures, notably the twin-family method, have provided important evidence for the genetic basis of specific disease entities, physical and mental, as well as of basically uniform patterns in the organization or disorganization of physical and mental capacities essential in effort tolerance, personality integration, and intellectual performance. The twin-family method involves an extension of the comparison of twins to include not only monozygotic and dizygotic pairs, but also their full-siblings, half-siblings, step-siblings, and marriage partners. Long-term, extensive studies of this sort may be expected to provide information of the most important kind on human individuality. The interaction of genetic and environmental factors has already been thoroughly studied by this method in relation to schizophrenia, tuberculosis, and significant variations in intelligence, aging, and longevity, and there can be little doubt of the important part played by variable genetic factors in the development of these phenomena. It has also been indicated by experimental studies of animal behavior that the genetic determination of behavior may extend to very intricate and precise coordinative processes.

Human beings differ in their taste responses to various substances. These responses show a clear dichotomy in some instances, such as the ability or lack of ability to taste phenyl-thio-carbamide or mercapto-benzo-selenozol, and the responses are inherited in a simple manner. In other instances, such as the threshold responses to various substances such as NaCl and HCl, there is continuous gradation and high variability.

A long series of inherited variations in the blood is known, including differences in the serum, and in the shape, size, structure, and function of the various kinds of blood cells. Many of these variations result in clinical manifestations of greater or less severity.

In fact, no system of the body is without its genetic differences. The precise genetic nature of some of them is known, but for others the individual genes concerned remain to be specified. More than 100 mutations are known affecting the development and functioning of the eye, and many genes have been described having effects on the muscles, the nerves, the glands, the skin, the hair, the teeth, and the nails.

III

Man is a gregarious animal and tends to cluster in groups. The analysis of human genetic individuality is essentially a study of population genetics. Although the geneticist does not like the term race, with its implication of essential intraracial similarity and interracial difference, of "superiority" of one race and "inferiority" of another, nevertheless he realizes that human individuals do occur in populations, which may differ one from the other. Within any population there is considerable variability, giving each human being his own individuality. This individuality must be described in terms of the presence or absence of various alleles,

plus the appropriate results of the exigencies of the environment to which the individual has been exposed. In addition to the description of the individual human being, however, the geneticist must describe *populations*. These are best characterized in terms of the relative proportions of the alleles, and of the results of the over-all environmental impacts.

Human populations differ genetically one from the other almost entirely in the varying *proportions* of the alleles of the various sets, and not in the *kinds* of alleles they contain. In all the instances extensively studied, it appears likely that no large population completely lacks any allele, but that one population may have a larger or smaller proportion of a given allele than another.

By special genetic methods it is possible to derive from the proportions of the various phenotypes in any population the relative proportions of the alleles producing these phenotypes. The genetic description of a population would then be the detailed listing of these proportions for the various alleles of all sets that have been identified and analyzed for the population. This type of description is as yet in its infancy for the human race, but its accomplishment is an urgent necessity for the understanding of human individuality. It is gratifying to note that this viewpoint is being adopted by physical anthropologists.

Since the genetic individuality of any human being is a function of the family and of the population to which he belongs, the principles of both Mendelian genetics and population genetics must be taken into account in its analysis. The principles of Mendelian genetics are now household commonplaces, but the principles of population genetics are not so widely understood. I have recently had occasion to organize these principles, and they might well be summarized at this point.

1. In a large population, with negligible or balancing effects of mutation, selection, and genetic drift, the proportions of the alleles of any set will remain constant from generation to generation. Under a system of random mating, the proportions of the genotypes formed by these alleles will likewise remain constant in the equilibrium ratio.

2. Under certain specifiable conditions, mutation and selection can change the proportions of the alleles in a predictable manner.

3. Classical Mendelian ratios are not necessarily to be expected among the pooled offspring of a series of families where the parental mating types are phenotypically identical.

4. Although classical Mendelian ratios are not to be expected in combined family data, nevertheless predictable ratios do occur. These ratios are population ratios, and are expressed in terms of gene proportions. Thus, where a common Mendelian ratio is $\frac{3}{4} : \frac{1}{4}$, an equally common and analogous population ratio is $\frac{1+2q}{(1+q)^2} : \frac{q^2}{(1+q)^2}$, where q represents the proportion of the recessive allele in the population.

5. The analysis of population ratios may serve as a means of estimating the number and kinds of genes in a population, just as the analysis of Mendelian ratios may serve as a means of estimating the number and kinds of genes involved in a laboratory experiment.

In closing let me attempt to illustrate by concrete examples an arrangement of human variations in an order of increasing importance of heredity as an effective variable.

At one extreme of such an arrangement we might place such a trait as the language spoken by an individual. The particular language used, and the accent with which it is spoken, are probably almost entirely due to the environment in which the individual is brought up, and are little if at all affected by his genetic make-up.

Going one step further, I will call to your attention a case recently given me by a physician who had been a student in one of my classes in medical genetics. A man of fifty-one years was admitted to a hospital with symptoms of extreme breathlessness. It was noted that he had clubbed fingers, and this combination of symptoms caused the probability of severe cardiac disease to leap to the minds of the examining physicians. After extensive studies no such pathology could be demonstrated, however. On at least three previous occasions the patient had been told by examining physicians that he had serious heart disease, and on two occasions he was refused lucrative employment following the diagnosis.

Recalling his training in medical genetics, the physician asked the patient if anyone else in his family had clubbed fingers. The patient replied that his father had them, as did two of his five children. The final and correct diagnosis of this case was that the patient had inherited clubbed fingers, and that after being told so often by doctors, on the basis of this trait, that he had serious heart disease, he had developed an almost incapacitating anxiety reaction.

This case is obviously one where the trait is en-

vironmentally conditioned, but results indirectly from the presence of a genetic factor.

Going another step, we may consider a trait that is also environmental, but which results directly from the presence of a genetic factor. I refer to erythroblastosis, or hemolytic disease of the newborn. Here the condition is brought about by a chain of reactions, involving the inheritance in the embryo of an antigen present in the father but lacking in the mother, followed by the immunization of the mother by the antigen of the embryo, and the subsequent effect on the erythrocytes of the fetus, or later similar fetuses, of the maternal immune antibody.

The lines upon the fingertips, which account for much of the popular conception of human individuality, are the result of the interaction of both genetic and environmental influences. The similarity of fingerprints increases as the closeness of blood relationship increases, but the fortuitous stretching of the skin during embryonic development prevents them from ever becoming identical in any two persons. Likewise, many pathological conditions such as diphtheria, tuberculosis, cancer, and schizophrenia, in which both a genetic susceptibility and an environmental impact or infectious agent are required for their manifestation, illustrate traits that are obviously the result of both hereditary and environmental variables. Finally, we may point to such traits as the blood groups or the color of the eyes, in which genetic diversity appears to be the only effective variable, and in the variation of which environmental diversity seems to play no role.

Many and diverse are the modes of interaction between hereditary and environmental influences. And many and diverse are the resulting measurable human traits. The manifold combinations of these traits result in the almost infinite diversity of human individuality, a diversity we are just beginning to comprehend.

MEDICAL RESEARCH

New Betatron

The University of Illinois has set up a 22,000,000-volt betatron for pioneer medical work. X-rays now used in hospitals for treating deep cancers are of 200,000–2,000,000 volts energy. The new betatron may later become standard for deep treatment.

Professor Donald W. Kerst, the betatron's inventor, "using material which absorbs rays equally with a living body, has found that 22-million volt X-rays have their greatest effect approximately 1.5 inches (3–4 cm) inside the surface, and that after passing through 8 inches (20 cm) of the material their energy is only about one-half that of the maximum point. The 8 inches corresponds to an approximate thickness of the human body.

"This means for medicine that surface damage, at points where the ray beam enters and leaves the patient, will be less, and at the same time that the X-ray dosage is concentrated in maximum on the deep internal organs where it is desired."

Dr. Roger A. Harvey, head of the Department of Radiology, is directing preliminary research involving studies on depth dose and isodose distributions, rate and intensity effects, directing, focusing, and monitoring of the beam, and techniques for biological and clinical application.

Radioisotopes

The Veterans Administration has authorized the establishment of a radioisotope unit at the VA Hospital, Nashville, Tennessee. Research work will be directed toward development of improved methods for clinical diagnosis and medical treatment of veteran-patients. Radioisotopes to be used in the preliminary work are radio phosphorus, radiosodium, radioiron, and radioiodine. This is the eleventh such unit established by the Veterans Administration.

DOVAP—A METHOD FOR SURVEYING HIGH-ALTITUDE TRAJECTORIES

DORRIT HOFFLEIT

Dr. Hoffleit (Ph.D., Radcliffe, 1938) worked at the Harvard College Observatory on variable stars, meteors, and especially spectroscopic absolute magnitudes from 1929 until the war called her to the Ballistic Research Laboratories, Aberdeen Proving Ground. She is still serving both institutions. Radcliffe awarded her its Caroline Wilby Prize in 1938 for the "best original work in any department."

AT THE White Sands Proving Ground in New Mexico many sorts of instrumentation are employed for determining the paths of V-2 and other high-altitude, long-range missiles. The instruments used include a variety of optical devices, such as cine-theodolites, tracking telescopes, and ballistic cameras. There are several varieties of theodolites. Some are fixed in orientation and situated only a mile or two from the launcher. On movie film they record the initial part of the flight of the missile with a high degree of positional accuracy. Askania and Mitchell theodolites, using 35-mm film, are tracked on the missile and follow it about 30 miles—sometimes much higher, depending on illumination and atmospheric transparency. Ballistic cameras are fixed cameras operating on the same principle as Harvard's meteor cameras, which give positions and velocities from trails segmented by rotating shutters. A pair of synchronized optical instruments is required for triangulating the position of the missile. Numerous stations are distributed over the range in order to give best results at all available missile positions. Then there are radar devices. One in particular gives the slant range of the missile from the instrument to a moderate accuracy of 50 yards, with corresponding azimuth and elevation angles accurate to a few mils (1 mil = 360°/6400). One station alone can therefore determine an entire trajectory, and radar has the advantage over the optical devices that it can *see* the missile regardless of illumination.

All these types of systems had been much used with shorter-range missiles when the V-2 tests at White Sands were begun. Each has its peculiar advantages and disadvantages. No one system has yet proved "best" for all the information that is wanted on a missile's entire flight path and behavior (including velocity, deceleration, spin, yaw, etc.). Another system, now known as DOVAP (meaning DOppler Velocity And Position), although previously existing on paper, was developed by Ballistic Research Laboratory engineers as a practical instrumentation especially for the V-2 tests and for the future, in a more mature form, for newer, longer-range missiles. This system is of timely interest. It has definitely proved its practical value, yet nearly every record obtained seems to have opened up new vistas for research or engineering improvement. Moreover, in the analysis of the data obtained, new film-measuring and computing devices have either been developed or tested for the first time.

DOVAP uses continuous-wave radio signals to determine missile distances by a Doppler effect. A transmitter on the ground sends these signals simultaneously to a ground receiver station and to the missile. On the nose of the missile is a transceiver which receives the transmitter signals, doubles their frequency, and retransmits the doubled frequency down to the ground receiver. In the ground receiver this signal from the moving missile is mixed with double the frequency received directly from the transmitter. The resulting beat frequencies are recorded as Doppler waves along a time scale. If we count the number of recorded cycles from the instant the missile has left the ground to any specific future moment, t, the number of counted cycles tells us how much the total distance Transmitter-Missile-Receiver (TMR) has changed in that period of time. Let N_1 be the number of counted cycles and λ the Doppler wave length. This is determined from measures of the transmitted frequencies, f-, since $\lambda = c/2f$, where c is the velocity of propagation of radio waves (the velocity of light). Knowing from ground surveys the distance from the transmitter to the launcher (TL), and from the launcher to the receiver (LR), the Doppler information tells us that the distance $\mu_1 = TMR_1 = TL + LR_1 + N_1\lambda$. What this means is that the missile is somewhere on an ellipsoid of revolution (strictly a prolate

spheroid) whose foci are the transmitter and the receiver. If we now install a second receiver somewhere well separated from the first one, we obtain precisely similar information, namely, $\mu_2 = TMR_2 = TL + LR_2 + N_2\lambda$. Hence we know that the missile is somewhere on an ellipse which is the intersection of two prolate spheroids having one focus, the transmitter, in common. Finally, if we introduce a third receiver, served by the same transmitter, we find two points which satisfy all three conditions μ_1, μ_2, μ_3. There will be no trouble deciding which solution is the desired one—the extra solution is usually underground.

In practice, four receiver stations are actually used at White Sands, arranged roughly at the corners of a diamond, the transmitter and one of the receivers being close together and about 2 miles south of the launcher (Fig. 1). The distances to the other stations are about 14 miles. Four stations are used partly as precaution in case any one of the stations should fail, partly to enable us to evaluate the accuracy of the results. The Doppler signals obtained at the four receiver stations are all transmitted by ground wire to a common recording station, where they are recorded simultaneously, side by side on 35-mm movie film, together with the timing record (Fig. 2). As the transmitted frequency (38.5 mc) corresponds to a Doppler wave length of about 12.5 feet, and the time scale is accurate to better than 1 part in 100,000, it would appear at first sight that the distances μ_i, determined from counts of cycles (such as illustrated), might be accurate to within a foot. Actually, most of the records suffer blemishes, due, for example, to peculiarities in missile behavior, interference of radio waves, and atmospheric effects. Usually errors in the μ_i amount to about 50 feet; at high altitudes the errors may be much larger if refraction in the ionosphere is disregarded. Yet at 100 miles overhead absolute errors of even a few hundred feet still surpass in accuracy results from any of the other present types of position determinations. An average error of 100 feet in Transmitter-Missile-Receiver distances will intro-

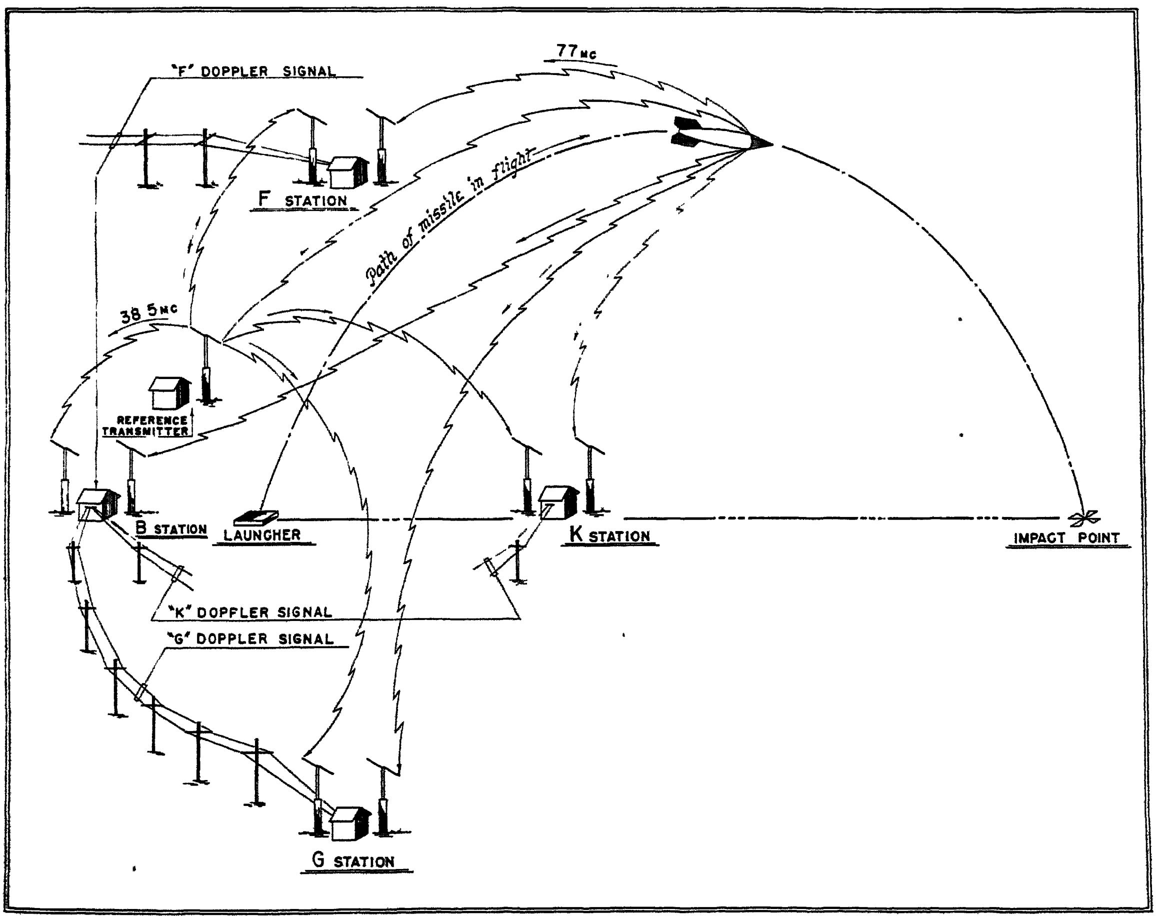


FIG. 1. Typical DOVAP Field Setup.

duce errors of under a mile in the horizontal component of the range and not over a few hundred feet in height. Moreover, improvements both in the instrumentation and data analysis are actively progressing.

The complete reductions of DOVAP data on any one round are apt to require a matter of several weeks of film reading, interpretation, and computation. There is, however, a quick method that has been used successfully for determining a few critical points on the trajectory within a few minutes after firing. The Doppler frequencies are recorded directly with an Esterline-Angus Recorder, giving a trace such as shown in Figure 3. The abscissa on this curve is time; the ordinate, frequency which is proportional to velocity. In the case of B-station receiver, which is close to the transmitter in comparison with the distances to the missile, we can assume that the recorded frequency is directly proportional to the radial velocity of the missile toward or away from the receiver. With the aid of the schematic diagrams in Figure 4 we can interpret the Esterline-Angus record. The upper diagram is a hypothetical V-2 trajectory. B-station and the transmitter are 2 miles behind the launcher at about the same height. The missile rises vertically so that initially (A) the radial component of its velocity as seen from B-station is zero. At fuel cutoff (B), at an altitude of about 20 miles, the missile has been guided so that its trajectory is nearly tangent from B-station. The radial velocity is consequently a maximum for two reasons: the missile's speed is a maximum, and the angle between the line of sight and the direction of motion is a minimum. Shortly after maximum altitude (C) the trajectory is perpendicular to the line of sight and the radial velocity is zero. On the continued downward path the radial velocity again increases (to D), but usually it does not reach as high a value as on the ascending branch because the line of sight is nowhere tangent to the path. A second null point (E) may, however, occur prior to impact (F). The lower diagram is

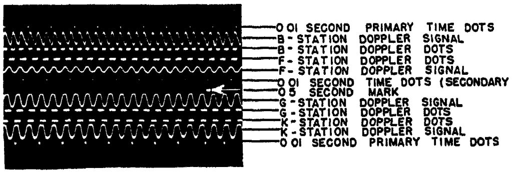


FIG. 2 Representative segment of DOVAP record.

thus a schematic Esterline-Angus record, which gives rate of change of distance.

If we take a planimeter and trace the area under the first hump of the record, from A through C, we have the complete change of distance from launching to the point slightly after maximum altitude. Since the V-2 trajectories are very steep, and B-station comparatively close to the launcher, it can be assumed within 5 percent that this net change of distance is the maximum altitude. The second hump of the tracing, from C to E, represents a *decrease* in the distance of the missile; the small final hump represents another increase. Thus, if we subtract the second area from the first and add the third, we have the difference in distance from launching to impact—the range.

From data from B-station alone we can in this manner get good estimates of the maximum velocity, maximum altitude, and range. The results obtained by the press right after a "shoot" have usually been obtained as described. With just a little more effort an approximate direction to the impact area may also be obtained.

The Esterline-Angus records for the other, outlying, stations give frequencies that are proportional not to radial velocities from the station, but to the sum of the radial velocities from the transmitter and the receiver station. To get radial velocities from the receiver we therefore simply subtract half the frequencies observed at B-station. Then we obtain as before the impact ranges from all four of the stations, defining the impact area by the intersections of four circles whose radii are the ranges and whose centers are the station posi-

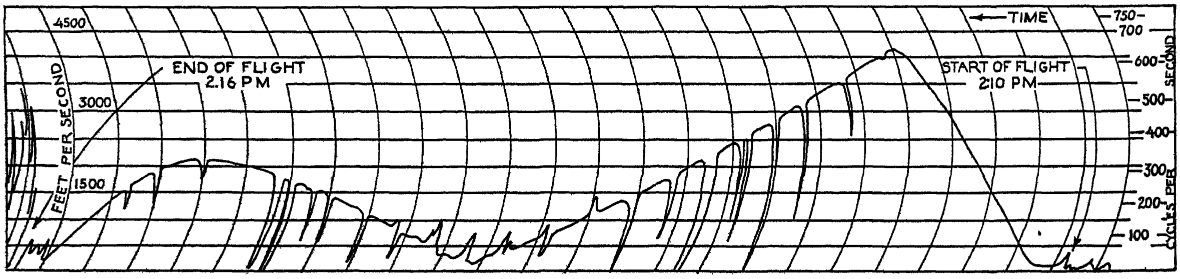


FIG. 3. Doppler frequencies registered directly with an Esterline-Angus Recorder.

tions. This area determined from three stations is about 2 × 4 miles.

These planimeter readings of Esterline-Angus records are gratifying for the speed with which they yield order-of-magnitude results. The detailed reductions of the DOVAP records themselves may be very time-consuming, even in comparison with the reductions of photographic measurements. If a missile's flight is successfully recorded from take-off through maximum altitude to warhead blowoff, or impact, we may have nearly a thousand feet of film to examine. If the maximum altitude reached was about 100 miles, approximately 40,000 cycles would have been recorded for the ascending branch of the trajectory for each of the four stations, and about the same number for the descending branch. Thus counts of more than 300,000 cycles would be made. Until recently all such counts were made purely by eye (with the aid of dividers). Counts were recorded for half-second intervals of flight time so that the smoothness of second differences might readily reveal mistakes. Even at such close intervals, up to 400 cycles per interval were counted at times near fuel cutoff. Photocell counters were tried but proved too inaccurate. Recently a mechanical counter operating on the stroboscopic principle has been developed and is proving successful.

After the Doppler cycles have been counted, the determination of missile coordinates requires some forty arithmetic operations for each point on the trajectory. A skilled computer using desk machines (Friden, Monroe, or Marchant) requires 15–45 minutes per point. A complete trajectory at half-second intervals would therefore require about 400 hours, or 10 weeks of working time.

The problem was first ready for numerical solution about the time that International Business Machines Corporation (IBM) had tested and delivered its new relay multipliers to Aberdeen. The estimated time for 800 points, including time of machine failure, was about 4 weeks on a 40-hour week basis. Now, two such machine units have been wired in tandem, and the problem is expected to take 5–8 minutes per point, or about 2 weeks. This estimate for the new procedure includes the time for complete checks on the accuracy of the computations.

The DOVAP problem was also among the first to be tackled by the ENIAC, which won great acclaim for its speed in the solution. The ENIAC requires about 2 days to get the machine set up for the specific problem. Thereafter (unless other problems intervene) it computes the coordinates in a time comparable to the time of flight of the missile itself: a 10-minute trajectory at half-second intervals in 10 minutes.

The Bell relay machines designed by Stibitz cannot boast this speed, but they do produce the results at a rate of 5 minutes per point without any significant preparatory set-up time. Moreover, they can run unattended all night. Thus their working day is 24 hours in comparison with 8 for the attendants. Hence, they could complete the 800-point trajectory in 3 days. If we have exactly one trajectory to reduce, these machines are proved highly efficient—comparable with the ENIAC. On the other hand, if we have data on 10 rounds at once the ENIAC would require the 2 days preparatory time plus only 100 minutes for all 10 trajectories, or a little more than 2 days.

Thus the DOVAP problem has automatically yielded practical intercomparisons on various high-speed computing devices demonstrating when a relatively slow but highly flexible machine is to be preferred over a higher-speed unit that requires much preparation. Our conclusions, however, reached a year ago, are already obsolescent; in the meanwhile, both the IBM relay multipliers and the ENIAC have undergone subsequent develop-

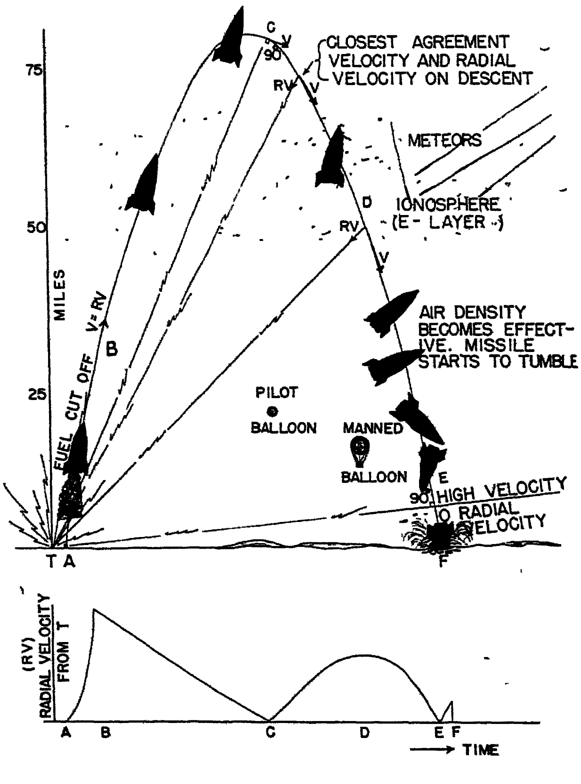


FIG. 4. Schematic diagrams interpret the Esterline-Angus record.

ment to reduce the "get-ready" time in the ENIAC and to increase the operating efficiency of the others.

Some of the DOVAP results are of interest. Figure 5 shows an array of trajectories determined prior to 1948. The round launched the night of December 17, 1946, reached the highest altitude, 116 miles, and had, throughout, the best Doppler recording on any round. For some rounds the records have faded out mysteriously at high altitudes, only to come in again, beautifully, after too long a gap for successful interpolation of counts across the gap. Could the ionosphere be responsible? All the other firings, except the round on December 17, were carried out in the daytime when ionization is stronger. We do not know the answer. One round seemed to defeat this attractive hypothesis by fading on the ascending branch of its flight path and reappearing shortly before maximum altitude (in the ionosphere) and returning good signals all the way on its downward path. Investigations are in progress to attempt to correlate high-altitude failures with solar activity.

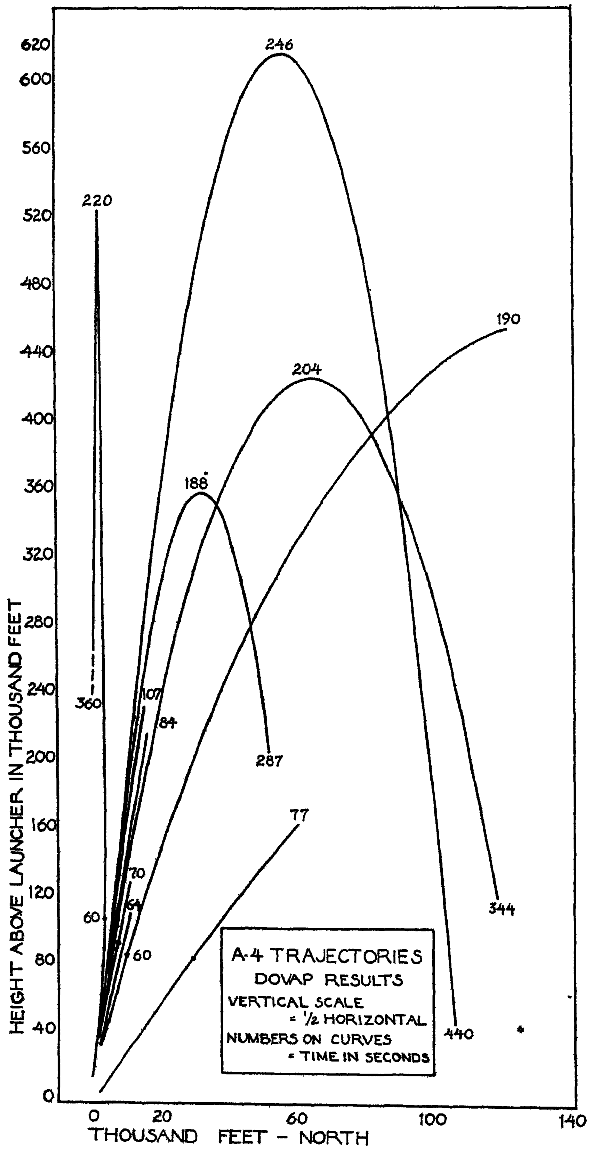


FIG. 5. Trajectories determined prior to 1948.

Sometimes short stretches of fading are found that could be due to the interference between radio waves reaching the missile directly and waves that were first reflected from the ground. But the terrain at White Sands is bumpy, with humps having dimensions comparable with the wave length. Absorption and scattering of the radiation are therefore more apt to be effective than the reflection of radio radiation.

Perhaps the most exhilarating observation was that a spinning missile gave Doppler records with periodic cancellations. Sometimes cycles could still be counted in the low-amplitude cancellation stretches, sometimes the signal loss was complete. Such breaks occurred usually twice per period of spin but sometimes four breaks per rotation occurred. On the basis of direct interpolated counts, it was found that the missile position for such rounds was very badly determined, as compared with optical or radar results. We must apply systematic corrections to the observed frequencies to get quantities that give change of distance directly when multiplied by the wave length. What the corrections should be was a problem in antenna design, supplemented with empirically ascertained correction factors. The missile roll-rate effect gave both pleasure and sorrow when it was first found. It meant we could learn other things from DOVAP about the missile, besides its flight path, velocities, and accelerations. On the other hand, it reduced what should otherwise have been very accurate counts to somewhat unsatisfactory problems in interpolation and interpretation. The engineers are working on the problem, however, to get legible records without cancellations or fadeouts; but the observed frequencies will still have to be corrected for roll rate. Provided the roll rate is not too fast it can still be determined throughout the time of flight from Esterline-Angus field-strength recordings and from tracking telescope records.

Various other types of apparent blemishes have also been observed on some of the records. Some may be due to spontaneous discharge of static picked up by the missile in flight. Some might have arisen from diffraction or reflection from the large,

smooth gypsum dunes at the White Sands National Monument, 25 miles north of the launcher. Here again, scattering and absorption may be the more important. Yet there are fairly regular configurations in the white sands that stimulate interest as possible sources for error in the interpretation of DOVAP records. Then there is the important problem of the effect of refraction of radio waves in the ionosphere. Neglect of this effect should make our high-altitude determinations wrong by several hundred feet. DOVAP is still a young system and loaded with research promise.

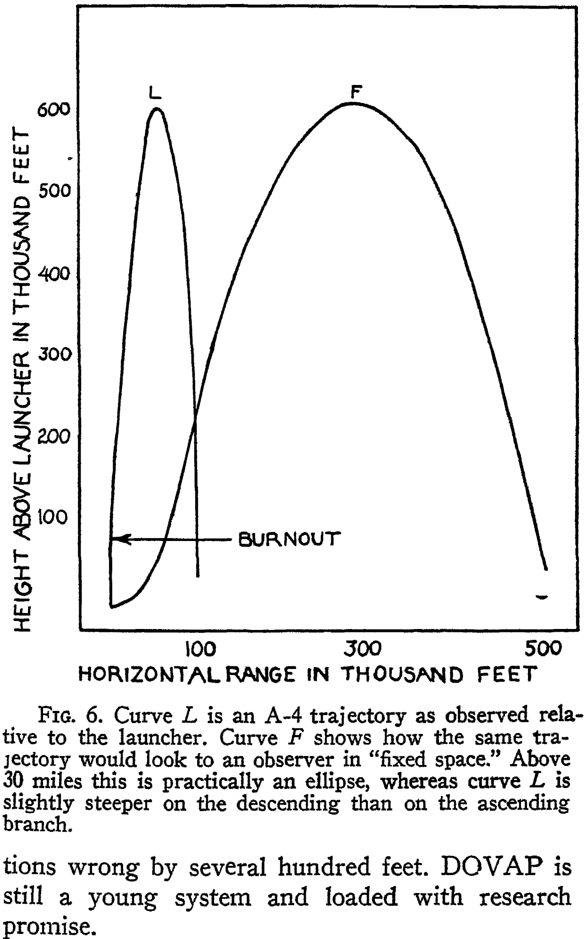


Fig. 6. Curve L is an A-4 trajectory as observed relative to the launcher. Curve F shows how the same trajectory would look to an observer in "fixed space." Above 30 miles this is practically an ellipse, whereas curve L is slightly steeper on the descending than on the ascending branch.

The accuracy of DOVAP-determined trajectories nevertheless compares favorably with results from older instrumentation. Some rounds have given trouble; but with the source of major troubles discovered, higher accuracy in the future seems assured. And this is important, not for V-2s, but for the longer-range missiles or space ships of the future.

From the internal consistency of four DOVAP results obtained by using three at a time of the four field stations, the determinations of altitude have usually been found to agree to much better than 100 feet, and the horizontal components of the slant range have agreed to half a mile at the largest observed distances.

Perhaps a more striking evaluation of the accuracy is found in a comparison between DOVAP and theoretically computed trajectories. For these high-altitude missiles we might assume that atmospheric resistance is negligible above 25 or 30 miles. Then we can compute a "vacuum trajectory" to see how well it fits the DOVAP results. The vacuum trajectory is really a Keplerian orbit; for the simple parabolic approximation found in first-year calculus texts is not adequate when the missile reaches 100-mile distances. Both the direction and the magnitude of the force of gravity vary sufficiently to give significant deviations from a parabolic trajectory. Assuming, then, coordinates and velocities determined by DOVAP for a point some 10 seconds after fuel cut-off, when there can be no question about any residual burning effects, we can compute an elliptical orbit. The coordinates of this orbit are given relative to a system of coordinate axes whose directions are fixed in space (Fig. 6). DOVAP-determined coordinates, on the other hand, are relative to the launcher, which is fixed on the surface of the earth but is therefore

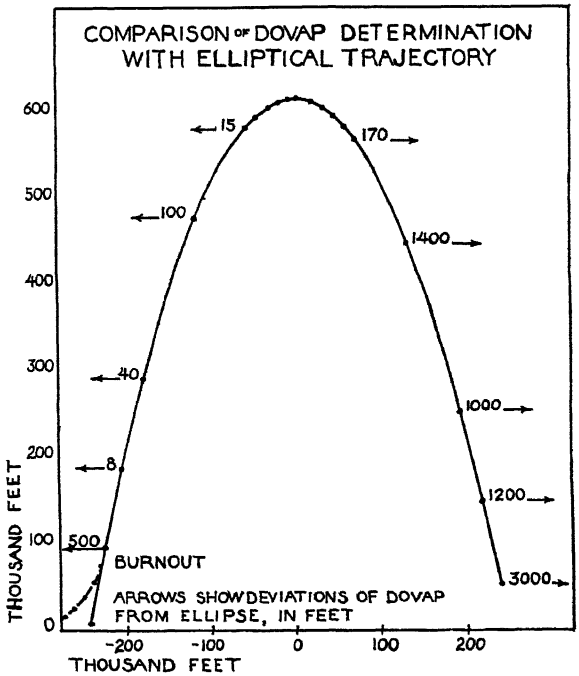


Fig. 7. The curve is an elliptical computed trajectory that depends on the observed trajectory data some ten seconds after burnout. The numbers and arrows show how well the complete observed trajectory agrees with the computed ellipse.

carried along an arc by the earth's rotation. (The earth's orbital motion we can still neglect!) Hence, for comparisons we must transform one system into the coordinate frame of the other (Fig. 7). Preferring the elliptical shape, we kept the fixed-space system and found the comparison shown in the last figure. The maximum altitudes, or maximum distances from the center of the earth, differed by only 200 feet; the greatest differences in the horizontal coordinate, occurring on the descending branch of the trajectory, amounted to approximately half a mile in forty. Other rounds gave comparable results; for poor DOVAP records the maximum altitude errors were of the order of a thousand feet.

As the discrepancies between observed and computed trajectories are scarcely greater than the inherent uncertainties of the reductions themselves, the comparisons suggest that accurate DOVAP reductions are essential only for the first 40-odd miles of travel, or to at least some 10 seconds after burnout. From the conditions observed at such points, computed trajectories should indicate the range to impact to the nearest mile. As the A-4 trajectories are very steep, the neglect of air resistance will not greatly influence the determination of the *horizontal* coordinate (though the drag could also readily be taken into consideration). On the other hand, the excellent agreement found in this analysis stimulates effort toward greater observational accuracy in order that DOVAP may develop into a more effective research tool—for example, for checking the properties of radio propagation, refraction, and other systematic physical effects.

FROM A MOUNTAINTOP

(IN AFTER YEARS)

JAMES G. HODGSON

I

Down from our heights were the "Plains of Man,"
Low in the foreground, high as they ran
Out there beyond where the sky began.

Rivers there were, with an upward tilt,
Winding their way in abandoned silt,
Wide in the basins that man had built;

Signs of the struggle that man had made
Taming the waters! The price he had paid?
How may we know how the balance weighed?

II

Here from the heights was the past in view,
Spanning the time when the dams were new,
Back to the days when the men were few.

FIG. 5. Traj

Green was the ground as it lay below,
Lush at the distance, the winter's snow
Lives on the plains as the grasses grow.

Strong waxed the heat as the summer passed,
Dry was the earth, but the plants stood fast,
Holding the soil where their roots were massed.

III

Came then the men who would plow and sow,
Treating the grass as a common foe,
Baring the earth for the winds to blow.

Folly was theirs! Like a robber band
Sternly they reaped with a heavy hand,
Only to starve what they loved—the land.

Poor was the earth when the rains came not,
Poor was the man who received his lot—
Such of the soil as the wind forgot.

IV

Water! They wailed—and the Engineer
Throttled the streams, both far and near,
Bringing back life with a ditch and weir.

Great were the dams, and the world with awe
Marveled and cheered at the thing it saw
Sucking its wealth down a greedy maw.

Slowly the streams, with their loads of silt,
Twisted and turned where their burdens spilt,
Clogging the basins that man had built.

V

Down from the heights are the plains where man
Labored and toiled, when the waters ran
Deep in the ditches where farms began.

Gone are the fields where the plow bit deep.
Only the ghosts of the lost crops weep,
Waiting alone where the grass roots creep.

Low in the valley the bright streams run
Up toward the sky, but the timeless sun
Sees but the close of a cycle—done.

EFFECTS OF CERTAIN ANIMALS THAT LIVE IN SOILS

JAMES THORP

Located at the University of Nebraska, Professor Thorp is Regional Inspector of Soil Surveys for the Great Plains, Division of Soils, USDA. He has taught in Puerto Rico, in China (at Nanking University), and at Earlham College, his alma mater.

IN discussing the biologic factor of soil formation, textbook authors are accustomed to lay great emphasis on vegetation as it affects soil profile development and to give less space to the influence of animal populations in soils. During many years of field work I have been repeatedly impressed by the extent and magnitude of the modifications of soil profiles accomplished by animals that live in the soil. The effects superficially most noticeable are those brought about by the larger animals, such as burrowing rodents and carnivores; but in many places the smaller animals, such as worms, insects, spiders, crustaceans, and myriapods have made very marked changes in soils and have affected their characteristics more fundamentally than have the larger animals. Animals that live in the soil range in size from minute protozoa, visible only under the microscope, to burrowing mammals of quite large size (wolves, badgers, and large rodents). No attempt will be made to discuss in detail the effects of protozoa on the conversion of raw organic matter into humus, and, of course, effects of these animals on the microstructure of soils. Some conception of the importance of the microbial population (both protozoa and microscopic plants) may be seen in F. Garbrecht's note entitled *¿Cuanto pesan los microbios de la tierra?* ("How much do the soil microbes weigh?") He estimates a total weight of 2,000 grams per square meter of cultivated soil to a depth of 20 centimeters, or a little less than 18,000 pounds (9 tons) per acre in the topmost 8 inches. Of course, the weight of the microbial population will vary greatly with the kind of soil and other factors.

The importance of microscopic life is also brought out by A. G. Norman, who states that a substantial portion of the humified organic matter in soils is composed of the dead remains of microorganisms that feed on the residues of higher forms of life.

Among the most important visible animals that affect soil profile development are very small insects, such as springtails, mites, and celaphids that

FIG. 5. Traj raw humus of the podzol soils; ants and termites, very active in many soils of the world; many small crustaceans, such as wood lice, or sow bugs; and millipedes, centipedes, and various kinds of spiders. Springtails and other minute insects consume the raw humus of the forests and convert it into new types of humus, the details of which have been covered by other workers (T. H. Eaton, Jr., and R. F. Chandler, Jr.) and need not be discussed here.

EARTHWORMS

In forested and cultivated soils, especially in soils of medium to heavy texture and where vegetation is of a type that is appetizing to them, earthworms are very active in converting raw vegetable matter to humus and in mixing the humus with the mineral portion of the soil. In many forested areas, and also in some grasslands, the superficial several inches of the soil consist almost entirely of earthworm castings that have given the soil a characteristic granular or crumb structure sometimes called "earthworm mull." This does not imply that all granular and crumb structures are due to the action of earthworms or other animals, but it is true that much soil structure originates in this way. In forested soils of some areas, especially in those of medium to clayey textures, it is conservatively estimated that 500–2,500 tons per acre of soil have been modified in structure and organic content by earthworms alone. Darwin estimated deposits of earthworm castings at rates of 7.5–18 tons per acre per year. A. C. Evans and W. J. Guild found up to 11.5 tons of castings per acre per year in pasture land, clay-with-flints soils, at Rothamsted Experimental Station, England. The same authors quote C. Beaugé as finding 107 tons of castings per acre deposited in the valley of the White Nile during one six-month rainy season.

In the spring of 1919, the earthworms were so abundant in the rendzina soils of the military parade grounds at Dôle du Jura in eastern France that the ground was almost covered with fresh castings. When soils were saturated following heavy rains, the worms were forced to the surface

by the water and could be heard withdrawing hastily into their burrows as one walked across the field.

Earthworms not only mix mineral matter with humus at the surface, but they also carry organic matter deep into the subsoil horizons and parent material when they retreat downward with the moisture during dry weather. Incidental to carrying organic matter to deep horizons and fresh minerals to surface horizons, they leave tunnels behind them that facilitate movement of water through the soils. In the course of studying cross sections of soils, I have seen water drip from earthworm burrows 2–3 feet below the surface when a pit was dug in Miami loam in southern Michigan soon after a rainstorm. Earthworm tunnels become coated with organic and mineral colloids and in many places provide passage space for roots through soil materials that are otherwise rather dense.

Earthworms have interesting methods of collecting food in forested areas. Apparently, Darwin was the first of recent times to discuss this subject in detail, and considerable work has been done still more recently on the Harvard and Yale forests in New England, in Ohio State University, and in England and other parts of Europe. These studies show that earthworms like to concentrate their activities in areas where plants are to their liking. For example, they like especially well the fallen leaves of ash, hickory, tulip tree, dogwood, large-toothed aspen, and several other species. Some kinds of earthworms go out during the night to gather up their favorite kinds of leaves and drag them to the mouths of their tunnels. They pull the leaves partially into the tunnels or heap them up around the tunnel mouths to form "earthworm middens" composed of petioles and other leaf fragments mixed with the feces of the earthworms (Fig. 1). In the early stages of development, middens comprise chiefly mounds of entire leaves and other plant remains.

Marked differences in organic content and structure of upper horizons of soils developed under hardwood cover versus those developed under coniferous forest are ascribed in part by P. R. Gast to differences in kind and degree of activity of the soil fauna. Referring to J. W. Johnston, he makes this statement relative to the preferences of earthworms for different kinds of leaves:

FIG. 1. *Left:* Earthworm middens on stony brown forest soil in the Yale Forest. Note tangled petioles heaped around burrows between rocks. Pencil, lower right, gives the scale. *Right:* Middens composed of earthworm feces and petioles of sugar maple, ash, tulip poplar, and linden leaves. Fox silt loam, deep phase, a gray-brown podzolic soil, at Earlham College, Richmond, Ind.

> Of importance here are the observations by Johnston on the food preferences of the angleworm. He found that of six species studied in laboratory feeding experiments, the large-toothed aspen, white ash, and basswood were accepted immediately in that order. Sugar maple and red maple were taken less avidly; the latter was not entirely consumed. Red oak was not eaten.

It is indicated further that white-pine needles, in the field tests, were only 30 percent decomposed two seasons after falling, the assumption being that the earthworms probably refused them as food, and that other biotic factors were responsible for their slow disappearance.

B. G. Griffith and others found earthworms were always associated with soils of good tilth under hardwood forest cover and were generally absent in the soils of pine forests. Exception noted was in a twenty-year-old forest where the soil was formerly well cultivated.

Abundant earthworm middens were observed at Quaker Hill and at Earlham College (Fig. 1), Richmond, Indiana, in the autumn of 1941 and again in June 1947. They were especially plentiful under ash and sugar-maple trees on Fox silt loam, deep phase, that had once been cultivated. Under one large maple tree, at Quaker Hill in June 1947, my nephew and I counted 27–35 middens per square yard, or roughly 145,000 per acre. The middens were about ¾ by weight of worm feces and about ¼ by weight of maple-leaf petioles and grass stems. The total moist material in the middens from one square yard weighed 3.25 pounds—a rate of more than 15,500 pounds per acre. This material represented the work of the worms in late fall 1946 and spring 1947. In this period of about eight months the worms could have been active for no more than five months.

In Lewis Woods, a remnant of virgin hardwood forest in Wayne County, Indiana, I. C. Brown and I noted in 1939 that the Miami silt loam there has a crumb-mull horizon, 1–4 inches thick, made up largely of earthworm castings and feces of other small invertebrates. Organic matter was high in this layer (6.2 percent), and the reaction was only slightly acid. Assuming an average thickness of 2 inches, this mull horizon represents about 56,000 pounds (28 tons) per acre total, of which about 3,500 pounds (1.75 tons) is humified organic matter, totally reworked by earthworms and their associates. This topmost 2 inches is only a fraction of the total soil in the profile that has been permeated and mixed by earthworms.

It is not necessary to assume that the worms have added any new material to the soil in this instance; but it is obvious that they have facilitated the conversion of raw organic matter to humus

FIG. 5. Tra[illegible] raw [illegible]

and have been instrumental in mixing the humus with soil minerals. Homer Hopp and H. T. Hopkins have demonstrated that earthworms add little or no new plant nutrients to the soil in which they live and grow; but their dead bodies, if added in large quantities to infertile soils, will nourish the plants in these soils.

In an oak-hickory forest of northern Indiana I once noticed that the earthworms had selected hickory leaves to the almost complete exclusion of oak leaves. In June or early July practically all the dead leaves remaining scattered evenly over the surface in this forest were of oak; the earthworm middens were made up almost entirely of petioles and other remnants of hickory leaves mixed with worm feces. The worms have a similar appetite for ash, dogwood, and some other species. Certain earthworms will eat oak, pine, and other leaves, but generally are not very abundant in the dense oak and pine forests of eastern United States.

There is evidence, not yet fully conclusive, that the palatability of vegetation to earthworms depends somewhat on the mineralogical composition of the soils. Well-nourished trees seem to have more palatable leaves than undernourished ones of the same species.

Earthworms are also very active in some prairie and chernozem soils, where they play an important part in mixing organic and mineral soil constituents and in the development of granular or crumb structure. Even in desert regions they soon become abundant when moisture conditions and food supply are improved by irrigation.

Earthworms do not always improve the structure of soils they inhabit, however. For example, A. Leahey, Head of the Dominion Soil Survey Staff of Canada, told me that soils of Alberta and Manitoba were damaged by earthworms that were introduced. Under virgin conditions, the highly fertile chernozem soils of Alberta, Saskatchewan, and Manitoba had no indigenous earthworms. Dr. Leahey made the following statement on the subject in a letter dated November 22, 1948:

> About 1931 I visited a farm north of Edmonton with J D. Newton. While there, the lady of the house asked us how to get rid of earthworms, as they had ruined one garden for her in a matter of two years and were rapidly ruining a second one. Her story was that the earthworms had turned the soil into a sticky, plastic mess. The soil was a Chernozem clay which normally had a good (granular) structure. Earthworms had a deleterious effect on the physical condition of the Chernozem clay soils. Instead of being improved, the granular friable clay broke down into a sticky mass that was extremely difficult to manage after the earthworms had multiplied sufficiently to have a noticeable effect.

R. L. Pendleton has made some observations on the activities of earthworms in certain tropical areas. In Siam, for example, earthworms attain a length of a foot or two and seem to prefer to live in imperfectly drained soils that have been highly leached and are very infertile. In contrast, dense colonization by earthworms in the United States usually is interpreted to be a fair indication of medium to high soil fertility. Pendleton states that the earthworm mounds of Siam, composed mainly of the feces of these animals, are as much as a foot high and a yard across. Fresh earthworm mounds may be chimneylike in form, similar to those built by crawfish and crabs in the United States and the West Indies.

W. M. Johnson writes (October 21, 1948) from Colombia, South America:

> . . . . you will be interested in the macro-fauna of these soils . . . , especially the earthworms. There are at least two species in the region, some relatively small that build towers up to about three inches high, with their casts—like miniature crawfish deposits; the other worms are big, up to a half inch in diameter and at least 18 inches long. Photos show the earth's surface to a depth of ¾ inch completely covered with casts from these big worms.

He goes on to say that, in spite of their hardness, the casts break down rapidly when the rains come, and he infers that the fresh castings are all of the current season. With two wet and two dry seasons each year, one may assume that the worms till the soil to a depth of at least 1.5 inches each year.

INFLUENCE OF ANTS

Estimates of insect and other invertebrate fauna populations at Rothamsted by H. M. Morris gave a total invertebrate fauna of 15,100,000 per acre of land that received 14 tons of manure per year for about seventy-eight years. Of this population, 7,720,000 were insects. A plot that had been unfertilized for eighty-two years had an invertebrate fauna population of 4,950,000, including 2,470,000 insects. Observations on a pasture plot in Cheshire by the same author revealed an insect population of 3,586,088. These figures give an idea of the order of magnitude of animal populations in soils. Most soil invertebrates are short-lived; a large proportion of them die and return the organic matter of their bodies to the soil within a year and in some instances in much less than a year. It takes a large number of most insects or of the other very small invertebrates to make a weight equal to one field mouse, but the vast numbers and rapid turnover of population among the invertebrates probably more than offset the differences in weight of individuals.

The ants are among the most interesting of the insects, so far as soil formation is concerned. They are of many different species, each of which has its characteristic habits and habitats. W. P. Taylor and W. G. McGinnies state:

> Everyone is familiar with the earthworm's outstanding reputation as a soil maker. That other animals play an important role is doubtless not so widely appreciated. Yet Shaler in his paper on "The Origin and Nature of Soils" says that ants produce a far greater effect on soils than earthworms. Shaler thinks the vertebrates exercise an influence on the soil perhaps as great as that of all their lower kindred.

The leaf-cutting ants march for long distances, cut out fragments of leaves and stems of plants that are to be used directly or indirectly for food, carry them back home, and store them in underground chambers. Here they are used to produce fungus that is in turn used for food. In this way organic matter, both in the forests of Central America and in the brushlands of northern Mexico and Texas, is incorporated in the soil and converted to humus through the activities of fungi (planted by the ants) and by the deposition of fecal matter by the ants themselves. In a private communication, R. L. Pendleton reports that mounds of these leaf-cutter ants in Central America frequently are as much as 15 feet across and 3 feet high. I have seen smaller ones in the brushy lands of south Texas. The mere tunneling operations of these ants have the effect of moving mineral material from one horizon of the soil to another, and after the anthills are abandoned the chambers provide channels for rapid water penetration to the deeper subsoil horizons.

Throughout the greater part of the subhumid and semiarid portions of the United States there is a large red ant (perhaps of more than one species) which builds a chambered nest beneath the surface of the ground and a conical mound of excavated material above the surface. The mound is riddled by chambers used for the storage of food and the rearing of the young. Many who read this doubtless are familiar with the anthills scattered over tremendous areas of the Great Plains and in the deserts and intermountain valleys of the United States. Somewhat similar anthills occur in the humid sections of the country. I have made conservative estimates of the amount of soil material in the anthills of representative areas of the Western plains. The figures for the amount of material affected are based on a conservative estimate of the average-sized anthill and of the number of anthills per acre in some parts of the country. The anthill in the photograph (Fig. 2) has a volume of approximately 1.9 cubic feet of fine earth mixed with coarse sand and fine gravel

brought up from subsoil horizons. Some of the fine gravel may have been gathered from the surface in near-by areas. It is used by the ants to form a sort of pavement or roof over the hill. If we estimate that one cubic foot of earth will weigh approximately 90 pounds, then the individual anthill will probably contain about 170 pounds of soil material. If, as is not uncommon, there are 20 anthills per acre, about 3,400 pounds of earth will have been piled up on the surface by the ants. This material is removed largely from subsoil horizons, with the net effect of rejuvenating the soil by bringing up fresh material to the surface from beneath. To be sure, the number of large anthills in much of the Great Plains is less than 20 per acre, but it is also true that some anthills are destroyed every year and new ones are built on different sites so that the effects over a long period of years are very great. In addition, many small anthills built by other species, are not counted in this estimate (Fig. 2).

An aerial photograph (Fig. 3) taken in Kiowa County, Colorado, shows about 400 anthills on a selected 40 acres of land. Doubtless many exist that do not show on the photograph. An oblique aerial photograph (Fig. 4) taken northeast of Denver, gives a closer view of clearings made by ants. The hills are visible on aerial photographs because the ants have cut all vegetation from around each hill in a circle, the diameter of which varies ordinarily from hill to hill from about 6 to 20 feet. The bare earth reflects the light and

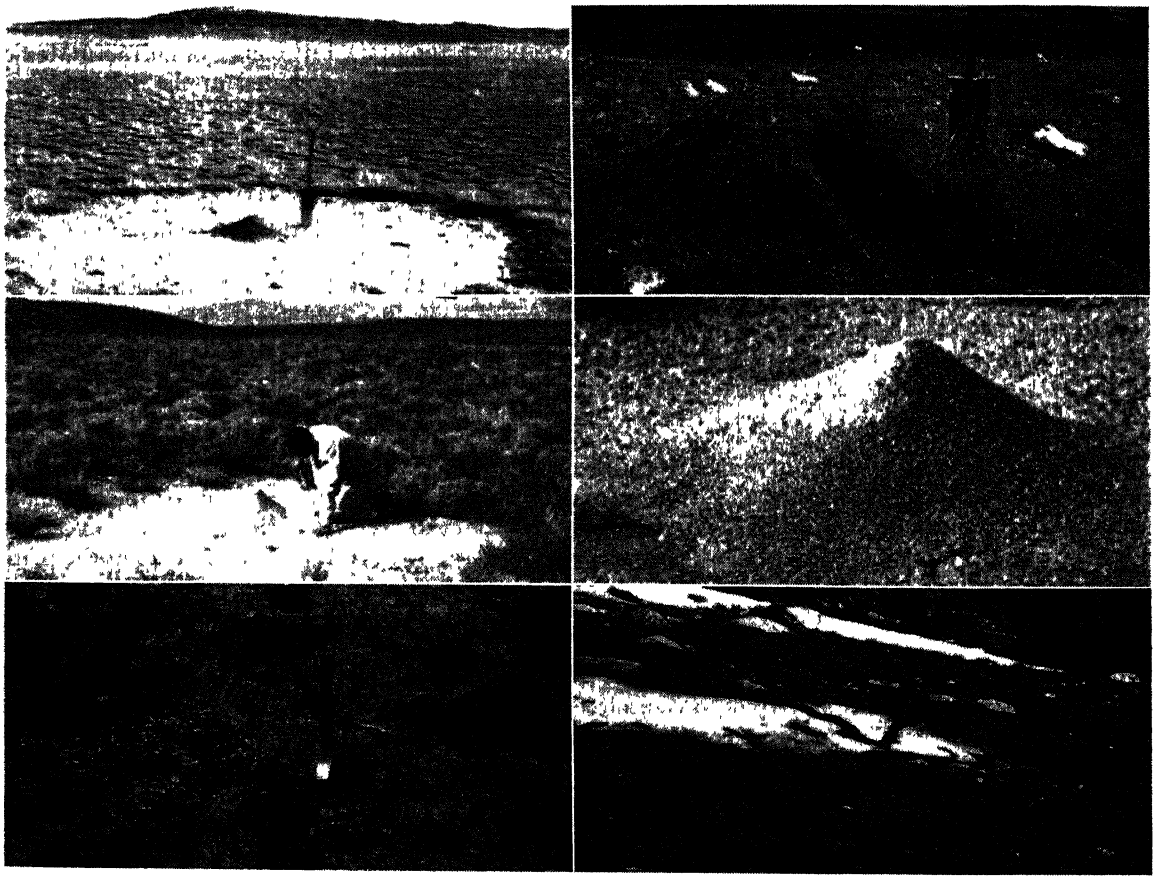


FIG. 2. *Top, left.* Anthill and cleared space in desert soil of southwestern Utah. Plants on rim of 12-foot cleared circle are slightly larger than those beyond because of reduced competition for water. *Right:* In the ponderosa pine forests of the West, ants sometimes build their nests almost entirely of pine needles, with a minimum of mineral material. When these are abandoned, they leave a concentrated supply of organic matter on the soil.

Center, left: Anthill on sierozem soil in Sevier County, Utah. *Right:* Close-up of same anthill, showing fine-gravel roof with which the ants protect their homes. The fine gravel is eventually incorporated into the A soil horizon.

Bottom, left: Geologist's hammer near center shows, by comparison, small size of one kind of anthill scattered through the United States. (Anthills outlined in black for clarity.) It was estimated that small ants piled 500 pounds per acre of sandy material on the surface at Archer, Wyo., within a few days' time. *Right.* Melting snow reveals "roll" of earth made by small burrowing mammals in the Medicine Bow Mountains, Wyo. Rolls soon break down and incorporated in the A horizon.

makes a whitish spot on the picture in each case. In some instances the hills are so close together that the bare spots merge and it is impossible to distinguish one hill from the next on small-scale aerial photographs. The largest mound I have observed so far was seen in the semidesert country of Duchesne County, Utah, where an anthill estimated to contain 300 pounds of earth was surrounded by a cleared area 40 feet in diameter.

Ants seem to prefer somewhat sandy and fine gravelly soil materials, probably because they can find suitable gravel for roofing their hills. I have seen anthills on the Houston and Wilson soils of Texas, however, where practically no satisfactory grit is available. There, the ants bring up balls of limy material from below ground and deposit them at the surface in lieu of fine gravel, thus tending to maintain a neutral or alkaline reaction in the surface soil.

Fragments of vegetation cut from the surface and removed to the anthills are eaten by the ants or stored in bins or granaries for future use. Commonly, great masses of seeds are stored in some of the bins of these anthills, presumably for use during the season of the year when vegetation is scarce. These materials are converted gradually to humus in the soil through the action of fungi and digestion by ants. The earth in anthills built on fine-textured soils frequently has a fine-granular structure induced by the activity of earthworms that feed on the organic material stored by the ants.

Clearings made by ants leave the soil exposed to erosive effects of wind and rain. On the older anthills one finds a slight depression around the mound itself where wind and water have removed the soil material; and small mounds of earth have collected around clumps of vegetation in the vicinity (Fig. 2). In this way soils are truncated and rejuvenated, little by little.

TERMITES

Termites have been studied to a limited extent by various soil scientists, especially in the tropics. G. Milne, of Africa (now deceased), Pendleton, formerly of Thailand, and G. Aubert have done some work on this problem. Some of the termite mounds are as much as 6–10 feet high and perhaps as much as 20–40 feet across. The termites gather vegetation from the soils of the neighborhood and carry it to their nests, where it is used for food either directly or for the cultivation of edible fungus. On some of the old strongly leached and

FIG. 3. Vertical aerial photo of ant-infested grassland area southeast of Denver. Minute light-gray specks represent circular bare spots where ants have harvested all vegetation. The 40 acres with cross lines are estimated to have about 400 anthills.

weathered soils of East Africa, Milne found that soils on certain plains where termites were active contained only a small trace of calcium, but that the soils in the termite mounds were very high in calcium; in fact, some of the mounds were calcareous throughout and had great masses of impure limestone (*caliche*) at the centers, which may possibly have formed through the hardening of lime left as a deposit from the waste matter from plants. It seems a reasonable hypothesis that over periods of centuries, and perhaps hundreds of centuries, the termites have gradually accumulated in their mounds practically all the calcium from the soils of the neighborhood by harvesting nearly all the vegetation that came up and carrying the calcium to the mounds in the form of organic matter. Milne suggests a few alternative hypotheses for the accumulation of lime carbonate, one of which is that the termites bring it up from deep substrata to use as cement.

Pendleton has raised the question, without solving it, as to why the termite mounds contain such a large amount of calcium without any apparent similar accumulation of phosphates. He reports that in Thailand a practical advantage was taken of the work of termites in accumulating calcium in the soils. Over a large area on the plains of Thailand most soils have been so highly leached and weathered that crop production on them is

almost out of the question without heavy fertilization. The soil supports only a scrubby growth of forest trees except in the vicinity of these ancient termite mounds. Here the *p*H is higher and the fertility is sufficient for the people to use the earth in the mounds as soil, and they can raise various food crops and tobacco with little fertilization. The termite mounds are partially leveled and used for farming, whereas the ancient soils between them are used only when heavy fertilization is possible. The process of leveling must be spread over a period of years in order to incorporate organic matter in the material of the lower horizons of the mounds. One could mention further that termites in the Temperate Zone of the United States perform the function of converting wood into humus in the forests and, unfortunately, in in our homes also if we are not watchful.

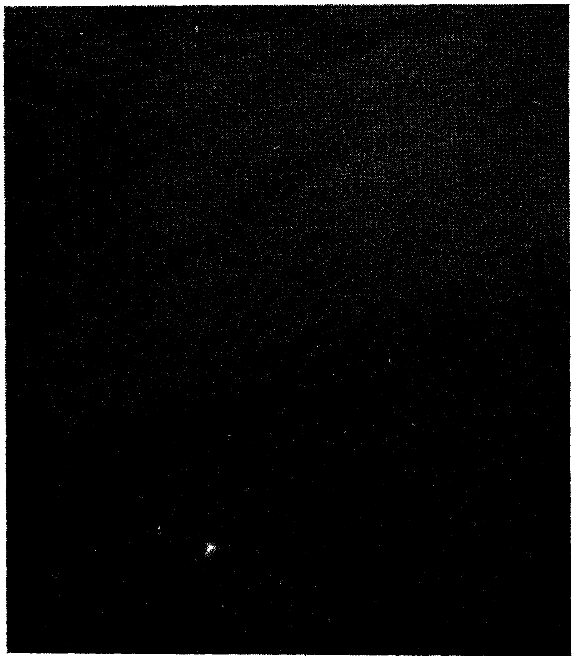


FIG. 4. Oblique aerial photo shows circular anthill clearings, 6–20 feet in diameter, on brown soils northeast of Denver.

Milne noted that native farmers in East Africa took similar advantage of fertility stored in termite mounds. He calculated that the mounds contain enough calcium carbonate to make a heavy lime application to the leached soils between them and recommended this as an agronomic amendment for some of the infertile soils of the region.

In a personal communication Aubert described termite mounds of several types in the French Sudan, French West Africa, and along the Ivory Coast. He states that some mounds contain concentrated lime carbonate, like those described by Milne, and some do not. All representatives from Africa at the 1948 Commonwealth Conference on tropical soils at Rothamsted mentioned the activities of termites in soils of Africa.

Wood lice, millipedes, centipedes, and spiders all affect soil profile development, especially to the extent that they consume organic matter of various sorts and convert it into other forms of humus. In forest soils the A_1 horizon in many places consists almost entirely of feces of these small animals mixed with those of earthworms. Some of this material retains its original form for a long time and imparts to the soil a characteristic and persistent very fine granular structure.

CRUSTACEANS

Large crustaceans, including crabs and crawfish, are very active in soils where the water table is within a few inches or a few feet of the surface. I have observed the activity of land crabs, especially in the West Indies, where they inhabit low swampy and marshy areas adjacent to the sea and build tunnels from the surface down to the water table. At night they bring up balls of earth tunneled from beneath and deposit them in the form of chimneys at the surface. In sugar-cane fields cultivation breaks down the chimneys and partially refills the burrows, which are cleared out again the night after the land is cultivated. An enormous amount of earth is thus kept in circulation. The crabs feed on vegetation, and possibly on carrion, and of course mix this organic matter with the soils. Incidentally, they are a great pest in cane fields and are poisoned systematically by agriculturists, or are trapped and used as food by the people.

Crawfish range in size from very small up to perhaps as much as one foot long. They are active in poorly and imperfectly drained soils from the Gulf Coast north to the Canadian border. They are especially active in the southern part of the gray-brown podzolic soils zone and throughout the zone of red and yellow podzolic soils, but they confine their activities largely to soils of the planosol, wiesenboden, and half bog groups. They build chimneys on the surface much like those made by the crabs. Chimneys on planosols in southern Indiana measure as much as 8 inches in height and about 4–8 inches in diameter (Fig. 5). The crawfish work at night, bringing mud from deep underground. Some of the vertical tunnels beneath the chimneys are as much as 15 feet deep, because the crawfish must have contact with the water table at all times. Their tunnels penetrate the clay

pans of the planosols, facilitating upward and downward movement of water, and they bring fresh soil material to the surface from great depths. The importance of their activities can be estimated from the photographs in Figure 5.

In one area studied in southern Indiana, a large chimney occurred about every 5–10 feet in each direction; all the chimneys had been built by the crawfish after the corn had been planted (Fig. 5). At the time this area was studied the corn was just ready for its first cultivation. Tunnels to the water table were about 10 feet deep. If one examines a field in which all chimneys have been destroyed by cultivation, one will find that a whole crop of new chimneys will have been built up a few days after the field is cultivated, as in this instance. The activity is especially great during the spring when there is plenty of moisture in the soil. Where crawfish chimneys weigh 3 pounds each and 625 of them occur per acre (1 every 8 feet in each direction), the total weight of earth heaped on the surface would be a little less than one ton per acre. If chimneys occur every 5 feet in each direction, the total weight would be a little less than 2.5 tons per acre. Perhaps the chimneys would be destroyed and rebuilt three or four times in one season, and 3–10 tons of earth would be moved. In 1946 E. Templin, H. Oakes, and I noted small crawfish chimneys on Beaumont clay in east Texas spaced so closely that there were 20,000–50,000 of them per acre in areas of greatest concentration. The water table was only a foot or two below the surface.

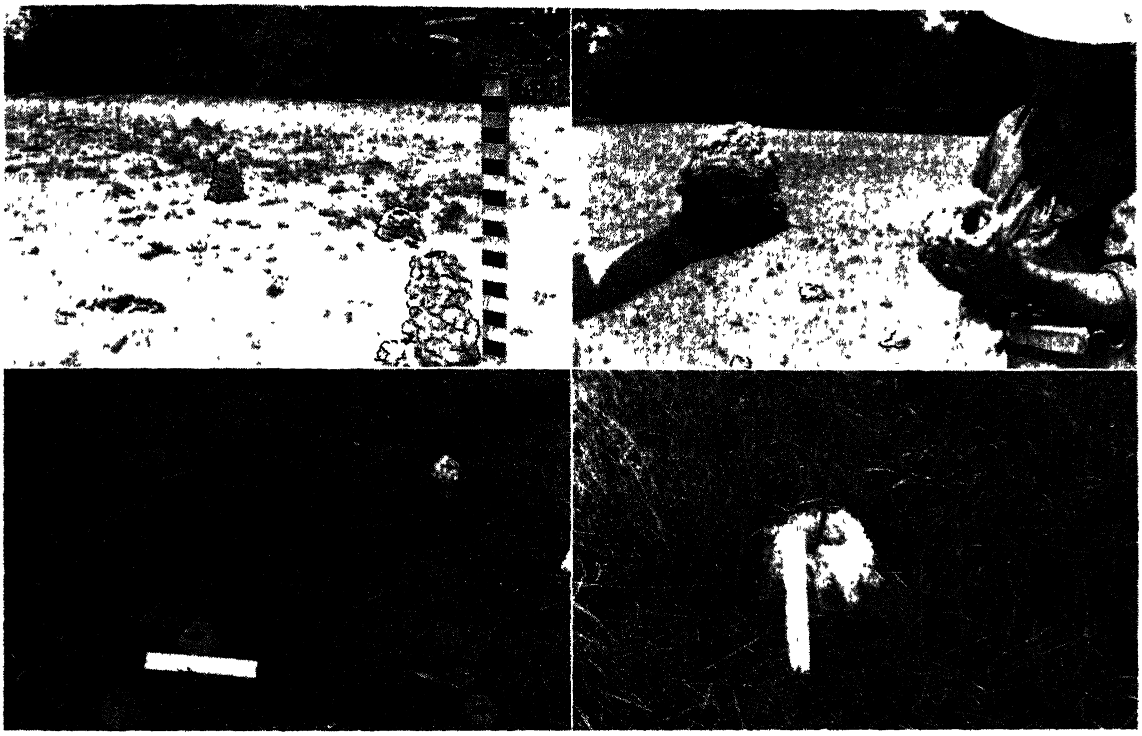


FIG. 5. *Upper left:* Crawfish chimneys in field of very young corn, on Robinson silt loam, a planosol of southern Indiana (Photo retouched to accentuate three of the chimneys.) *Upper right:* Close-up of two crawfish chimneys.
Lower left: Abundant small crawfish chimneys on poorly drained Plummer fine sandy loam, Beauregard Parish, La. Scale is 7 inches long. *Lower right:* Single crawfish chimney on Caddo fine sandy loam, same Parish.

One lasting effect of the activities of crawfish and crabs can be seen in very old soil profiles in certain red podzolic soils of southern Alabama. At Grand Bay, in Mobile County, a group of us studied a railroad cut through a low ridge on which there was a red podzolic soil with an incipient lateritic hardpan 2 or 3 feet below the surface. In this cut were several "fossilized" crawfish tunnels which extended from approximately the surface to a depth of 6 or 8 feet in the cut. The tunnels were lined with hard limonite to a thickness of about 0.5 inch, and the interiors were filled with clayey material. Each tunnel terminated at the bottom in an ovate chamber about 6 × 10 inches. It must have been thousands of years since this particular set of tunnels was made by the crawfish, because the water table has long since retreated many

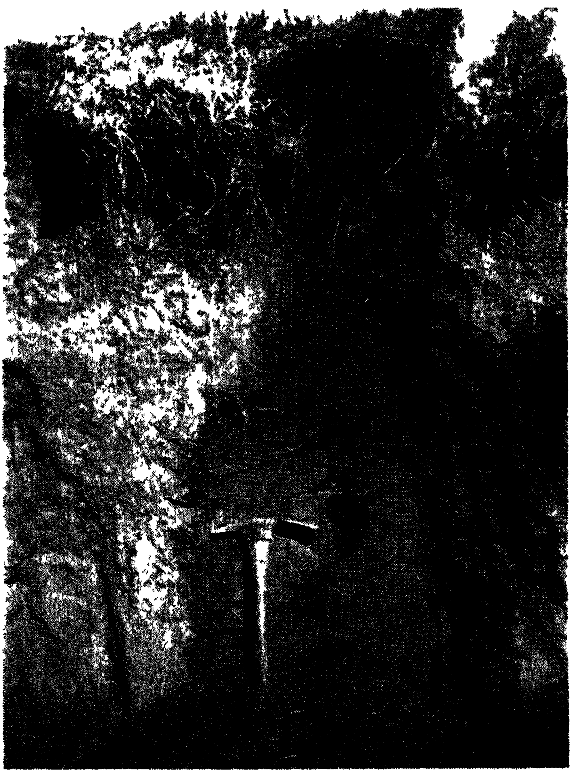


FIG. 6. Profile of Marshall silt loam, a prairie soil of southwestern Iowa, shows dark blotches in deep subsoil where a gopher burrow has been filled by dark earth from the A horizon.

feet below the bottoms of the chambers. The fossil crawfish tunnels are in a remnant of a former ground-water laterite soil which formerly extended over a considerably greater area than it now does. In recent times it has been converted to a red podzolic soil following a lowering of the water table brought about by dissection of the land. The soil occurs on the highest Pleistocene terrace remnant.

OTHER ANIMALS

Burrowing mammals and, in some instances, amphibia and lizards are important factors in the rejuvenation of soils in many parts of the world. Their activities are especially noticeable in grassland areas of the Great Plains and in the deserts and semideserts where prairie dogs, gophers, and other rodents are very abundant and active; but the total effects are probably just as great, though less noticeable, in forested areas. These animals burrow through the entire *solum* and well into the parent material or substrata beneath the soil, and parts of all horizons and underlying materials are brought to the surface and there thrown out as a heterogeneous mixture. In some prairie-dog towns almost the entire soil has been completely churned and rejuvenated by the activities of the animals. Prairie dogs frequently build their towns in places where subsoil horizons are clayey, and in some instances they select areas of solonetz or solodized solonetz for their homes. Their activities tend to destroy parts of the clayey layers and to heap fresh limy or salty material on the surface.

Figure 6 shows how rodent burrows in deep subsoils become filled with dark soil from surface soils of the prairie, chernozem, and chestnut soils zones. J. E. Weaver, of the Botany Department, University of Nebraska, tells me that he always finds burrow fillings in excavations made for studying grass roots in soils of the Great Plains. This checks with the experience of soil scientists in the same region. Some of the small burrowing mammals bring soil materials to the surface and use it as backfill when they burrow in deep snow. These peculiar "rolls" of material are exposed when the snow melts, and break down to form "ribbons" of light-colored subsoil material on the darker surface soils (Fig. 2).

In May 1947, L. T. Alexander and I estimated roughly the amount of earth that burrowing animals had piled on the surface or mixed with the surface soil of Rago silt loam at the Dry Land Experiment Farm, Akron, Colorado. We measured off a 4-acre plot of land in a virgin pasture where sandy and gravelly mounds marked the former dwellings of prairie dogs and badgers (Fig. 7). Sixty-nine mounds were counted in the 4 acres, an average of about 17 per acre. A deep excavation showed the original soil to be developed from calcareous loess, of silt loam texture, that overlies strata of gravel and sand at depths ranging approximately 6–10 feet. In the course of soil formation the lime carbonate has been leached out to depths ranging from about 14 to 20 inches. That all the burrows had reached down as far as the sand and gravel layers is proved by the preponderance of sand and gravel in the mounds. The undisturbed soil contains no gravel or medium- or coarse-grained sand. Two of the several large mounds measured proved to have a maximum thickness of 18–19 inches and a diameter of 24 feet. For convenience in estimating the volume it was assumed that the mounds were conical in shape, an assumption that will give a conservative estimate of the volume of the mound. The volume of a cone is calculated as follows:

$$V = \pi \frac{r^2 \times h}{3}$$

where r is the radius of the base and h is the alti-

tude of the cone; substituting measured values for r and h,

$$V = \frac{\frac{22}{7} \times 12^2 \times 1.5}{3} = 226.3 \text{ cubic feet.}$$

One cubic foot of dry loam or sandy loam, with some gravel, will weigh about 100 pounds. The weight of the mound is therefore about 22,630 pounds, or 11.3 tons. By similar methods, weights of mounds of several different sizes were estimated to range from a minimum of 210 to the maximum of 22,630 pounds given above.

Estimating the average size at 3,770 pounds, we obtain a total weight of 130 tons for 4 acres, or 32.5 tons for 1 acre. By another method of estimating we get a higher figure (Table 1).

TABLE 1

No of Mounds of Given Size	Weight of Each Mound	Total Weight in Pounds
10	22,600	226,000
15	3,850	57,750
24	1,000	24,000
20	210	4,200
		311,950

This is equivalent to 156 tons on 4 acres, or 39 tons on 1 acre. Thus we may say conservatively that the burrowing animals have brought 30–40 tons of mixed sand, gravel, and subsoil material to the surface. Much of this came from a depth of 8–10 feet underground.

Near the same place, we counted 50 fresh mounds in a prairie-dog town on one measured acre of land (Fig. 8). The mounds ranged from 2 feet in diameter, 6 inches high, to 18 feet in diameter, 18 inches high. Some of these mounds were largely gravel and sand; others, where the loess is thicker, were almost entirely of yellowish calcareous loess of silt loam texture. On this acre of land a calculation based on the weight of an average mound gives a total of about 22.5 tons of earth piled on the surface.

On this field the prairie dogs were keeping the grass, largely western wheat (*Agropyron smithii*) and blue grama (*Bouteloua gracilis*), cut close to the ground. Heaps of rodent manure were piled around the borders of the mounds; and these, with the urine from the animals, were serving to fertilize the next crop of grass. One of the characteristic phenomena around old mounds is the dark-green color of the vegetation—evidence of abundant nitrogen.

Surface soils on about one third of the experimental plots at the Akron Station have been converted from silt loam to loam through the activities of rodents and badgers. Apparently a colony of these animals lived here before the station was established. Subsequent tillage operations have thoroughly mixed the gravelly and sandy materials of the mounds with the original silt loam surface soil. The thickness of loess and soil over stratified sand and gravel on the plots, where measured, ranged from about 6 to about 10 feet.

The common pocket gophers and ground squirrels of the Great Plains appear in large colonies from time to time. In 1947 Earl D. Fowler and I made some measurements of earth moved in a few weeks by a colony of gophers at Lincoln, Nebraska (Fig. 8). We calculated that 56,000 pounds, or 28 tons, of earth had been piled up by these animals on one acre. The smaller mounds weighed about 15–20 pounds each, and the largest was estimated to weigh 400 pounds.

This brings to mind the paper by V. B. Scheffer on the Mima mounds of western Washington.

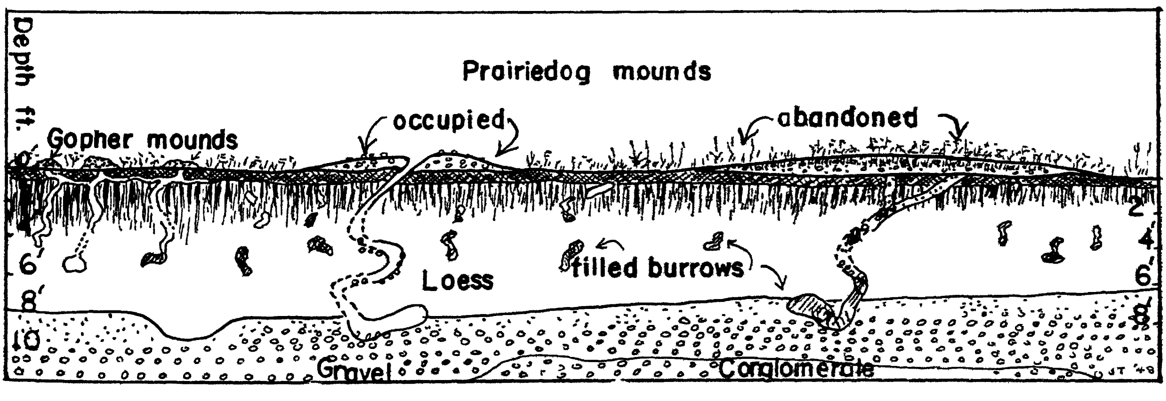


FIG. 7. Diagram of Rago soils on virgin pasture land of the Dry Land Experimental Station, Akron, Colo. Rago silt loam occurs between the mounds built by small mammals. Soils on the mounds contain a mixture of silt loam with sand and gravel brought up from deep substrata. In adjacent cultivated fields most of the mounds and intervening soils have been mixed to form Rago loam. Note dark earth in vacated burrows.

Scheffer suggests that these phenomenal mounds, and possibly the famous San Joaquin soil mounds of California, are the work of gophers. R. C. Roberts and W. J. Leighty, of the Division of Soil Survey, and L. C. Wheeting, of Washington State College, visited the mounds with me in September 1948, and we found it difficult to suggest any other satisfactory explanation for their development. The soils on these mounds are very gravelly and contain a high percentage of black humified organic matter. The black horizon is more than 30 inches thick on many of the mounds, which is much thicker than in most prairie soils of normal development, and lends weight to Scheffer's hypothesis. Figure 8 shows some mounds in Montana that are similar to the lower of the Mima mounds.

Where rodent colonies become too dense, most of the vegetation is destroyed and erosion may be accelerated. A particularly noteworthy example of this was seen on the Tibetan borderland and reported to me by a Chinese scientist, C. C. Ku, who traveled in that region. In this area certain burrowing rodents had become so abundant that all the soil was honeycombed by their burrows. Yaks and other domesticated animals, pastured in these areas, broke through the tunnels and exposed the soils to wind and water erosion. In some places all the soil was swept from large areas of hillsides in this region.

With reference to burrowing animals, Taylor states:

> Mice and other burrowing species, notably the pocket gopher, in many of the forests of the West are continually cultivating the soil, letting in water and air, carrying down vegetation, bringing up earth, in general helping the great soil complex to function. There is little doubt that all these creatures have their place in maintaining the natural equilibrium between soil, climate, plants, and animals

Field and woods mice, moles, and shrews have intricate systems of tunnels in the soils of forests and grasslands. Ground squirrels and gophers are so abundant in some places (e.g., south Texas) as to turn over 15–20 percent of the surface soils in a single season. Kangaroo rats and other burrow-

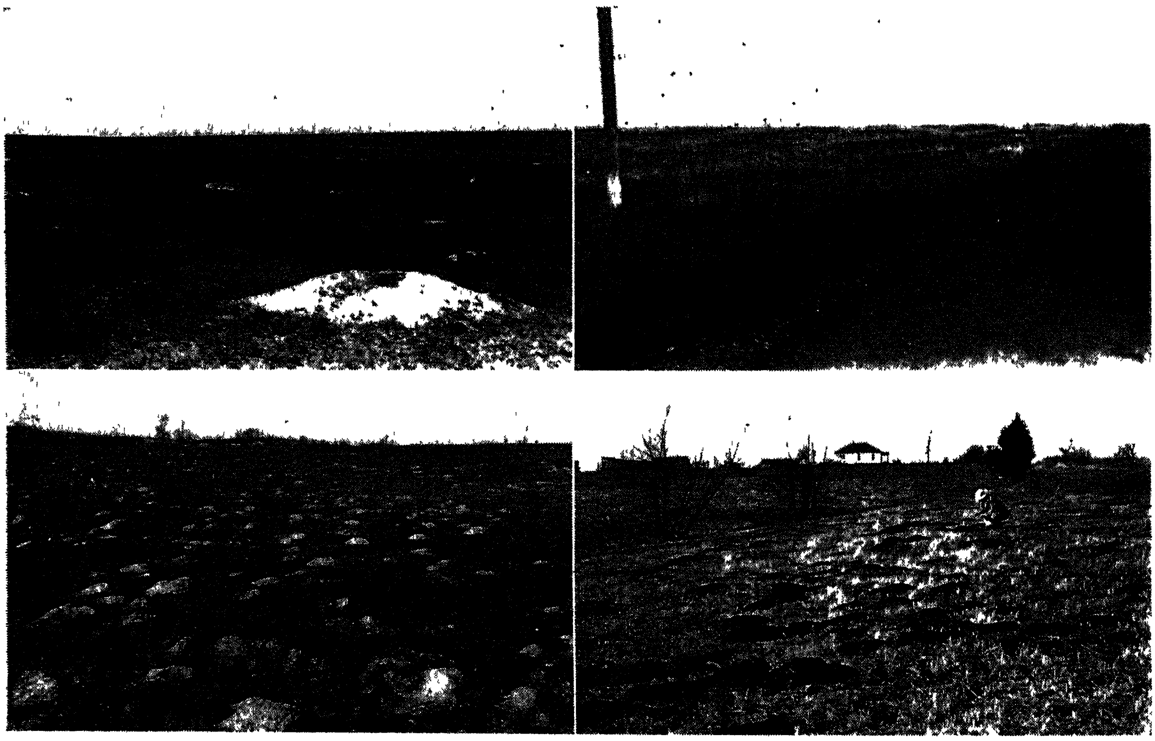


FIG. 8. *Upper left:* Prairie-dog mound, Akron, Colo., about 8 feet in diameter and 10 inches high. Mound contains much sand and gravel, whereas surrounding soils do not *Upper right.* Low, broad mounds on chestnut soils on a high terrace, 10 miles west of Wisdom, Beaverhead Co., Mont. An intimate mixture of silt loam, gravel, and organic material, the mounds resemble the lower of the Mima mounds of western Washington. Burrowing rodents are very active in them.

Lower left: Small gopher mounds southeast of Lincoln, Nebr., made in autumn 1947. Total earth piled on surface is about 56,000 pounds (28 tons) per acre. (Photo by W. M. Johnson.) *Lower right:* Gopher mounds in deep sandy soils of Cherokee County, Texas, tentatively correlated as Eustis loamy fine sand. About one third of surface soil has been "plowed" in one season (fall–winter, 1947–48).

ing mammals in desert and semidesert regions either build mounds 6–10 feet in diameter and 1–3 feet in height, or build their homes in sandy mounds developed by wind action. In some places the colonies of the kangaroo rats make up fully 30 percent of the land surface. Taylor records as much as 12.5 pounds of sections, crowns, and seeds of grasses stored by kangaroo rats in one burrow.

One might go on to enumerate many other kinds of animals that burrow in the soils and bequeath their waste materials and dead bodies to the organic component. Suffice it to say that the animals that live in the soil in many places are almost as important in the development of soil profiles as the vegetation of the region. If we consider the effects of all land animals on soils, we may say that animals as a whole probably are as important in soil profile development as vegetation, except, of course, that animals cannot exist without vegetation. Doubtless we should have soils if there were no animals, but we could not have soils without plants. It is to be hoped that in the future more soil research will be directed toward the investigation of the influences of animals on soil character. Truly, the biological factor of soil formation *is* a *biological* one, involving symbiotic and antipathetic relationships among animals and animals, plants and plants, and among plants and animals.

SCIENCE, THE ENDLESS FRONTIER

Atomic Clock

The National Bureau of Standards has announced the development of a new primary standard of frequency and time, invariant with age. This is the atomic clock, based on a constant natural frequency associated with the vibration of the atoms in the ammonia molecule. The improvements in frequency and time measurement are expected to alleviate the crowding of the radio spectrum, to aid in astronomical measurements, to facilitate long-range radio navigation systems, microwave spectroscopy, and the like.

Electronic Letter Reader

Developed for the use of the blind, the laboratory model of an electronic device that converts printed letters into actual sounds was exhibited recently at the Museum of the Franklin Institute. The device, designed for institutional use at the suggestion of the Office of Scientific Research and Development, is large and costly. According to the Franklin Institute *News:* "As the user moves the scanning device along a line of type, a miniature cathode-ray tube explores each letter with eight spots of light arranged in a vertical line. When the spot of light passes over any black portion of a letter, an impulse is sent to the selector unit. There impulses are counted electronically. After the letter has been completely scanned, the total number of impulses is noted by the selector unit. This number, which is unique for each letter, actuates a magnetic tape recording of that letter, and the audible sound is reproduced through the loudspeaker."

Ancient Book

A papyrus estimated to be 3,600 years old, "the oldest scientific book in America and the oldest nucleus of really scientific knowledge in the world," has been presented to the New York Academy of Medicine for its rare book collection. Gift of the New York Historical Society and the Brooklyn Museum, the *Edwin Smith Papyrus* is divisible into three parts: the first and most valuable deals with 48 surgical cases, each of which is methodically presented, with explanatory comments added by later contributors; the second is entitled "Incantation of Expelling the Wind of the Year of Pest;" the third, "Incantation of Transforming an Old Man into a Youth of Twenty."

Rocks

Two tons of rock samples, drilled from beneath the surface of Funafuti Atoll more than fifty years ago by British and Australian scientists, have been lent by the British Museum to U. S. scientists for extensive study. They are housed at the National Museum in Washington side by side with other specimens from Bikini. Experts hope eventually to solve the mystery of the geologic past of the Pacific's thousands of atolls.

ON DEFINING "SCIENCE"

A. CORNELIUS BENJAMIN

Dr. Benjamin (Ph.D., Michigan, 1924) is John H. Lathrop professor of philosophy and chairman of the Department at the University of Missouri. He has taught also at Illinois and Chicago and studied in France and England as a Guggenheim Fellow. He is the author of The Logical Structure of Science *and* Introduction to the Philosophy of Science, *as well as of numerous educational and philosophical articles.*

IT IS no easy task to define science. There are at least two reasons for this. In the first place the word has had a long and complicated history. As a result it has taken on a great variety of meanings, often inconsistent with one another. In the second place, and much more important, it has recently become a term of praise; since science is the "best" way of doing things we demand that advertising, detective stories, social planning, and even religion itself be scientific in method and intent. As a consequence the term has become dangerous because of its misleading associations; when anything calls itself "science," beware!

The problem is further complicated by the fact that every definition is, as everyone knows, to a certain extent arbitrary. One can, like Humpty Dumpty, make words mean exactly what he chooses to have them mean. But all of us, no doubt, feel the social pressure of conventional usage. When Eve saw Adam for the first time, as Mark Twain tells us in *Eve's Diary,* she decided to call him a "man." Why? Because he looked like a man. If as members of a social group we are to use the word "science," we ought to apply it to something that *looks* like science.

Two definitions that are in common use seem to confuse rather than clarify the issue. One of these identifies science with something that is commonly called the "scientific spirit." This, in turn, is defined rather vaguely as a way of looking at the world which is characterized by impartiality, freedom from prejudice, and respect for the criteria of truth. The scientist seems to feel to a very high degree his responsibility to the ideal of knowledge, and cautiously avoids temptations to fall into wishful thinking, or to be duped by his senses, or to accept superstitions or unsupported authority as adequate knowledge. Science, then, can be defined as any study that is characterized by objectivity, unemotionality, rigor, and control.

The other definition swings as far in the opposite direction; it identifies science with the quantitative method as applied in laboratory and experimental techniques. This is in accord with the traditional picture of the scientist, who is commonly represented as a white-coated individual surrounded by test tubes, flasks, and intricate mechanical contrivances. Science, according to this conception, first became scientific when it abandoned purely observational techniques and substituted, on the one hand, manipulatory acts that involved setting up situations not normally occurring in nature apart from human intervention, and, on the other, instrumental aids to increase the range of the sense organs and to render measurement operations more precise.

Both these definitions are perfectly satisfactory if one is willing to accept the consequences of their usage. According to the former, of course, much will be science that is not commonly called by this name—philosophy, history, all the social studies, theology, and many authoritarian studies which, at least within the framework of their method, may be pursued with true regard for the principles of objectivity and control. According to the latter, much will not be science that is commonly called by this name—mathematics (which has no laboratory and performs no experiments); astronomy (which, though it uses instrumental aids, has no laboratory and performs no experiments in the strict sense of the word); theoretical mechanics (which is no more experimental than is geometry); and much of biology and psychology (which are highly restricted in their use of quantitative techniques). If one wishes, therefore, to win friends and influence people he has only to adopt the former definition, according to which he will be in a position to call practically everyone scientific, thus distributing honors widely. If, on the other hand, he is not averse to making enemies, he has only to adopt the latter definition, according to which very few of his associates will be entitled to the dignity of the name.

A much happier procedure, presumably, would

be to choose a definition somewhat less broad than the former and somewhat less narrow than the latter. We do not gain in the understanding of a term if we define it so that everything falls under it or so that nothing falls under it. I propose, therefore, as a compromise, that science be defined as "the method of verified hypotheses."

Without further discussion and clarification, however, such a definition carries no enlightenment whatever. We define only when we equate notions we do not understand with notions which we do. Consequently, unless we have a fairly clear conception of what is meant by "hypotheses" and by "verification," we have made no advance. In fact, the definition, at least on the surface, does not look too good, for mathematics is concerned neither with hypotheses nor with verification, and there is a long tradition among natural scientists, beginning with Newton's *Hypotheses non fingo,* running through Mach and Pearson, and ending with contemporary positivists, to the effect that science makes most rapid progress when it abandons the search for hypothetical explanations and confines its attention to mere description and classification.

I

The first step in understanding this definition of science is clarifying the general goal of the scientific enterprise. In the most elementary terms possible, the job of science is to discover facts. Usually this is a simple matter. Many facts impose themselves upon us and we cannot avoid them; living, in fact, is merely a matter of adjusting ourselves to these ever-present facts. But the scientist is instilled with the desire to discover more and more facts. In the attempt to satisfy this need he could, by proceeding very inefficiently, simply look about here and there, waiting, like Mr. Micawber, for something to turn up. But he has been provided by nature with a tool that enables him to do the job much more effectively. Through the activity of his mind he is able to make guesses, controlled by the facts he has already discovered, as to the existence of certain specified facts, in certain specified areas, which he is likely to find if he looks further. Thus by using his mind rather than passively listening to nature he is able to save a great deal of time and effort, since he now knows what to look for and where to look. This directed observation is possible by virtue of his capacity, through imagination and inference, to go beyond the facts already observed and formulate, through the use of symbols and other thought constructions, ideas of objects and processes which may not themselves be capable of observation, but are merely hinted at and suggested by the facts already accumulated. Hence we can describe the job of science more accurately by saying that it is concerned with the construction of a system of ideas that is presumed to portray the realm of facts. At any given stage in science, of course, our knowledge does not fit the facts precisely. On the one hand, since there are many facts that have not yet been discovered, knowledge is always less than the facts; but, on the other hand, since we have anticipated what nature is going to reveal, knowledge is always more than the facts. The final goal of science is to make knowledge fit the facts in all its parts and in all its details.

The method of verified hypotheses can be best understood by considering it from the point of view of its three phases or aspects. One is tempted to speak rather of "stages" than "aspects," and this terminology has a certain advantage. But it suggests a temporal order among the phases of the method, which is somewhat misleading. The important thing is the relations of logical dependence among the phases, not the historical order in which they occur.

The first aspect can be called "getting the facts." In the broad sense, however, it includes not only getting the facts but manipulating, classifying, and correlating them, and perhaps even measuring them as well. The two main processes for getting the data are observation (plus introspection if one wishes to include certain types of psychology) and interpreting reports—both human and instrumental. No one, I should suppose, would call into question the need for resting science ultimately upon observed data; it both begins and ends with things that we see, hear, smell, taste, and touch. This does not mean, of course, that all the entities that are talked about by science are observable, but it does mean that any such as are not directly perceivable must be capable of manifesting themselves indirectly in terms of observable phenomena. Reports are another important device for getting data. What is not commonly recognized is that human reports function in essentially the same way as do instrumental reports. In both cases, we have something that is given through observation (the written or spoken word of another individual, the image on the eyepiece of a microscope), on the basis of which, through a knowledge of the transmitting medium (reliability of the witness, principles of optics), we judge ourselves to be witnessing a natural object (a total eclipse of the sun, the structure of a cell).

Manipulating the data includes all processes—much too numerous even to list—such as combining and isolating, increasing and decreasing in intensity, speeding and slowing, heating, cooling, dissolving, magnetizing, dissecting, which are designed to produce a state of affairs that would not have occurred if we had not put our fingers into nature. These are the so-called experimental techniques, though we shall see in a moment that experimentation functions in a much more definitive way in another phase of science. Here we are concerned primarily with what are commonly called "experiments for discovery" rather than "experiments for proof." These techniques are designed to induce a reluctant nature to yield her secrets; if she will not speak freely we poke and prod her, thus forcing her to respond in unusual ways.

The descriptive processes are many, though for the purposes of this discussion they may be limited to three: classification, correlation, and measurement. Classification involves the activity of grouping similar objects into classes on the basis of common attributes. Correlation, which depends on classification, involves the determination of the frequency with which an object lies in two classes at once, or an object of one class is associated with an object of another class. Measurement is the process of representing the qualities of objects by numbers.

The result of the activities of getting the data, manipulating them, and describing them may be called the "descriptive phase" of science. Many insist that this is all there is to science; a science that has correctly classified and measured all available data has completed its job, and there is nothing to be done except await the discovery of further data, which will then have to be described according to the same pattern. But, whatever may be said for this claim, certainly much of what is commonly included in science is not found here. There are no hypotheses or theories. In the strict sense of the words, neither induction nor deduction has been employed; for the laws, if they are to be accurately descriptive of the data, must be statistical correlations and not universal connections, and no deductive anticipations of future data have been made. There is, in fact, no logical structure evident in the body of symbols constituting a purely descriptive science; each symbol describes its fact, and there is no method by which from the knowledge of one fact we could infer knowledge of another. Consequently explanation is totally absent from such a science; it answers the question How? but does not answer the question Why? A diagram of such a science is given in Figure 1.

The need for including in science something more than pure description arises when the scientist, on close inspection of a descriptive science, notes an important fact. Frequently he discovers that among the symbols constituting such a science one of them, *A*, implies another, *B*. This means that if *A* is true, *B* must also be true. Of course, *B* is already known to be true, since it was derived by the direct description of the fact which it represents. But the scientist now knows that if he hadn't happened to discover *B*, he could have anticipated it from his knowledge of *A*; thus he could have known of the existence of a fact before he observed it. From this recognition it is but a step to two other very important conclusions. May it not be possible to find another symbol of his scheme, *C*, which will be found to imply *Q*, which is not now part of his science but must be included since it is logically demanded by *C*? And may it not be possible to devise a symbol, *K*, which is not now part of the science but will imply *A*, which is already part of the scheme? If the answer to these questions is in the affirmative, he has provided himself with very significant techniques for extending his science into the realm of previously undiscovered facts; he can now anticipate, through an activity of mind based on fact, what nature is likely to reveal in areas that are still unexplored.

This aspect of science may be called the "phase

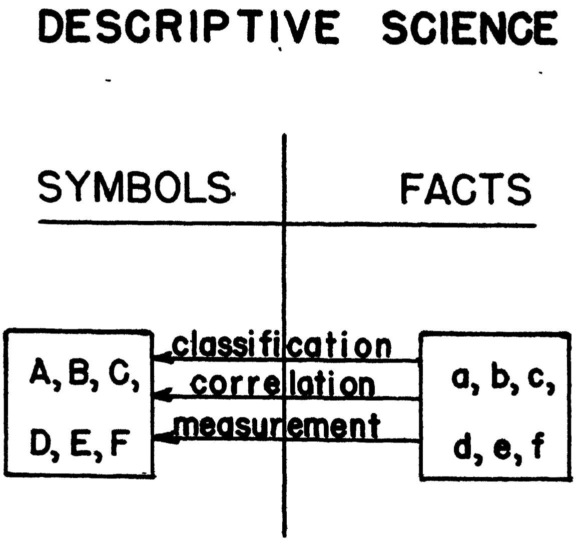


FIG. 1. *a,b,c,d,e,f* are facts obtained through direct observation, through reports, and through manipulatory operations. *A,B,C,D,E,F* are descriptive symbols obtained from these facts by the operations of classification, correlation, and measurement.

of discovery." In actual scientific procedure deductive expansion of a scheme—i.e., expansion by finding further symbols which are implied by symbols in the scheme—is comparatively rare, and the cases in which it occurs are usually trivial. But expansion through induction—i.e., by finding symbols not in the system which imply symbols which are in the system—is a highly significant method. It is, in fact, precisely what is meant by the discovery of hypotheses. The nature of this movement has not yielded to logical analysis. We call it variously "insight," "intuition," "hunch," "illumination"—attempting by all these words to cover the fact that we do not know precisely how it occurs. Deduction is well understood; we know how it operates and we can set up the rules for its validity. But induction is still essentially a mystery. All that we can say about it can be exhausted in a few propositions. It is essential to science. It does occur on numerous occasions. It produces most significant results when preceded by a long and persistent study of facts. Ability to make inductions is distributed very unequally among men.

The most significant defect of induction, however, is that it never gives us truth, but only possibility or, at best, probability. This can be seen from the nature of the implicative relation. If K implies A, then if K is true A must be true. But if A is true we cannot infer from this to the truth of K. K might be false and still imply a true proposition. Since the relation of implication is more commonly expressed in terms of explanation, we can state the same fact by saying that whereas K explains A something other than K might explain A equally well. To be sure, when K occurs to a scientist through an act of illumination or a flash of insight, it usually carries with it a strong feeling of conviction. He feels at the moment that it *must* be true. But later experience usually shows him to have been mistaken, as he quickly learns if he tries to play the races on similar hunches, and as he would readily admit if historians had reported the scientific failures as faithfully as they have preserved the records of scientific successes. The plain fact is that ideas obtained through this act of inspiration, however plausible they may appear at the time, are nothing more than well-founded guesses, and they must be substantiated further before they can be included among the established truths of science. This testing requires the examination of a further phase of science, the phase of verification. Before passing to such considerations, however, mention must be made of another feature of a science that is undergoing the transition from the descriptive to the explanatory stage.

In the search for implicative relations among descriptive symbols the scientist readily sees that these relations do not hold among fuzzy concepts or highly complicated propositions. If, for example, he were to determine the sum of the angles of a triangle by actual measurement, he would find this very rarely to be exactly 180°; in the great majority of cases it would be greater or less than this amount and to varying degrees. Consequently, if geometry were a descriptive science, he would have to express his results statistically, stating, with regard to any class of triangles, how many had the sum equal to 180°, how many were less than this by one second, how many were greater by one second, how many less by two seconds, and so on. It would be extremely difficult, if not impossible, to construct a theory of the triangle in terms of which this could be understood. But it is readily seen that these values fluctuate around 180°, and it requires no great stretch of the imagination to suppose that if the triangles were perfectly drawn the sum would always exactly equal this amount. A simple substitution is then made that immediately brings about this result; the meaning of the word "triangle" is changed to mean "perfect triangle" rather than "actual triangle." The law of triangles then becomes exactly true of perfect triangles, though only approximately true of actual triangles, and can then be explained in terms of the simple definitions and postulates we learned in our high-school geometry.

The frequency with which such notions occur in science, and their importance for a general theory of science, are not generally recognized. Geometry, in the strict sense of the word, is made up wholly of concepts of this kind, with consequences which we shall see later. Physics would find it hard to dispense with perfect gases, ideal levers, frictionless motion, and the like. In the social sciences frequent use is made of such fictions as the completely isolated individual, the ideal State, and the purely economic man. What should be clearly understood in connection with such notions as this is that a science in which they occur has, to this extent, lost its descriptive value. In this elaborating and refining process, which Eddington calls the method of "just like this only more so," we attempt to get away from the complexities and crudities of sense objects in order to replace them by simpler and more precise counterparts. But to the extent to which they are simpler and more precise they are no longer counterparts.

The world does not contain perfect levers, ideal gases, economic men, and utopias; when we build science on such notions, therefore, we are no longer talking about the world in which we live. Many who praise the exactness and precision of science fail to realize that only through the use of such fictions and idealizations does science have its mathematical rigor. Applied science fits the world better, but it lacks the neatness and logical structure of idealized science; idealized science has the required coherence, but it loses to a great extent its descriptive value. Einstein has expressed this very well in a statement that is often quoted: "As far as the laws of mathematics refer to reality, they are not certain; and as far as they are certain, they do not refer to reality."

II

Let us now turn to a consideration of the next phase of science—verification. This may be broken up into two subphases. The first involves making predictions on the basis of the assumed hypotheses. This is deductive in character; it begins not with the descriptive symbols, however, but with the hypotheses. The scientist simply asks himself what would have to be true in nature *if* his hypothesis were correct. Such predictions always have the *if . . . then . . .* form, where what follows the *if* is the hypothesis, and what follows the *then* is the anticipated observations, either experimental or nonexperimental. The second of the two phases involves the checking activity. The predicted consequences are compared with the actual facts as disclosed through observation; and the hypothesis is rendered more probable or rejected according as these agree or disagree. In this phase the techniques are the same as those in the descriptive phase—getting and manipulating the data, and classifying, correlating, and measuring them. The direction of the activities is reversed, however: there we were given the data and were concerned with devising symbols; here we are given the symbols and are attempting to verify them in the data. It is in this stage, rather than in the descriptive stage, that experimentation plays its distinctive role.

A science that exhibits all three phases may be called an "explanatory science." This is what is commonly meant by the term when it is employed in ordinary discourse. In a sense, of course, an explanatory science includes a descriptive science, since the former was obtained from the latter by extension through the use of hypotheses and theories. But, as has already been pointed out, this is not quite the case. An explanatory science, by virtue of the use of abstractive and idealizing activities, has lost some of its direct descriptive value. It has lost contact with reality to the extent to which it has achieved a certain logical structure. Whether this involves a net gain for the science itself is debatable; thus we cannot dogmatically assert that an explanatory science is "better" than a descriptive one. But certainly in our recognition of the possibility of such a thing as an explanatory science, we have advanced in our *conception* of science; we now see more clearly what "science"

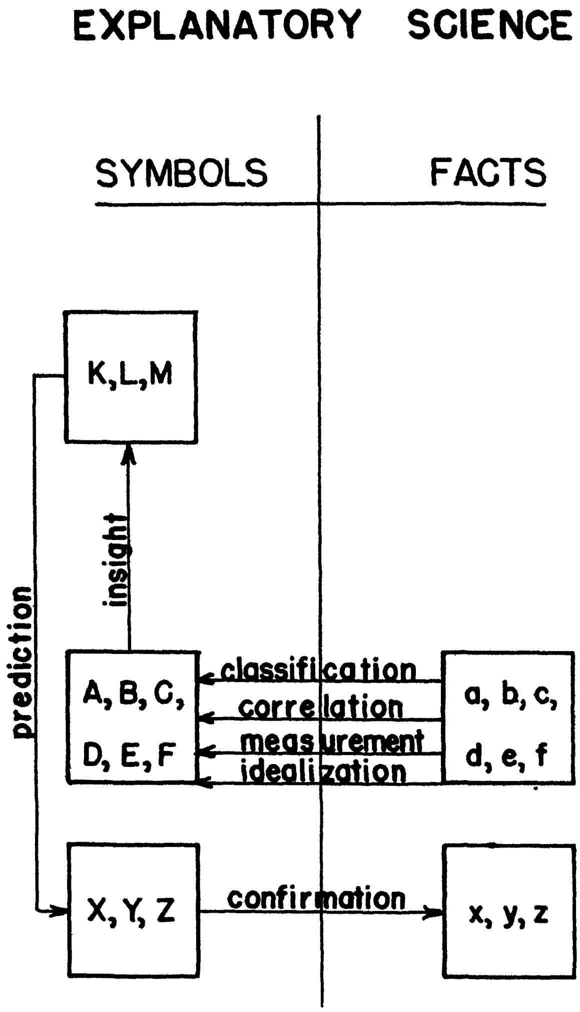


FIG. 2. *a,b,c,d,e,f* are facts obtained through direct observation, through reports, and through manipulatory operations. *A,B,C,D,E,F* are descriptive symbols obtained from these facts by the operations of classification, correlation, measurement, and idealization. *K,L,M* are hypotheses or theories obtained from the descriptive symbols through insight (induction). *X,Y,Z* are the predicted consequences of the hypotheses, derived from them by deduction. *x,y,z* are the newly discovered facts which confirm these predicted consequences.

means. A diagram of explanatory science is given in Figure 2.

But we are now in a position to realize that the picture is still incomplete. In fairness to mathematics and certain parts of physics, further elaboration of the notion of science is required.

A candid examination of an explanatory science, even one that is highly developed, discloses some significant inadequacies. Rarely is it true that the total body of descriptive symbols can be completely explained in terms of the hypotheses. There are almost certain to be some loose ends, some brute facts, some statements that are descriptively true but cannot be fitted into the scheme. In some cases we may succeed in integrating small groups of the descriptive statements by devising theories that explain them. But these remain islands of order in a sea of disorder. Only rarely can we find a more comprehensive theory that integrates the subgroups with one another just as the simpler theory integrates the statements within the subgroup. All these inadequacies lead us to speculate concerning an ideal state of science in which the logical organization would be perfect and complete. This would presumably be one in which all descriptive statements would be capable of being derived by pure reason from a single theory, this theory itself being so constituted as to be as simple as possible and to involve no internal contradictions. Such an ideal would be achieved in physics, for example, if from a theory of the electron, or some other elementary particle, we could deduce all the phenomena of physics—dynamical, acoustical, electrical, optical, and perhaps even chemical as well.

If, however, we step outside the field of the so-called natural sciences and look about for such a logically complete science, we find one immediately at hand. Geometry (and, in fact, most of mathematics) is precisely such a science. In order to see that this is the case we have only to introduce a substitute terminology. Instead of hypotheses and theories we must speak of axioms, definitions, and postulates, and instead of laws and descriptive statements we must speak of theorems and corollaries. The logical structure is complete, for, at least in a perfect scheme of this type, all theorems and corollaries can be proved in terms of the axioms, definitions, and postulates. But when we consider the abstract character of such a scheme as this, and when we recognize the existence of non-Euclidean geometries, we readily see that the gain in logical cogency has been achieved at the sacrifice of descriptive accuracy. The idealizing activities, which were mentioned earlier, have so dominated the science that it can no longer be said to be about the world at all. The existence of such a science as this shows that the human mind has the capacity for taking at random any group of ideas, actual or fanciful, combining them and deriving their consequences by pure reasoning, and thus constructing a system of underived and derived notions which is internally perfectly consistent, which obeys the rules of logic throughout, but which may convey no information whatever about the world in which we live. Such a science may be called "rational" or "autonomous" and may be represented diagrammatically as in Figure 3.

III

If our analysis of science is correct, each of the sciences should fall roughly into one of these three

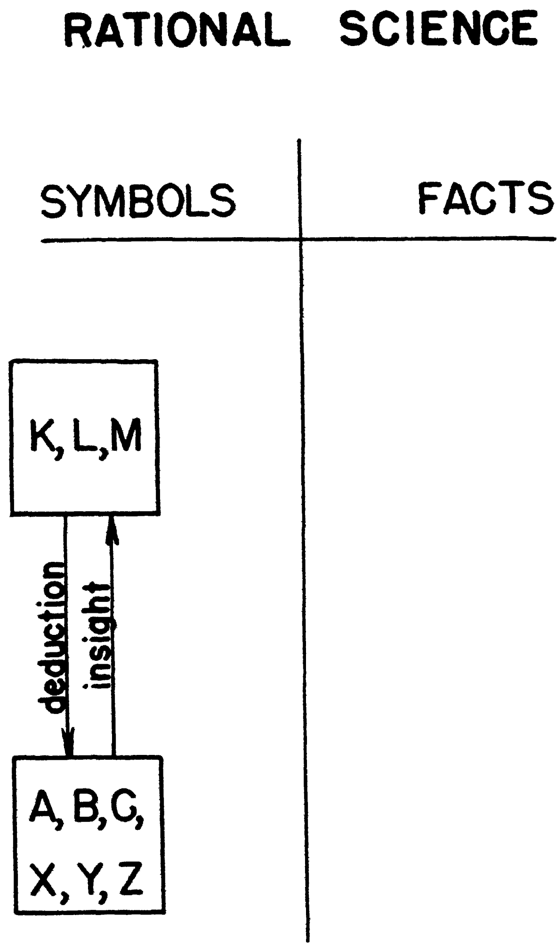
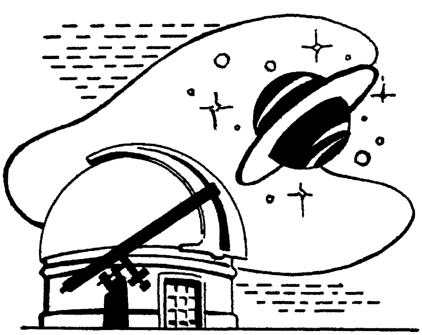


FIG. 3. *K,L,M* are the underived ideas (axioms, postulates, and undefined terms). The axioms and postulates are assumed to be true, and the undefined terms are assumed to be understood. *A,B,C,X,Y,Z* are the theorems. They are proved to be true by deduction through pure reason from the underived ideas.

classes—descriptive, explanatory, or autonomous. Just where it is located will depend on a number of factors—who is doing the classifying, what part of the science is being emphasized, how certain words in the science are to be defined (for example, does "line" mean "something that can be drawn on paper with a pencil" or "length without breadth and thickness"?), the historical stage in which the science is being considered, and so on. Few, however, would deny that autonomous sciences are today well represented by mathematics, rational mechanics, and perhaps other limited areas in physics; explanatory sciences by the rest of the physical sciences and biology; descriptive sciences by the social sciences and psychology, with many, perhaps, preferring to place biology in this class rather than in the class of explanatory sciences. No doubt there is also a *rough* correlation between the development from description through explanation to autonomy, and the actual history of any science. If this correlation were perfect, the autonomous sciences would all be the oldest, and the descriptive sciences the youngest. The fact that geometry was already well developed as a deductive science in the days of the Greeks, and that psychology, which began with the Greeks, is still essentially in its descriptive phase today argues against identifying the logical and the historical developments. No doubt something more than *time* is required for a science to pass from description to autonomy; certainly the character of the subject matter plays a definitive role. It does appear, however, if the above analysis is correct, that a descriptive science remains in this stage only with difficulty. There is, first, the irresistible desire on the part of the human mind to ask the question Why? and to attempt to answer it by engaging in flights of imagination. Second, there is the recognition of the heuristic value of hypotheses in the discovery of new data. To this extent the positivist is wrong in insisting that descriptive science represents science at its maturity. Furthermore, once the value of logical structure has been appreciated fully, there is an unrestrainable desire to idealize the subject matter of the science, break its connections with the world, and set it up as a deductive scheme. To this extent the natural scientist should not look with too much scorn on the purely formal character of mathematics.

The type of science that one prefers is, I believe, largely a matter of temperament. The positivist insists on being close to the facts and avoiding speculative notions; the advocate of explanatory science wants to get behind and beneath things to see how they run; the formalist is unhappy in this higgledy-piggledy world and much prefers the neatness of abstract systems.



ENLIGHTENED AFRICA

LOUISA CLARK WILLIAMS

Mrs Williams (M.A., Chicago) has taught at the University of Wisconsin and studied in Europe as a Marquand Fellow Since her marriage to Dr. Williams, for the past thirty-one years an entomologist of the Hawaiian Sugar Planters' Association, she has accompanied him on expeditions to New Caldeonia and to British East Africa (1947–48). Mrs. Williams is a contributor to various periodicals.

A United States Navy request mission sent us to British East Africa to look for enemies of the giant African snail *Achatina* and to ship suitable ones to the Palau Islands in the Pacific, where these voracious vegetarians were causing serious damage to the local food supply. The big snails swarm en masse over the roads, and even whole hillsides, of these tiny islands, so that cars crunch and skid queerly over them, and the brown slopes seem to be crawling.

While carrying out this mission, Dr. Williams and I "discovered" such an enlightened series of vigorous, well-developed agricultural research organizations in the heart of "Darkest Africa" that my idea, at least, of Africa was revolutionized. This "discovery" would surely surprise many another American who also knows equatorial Africa only from museums, zoos, books on anthropology, and the tales of the big-game safaris. For me, it rivaled in interest the successful hunt for snail enemies.

Many species of *Achatina* have been identified, and it is well established that this snail is indigenous to Africa. The shell is cone-shaped and gray, streaked with brown. About two years after it hatches from its egg (the size of a lentil), the shell grows to be, in most species, five to seven inches long. Some in West Africa grow to be even larger.

The most widely traveled species is *Achatina fulica*. It is native to the lowlands of tropical East Africa, including Zanzibar Island, 25 miles off the coast of Tanganyika. The record of its travels begins on the great island of Mauritius, still farther out in the Indian Ocean. During the past hundred years or more, *A. fulica* has been carried, in general deliberately, by human beings as food for themselves and their poultry, to most of the tropical lands of the Indian and Pacific Oceans.

A record dated about 1857 speaks of an enthusiastic malacologist who took some to Calcutta for purposes of dissection. In 1900 great damage in Ceylon was reported. Similar reports came from Malaya in 1923, Singapore in 1928, and Hawaii in 1938. The Japanese spread *Achatina fulica* throughout their Mandated Islands in Micronesia. Now these hungry snails are reported also from Java, Borneo, Amoy (China), the Philippines, Formosa, and (for a short time) California; a giant snail reported to be *Achatina panthera* is causing great alarm in New Guinea now.

In each new, lush, tropical region this snail has increased rapidly, far from its natural enemies. Imported for food, it is soon eating up more food—growing young food plants—than it could ever supply, even to those with a taste for snails. In 1936 in the Hawaiian Islands, for example, a local Japanese had twelve *A. fulica* brought in through the mail, thus escaping the vigilant agricultural quarantine officers. He was busily raising and eating the snails in his own house until 1938. Then the Territorial Board of Agriculture and Forestry heard about them by accident when another Japanese innocently asked the Board how to raise the big snails, a few of which had been given him by the original importer. Quarantine officers immediately went to the house of the busy snail raiser and there found and destroyed 1,387 *A. fulica* of various sizes, as well as a large quantity of eggs and newly hatched young. Even so, some snails must have escaped, for they soon showed themselves happily established in gardens of the neighborhood, eating, multiplying, and growing fast.

On another island of the Hawaiian group, a Japanese woman imported some *A. fulica* from Formosa. Before they could be discovered and killed, some of these also escaped into the gardens roundabout and went to work. Today, in Hawaii's year-round warm, rainy weather, these big snails have become a serious threat to all vegetable and flower growers. They have been kept down to a threat only by the constant watchfulness of the Board of Agriculture and Forestry, the alarmed reports of their presence, and quick action in destroying all that are reported. Large sums of the taxpayers' money have had to be spent in poisoning these pests. [illegible]

Two views of *Achatina.*

ago concerned only two *Achatina* that had been seen in a garden. Poison was promptly spread by the plant inspectors, and the next morning more than 200 dead *Achatina* were counted in that garden. (The snails come out mainly at night to feed, mate, and lay eggs.)

We can better understand the alarm of vegetable and flower growers over only two of these snails when we realize that just one of them, in a single night, could destroy the results of weeks of hard, devoted work and thus much of the season's income, especially when the plants are young. Poisoning, however, is an expensive and never-ending procedure. It is a losing battle against insect or snail pest. For many years now, the entomologists of Hawaii, concerned with the great sugar and pineapple industries, as well as with the routine food crops, have been using biological control methods to deal with each newly arrived pest, often with spectacular results. When, therefore, the Navy, from its office of Island Governments in Washington, turned to the Pacific Science Board of the National Research Council for help, the entomologists in Hawaii were among the first to be consulted.

Dr. Williams, a veteran of more than thirty years in explorations for this method of pest control by the Experiment Station of the Hawaiian Sugar Planters' Association, was available and was asked to undertake a Navy mission to Africa to discover the enemies there that keep down the numbers of the giant snail so that it never becomes a pest where it is indigenous.

ON THE ISLE OF CLOVES

Dr. Williams and I arrived in Mombasa early in December 1947 and left late in June 1948. For three months of that time, during the dry season when snails and their enemies are estivating, we traveled through Kenya, Tanganyika, and Uganda, consulting the local scientists about *Achatina* and making direct observations. Both agricultural scientists and laymen there knew the monster African mollusk, but only as just another curious wild creature. They were astonished and interested to hear of its wide distribution and its depredations. Dr. E. B. Worthington, Scientific Secretary, Office of the East Africa High Commission—the dynamo at the heart of all the far-flung research of many kinds—in contrast, spoke at once of the need to determine and import an effective enemy of *Achatina* into Singapore and Malaya.

We spent a month on Zanzibar, a low, almost flat, coral island—an Arabian Nights sort of world under a hereditary Arab ruler, the Sultan. The British Protectorate rather encourages the dreamlike quality of life on Zanzibar, even while it maintains order and hygienic conditions and tries to promote better methods of agriculture. It was there that we visited our first agricultural research stations, in fact. (We went there chiefly to ship out *Scolia* wasps, which prey on the coconut beetle *Oryctes,* which is reducing the coconut crop in Micronesia.)

The principal industry of Zanzibar is cloves. There, in a laboratory that was once the home of David Livingstone, a group of five or six trained British scientists of the Clove Research Scheme is working to save the industry that supplies most of the world's cloves—to season your baked ham, to give candies a "vanilla" flavor, and to provide oil of cloves for many kinds of medicine. Dr. F. J. Nutman, the Director, says in a letter:

> As you know, the main wealth of Zanzibar lies in the clove industry, and it is very difficult to think of any other crop which could be an adequate substitute. Furthermore, the locals [descendants of Arabs who started the great

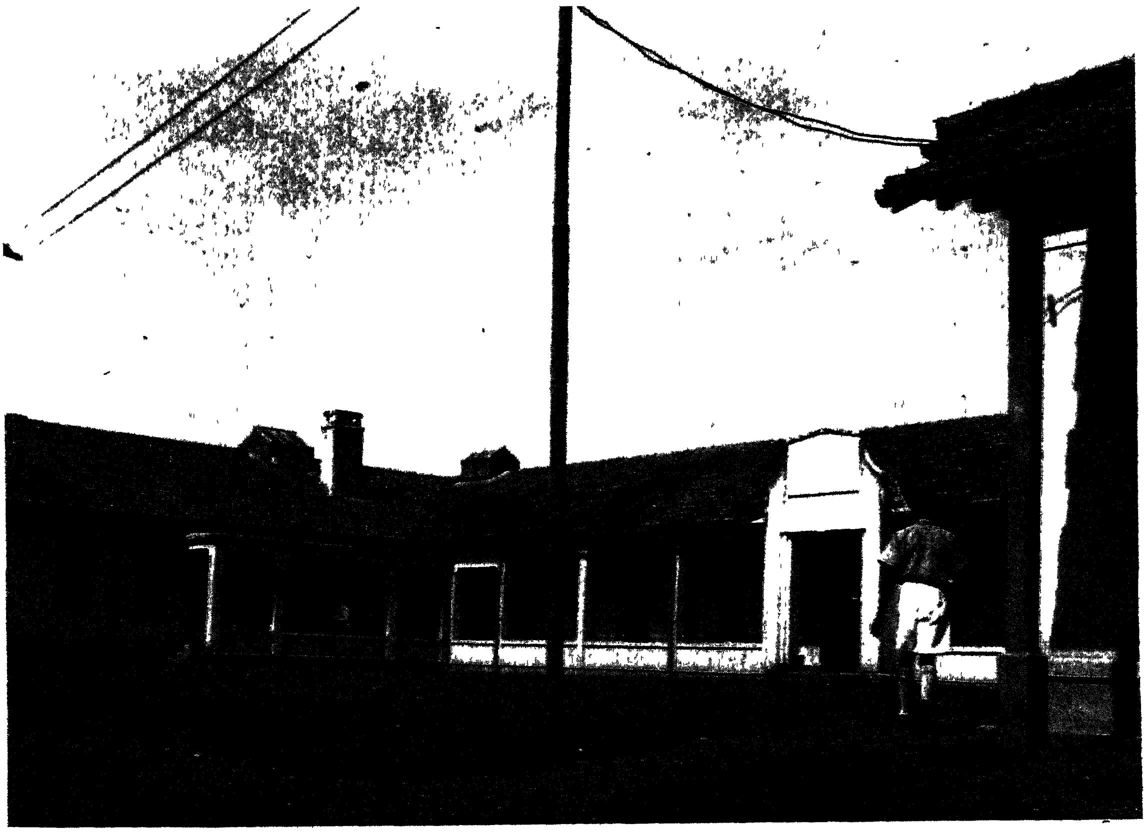
clove plantations around the year 1800] have a kind of psychological attachment to the tree, although I believe this is wearing a bit thin these days. The disease known as Sudden Death has been present for many years—so far as one can tell. But it is only comparatively recently that its seriousness has been realized. . . .

During 1946–7 I studied the disease and drew up a programme of work. This was very thoroughly examined during 1947 by leading authorities in England, and I was able to pay a visit to the States where I received an uncommon amount of help and kindness . . . The British taxpayer is providing most of the money for the next few years.

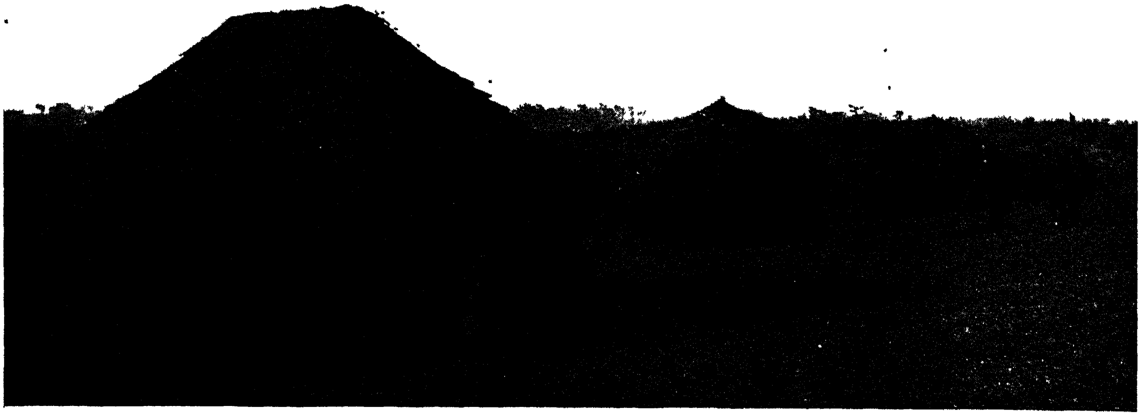
When "Sudden Death" strikes, a great clove tree is one day apparently as vigorous as ever, and three days later a stark, completely dead skeleton. The initial approach to the problem was that the disease might be caused by a virus carried by scale insects fostered by ants. With this in mind Dr. Nutman was studying what had been done on the diseases of elm and citrus trees, which appeared to be similar.

For diversified agricultural research, Zanzibar has the experimental farm and research station at Kizimbani, about 10 miles from the town of Zanzibar. On the way to Kizimbani, we drove through the narrow, crooked streets of the town between stone walls that crowded closely on each side, and soon came to good country roads. These took us through clove plantations, where many great trees had fallen victim to "Sudden Death." Nevertheless, in 1947 the products of the clove tree were responsible for 58.6 percent of the Protectorate's total exports. There were many coconut plantations also, for the coconut palm is responsible for another 33.18 percent of exports. The Kizimbani Agricultural Research Station is on a farm of some 500 acres in the middle of a group of plantations totaling 3,500 acres. The agriculturist and his family live comfortably in a cool stone house of European style. There, of course, we enjoyed the gracious English custom of afternoon tea on the veranda overlooking a wide, immaculately groomed lawn made colorful by old trees supporting flaming bougainvillaea. In contrast to this luxury, the farm buildings and farm work and equipment are kept only a step or two ahead of the local Arab and African practices in order to be of the most use to the most people of the Protectorate.

An important new development is the selection of a crop alternate to the ailing clove industry.



Main office and laboratory buildings of the Kawanda Agricultural Research Station in Uganda.



Barns at Kawanda Agricultural Research Station, Uganda.

Cacao, the source of chocolate, has been chosen. Several old cacao trees of the superior Crillo type have been found still growing on Zanzibar after fifty years of almost complete neglect, as well as occasional fires. These trees, or others of this stock, should supply hardy plants for the new plantations. If not necessary now, this new industry is considered at least an essential insurance policy. Cacao products should be salable under almost any market conditions.

Thus Zanzibar showed us in microcosm the pattern of agricultural research that we found everywhere in British East Africa. A special research organization served the major local crop, and general agricultural research and education dealt with the diversified products—all at government expense. This is the pattern of agricultural development with which we were familiar in Hawaii, although there the two major agricultural industries, sugar and pineapples, support their research privately. Unique for Zanzibar is the fact that, among the various sections in the Department of Agriculture, there is the Plantation Section, which looks after the extensive properties under the control of His Highness the Sultan's government.

RESEARCH IN UGANDA

One of the older and better-developed of the organizations for diversified research in British East Africa is at Kawanda, near Kampala in Uganda. How completely different from Zanzibar is that cool, green, hilly country, four thousand feet above sea level!

We were not surprised to find such a strong group of agricultural scientists where British and other missionary and commercial work has been going on longer than elsewhere in East Africa. We read that "Buganda Province of Uganda is one of the most productive areas in all Africa, being blessed with good rainfall and fertile soils" (Notes on Agriculture in Buganda and on Kawanda Agricultural Research Stations. July 1946. Mimeograph). In Uganda, we were told, there are almost no European farmers. This is quite unlike Kenya, where most of the agricultural work, mainly in coffee and sisal, is on large plantations owned and managed by Europeans using African labor. The only large holdings of any kind in Uganda are the three great sugar plantations, of which two are owned by East Indians and one by an Englishman. Very significant is the fact that cotton, the chief and most profitable crop of Uganda, is all raised by Africans on their small holdings. Here, too, the African farmers raise the other commercial crops, tea, coffee, and bananas.

The entomologist at Kawanda, Dr. W. V. Harris, told us that in 1937 various somewhat independent research departments were combined and established on an old rubber plantation of about 399 acres that had been bought by the government in 1934. Later, 750 acres more were bought for a seed farm. Several of these separate departments—such as the entomology department, which was started in 1908—had been at work for many years.

Today, to house the large British research staff and their families, an attractive village of roomy brick houses and trim gardens has grown up along the crest of the wide-spreading ridge that constitutes the farm, and appropriate cottages for the many trained African assistants are not far away. Extensive modern laboratories and machinery

equip the special skills of this varied staff. The farm buildings, however, as on Zanzibar, are of the local African type, with thatched roofs over mud and wattle walls. And the dairy cattle are only improved strains of the local zebu stock.

The type of basic research carried on at Kawanda is suggested in the Notes:

The system of management has been based upon the use of a grass ley for the maintenance of fertility. As a preliminary for initial cropping most of the cultivable area was planted up with elephant grass after opening in order to restore soil structure. This was particularly important on the land previously under rubber where . . . the soil had become hard and compacted. Apart from the establishment of coffee and other permanent crops there was little development of the farm until 1940. . . .

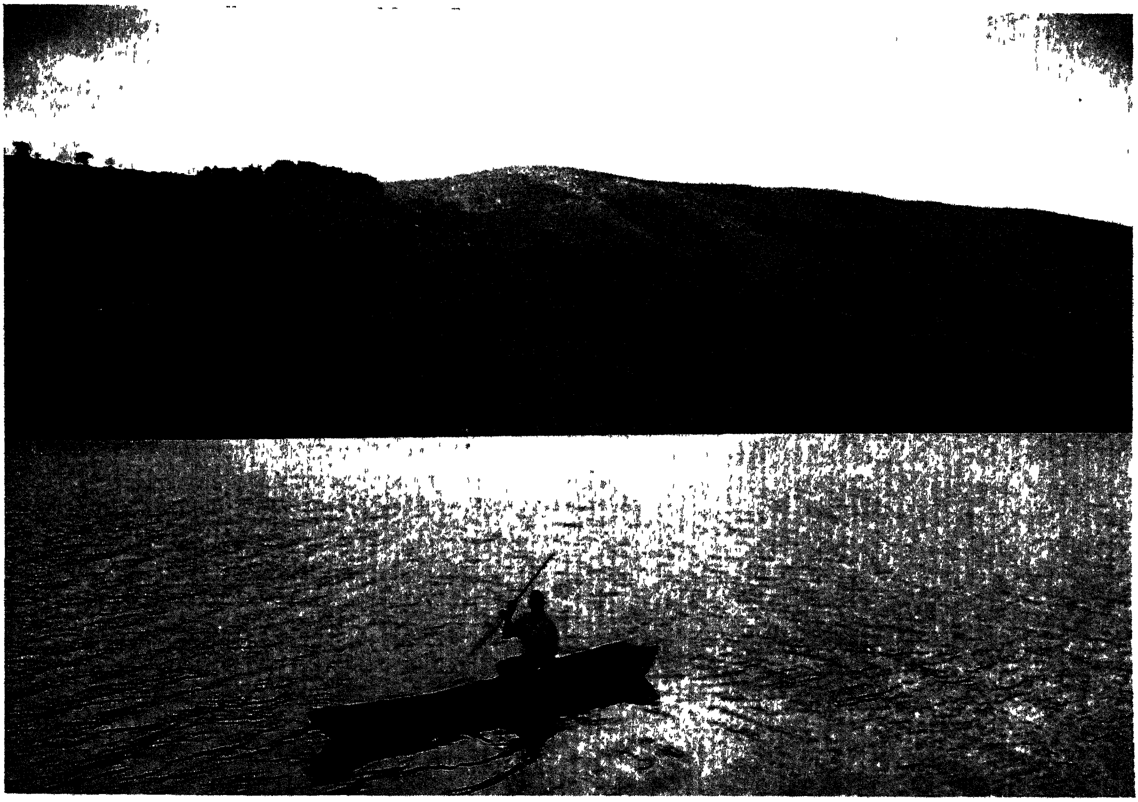
A system of strip cropping with grass leys is followed on all blocks cropped with annual crops. The width of strip varies from 20 to 50 yards and rotations have been designed to obtain information on the Cropping: Rest ratio and the management of the land during resting periods. The cropping periods are from three to four years combined with grass ley of from two to four years. The grass leys used are elephant grass, which is either left undisturbed until brought back into cultivation or grazed in the final year, and selected short grasses on which grazing trials are conducted. Farmyard manure is applied in the first year of the grass ley on certain blocks. The cropping rotations are of an indefinite nature and permit the widest latitude of cropping within a simple framework of legume, straw and root crops associated with cotton.

Cotton being Uganda's principal crop, the research station naturally devotes much effort to methods of its cultivation and to the distribution of improved cottonseed. By-products of the seed, such as oil and seedcake for stock, also receive attention. A cotton gin at the research station handles, and thus can control the selection of, all this cottonseed.

Coffee, of the *Robusta* variety, is important and receives considerable attention from the scientists as to improved varieties, pest control, and cultivation. A well-known factor in improving the flavor, it seems, is not so much whether the coffee is grown in the shade as it is the care with which the last of the pulp around the bean is fermented off. Coffee brings such a good price these days that even the various coffee research projects make a good profit—"for the King."

Still other crops studied—though they are as yet less important commercially—are tea, hemp, annis, peanuts, cinchona, and bananas. The sugar growers' problems also receive consideration.

To spread improved methods of farming, breeds



On beautiful Lake Bunyonyi among the green hills of western Uganda.

of cattle, and varieties of seed, the Protectorate of Uganda is divided into districts, each with a European agriculturist and several African assistants, who make the most direct contacts. At Bukalasa—an older agricultural station—agricultural instructors are trained, and courses are given for chiefs, cultivators, and schoolmasters. A temporary center for training ex-soldiers has also been opened there. Makerere College in Kampala, with nearly three hundred students and a well-developed curriculum, is another center for training native African agriculturists. It has especially strong departments of biology and agriculture. Its students in this latter field spend their last school year (six months in length) in training at the Kawanda Station.

There is evidence that this research and education are taking hold among the small farmers of Uganda in other lines than the production of cotton. For example, proper milk production is becoming popular; and pig raising has increased so much that a bacon factory has been started.

W. V. Harris beside termite skyscraper, details of which he has been studying. Many termite cities are taller than this one. The soil from these mounds packs into an excellent surface for tennis courts or the floors of native huts.

KENYA AND TANGANYIKA

In Tanganyika, lonely, beautiful Kilimanjaro has many coffee plantations on its far-spreading lower slopes. The highest mountain in all Africa (over 19,000 feet), it wears a cap of perpetual snow. Well-watered forests extend below this. Together, the snow and the forests cool and water the country roundabout, making ideal conditions for coffeegrowing. The Tanganyika Coffee Research Station, near Moshi, serves this region.

Mount Kenya, part of a fairly long range, also has a snow-clad peak and a belt of rainy forests to cool and water the many plantations of *Arabica* coffee on its broad slopes. Coffee is grown extensively elsewhere in Kenya Colony. Naturally, then, we found a coffee research station in Nairobi, capital of the Colony. It was located in part of the Scott Agricultural Laboratories. The work of this early establishment is now being transferred to a larger development on a farm of 350 acres some twenty miles from Nairobi, the Jacaranda Coffee Research Station, where everything is still very new. Their scientists, however, have had long experience, and the results of their work, with themselves as the extension agents as well as the research scientists, are in great demand wherever coffee is grown in Kenya.

The most conspicuous achievement of the entomologists, R. H. Le Pelley and A. R. Melville, is the control of a certain very destructive mealy bug on coffee, *Pseudococcus kenyae*. First noticed as a serious pest in 1923, this mealy bug, like many other creatures in Africa, was new to the scientists. In search of an insect to destroy it, Dr. Le Pelley traveled for a number of years in all parts of the world. He sent back to Nairobi, from four continents and several large tropical islands, enemies of mealy bugs. But nothing happened in the coffee plantations of Kenya Then, in 1938, the two entomologists began looking around in Africa. Perhaps, they began to think, this mealy bug was a strictly local insect. Perhaps, therefore, its enemies would be strictly local, too. They were right. Mr. Melville discovered, practically next door in Uganda, a "pocket" of exactly the same kind of mealy bug and enough of its enemies to make a start in the battle against it. These enemies, imported into Kenya's coffee plantations, soon had the mealy bug under control. The most effective of several enemies imported was *Anagyrus* near *kivuensis*. Since then the chief work of that part of the Scott Laboratories has been to breed these beneficial insects and liberate them in coffee plantations.

A whole delightful week as the house guests of Dr. D. W. Duthie, the Acting Director of the East African Agricultural Research Institute at Amani, gave us a thorough view of its plant and some of its work. Amani is one of the central research institutions serving all of British East Africa. Though its work is mainly in agriculture, it is equipped to turn to other fields when necessary—such as during World War II.

The Germans gave expression to their love of

the picturesque by planting a vast horticultural garden in the midst of virgin jungle on the steep mountainsides. When the British took over Tanganyika after the first world war, they eventually made many improvements. Now fifteen comfortable dwellings and ten work buildings—laboratories, greenhouses, a library, and an administration building—perch on the various ridges, each commanding a magnificent view. The extensive grounds are beautifully arranged and always well groomed. Trim hedges separate the lawns, rose arbors, flower beds, and pools from the tremendous jungle that encroaches on three sides. Two-hundred-foot treetops tower over this rather prim bit of civilization, and monkeys swing on loops of great vines.

At its largest, Amani had a staff of sixteen European scientists. The number is fewer now, mainly because the excellent and experienced men of Amani are so much in demand for important posts elsewhere. For example, Dr. H. H. Storey, F. R. S., is now in London as Secretary to the Colonial Agricultural Research Committee. The most recently published report of research there covers the war years 1942–45. Much of the normal agricultural research was in abeyance, but those of the staff who remained carried out wartime assignments such as the following:

Camphor. As a result of successful pilot-plant experiments, a camphor factory for the exploitation of *Cinnamomum camphora* was erected at Lushoto by the Amani staff in 1942. The Tanganyika Railways designed and supplied most of the essential machinery. The factory was closed in July 1943, the supplies of camphor wood having been exhausted.

Ceramics. Following the demand for pottery utensils and containers, experiments were undertaken in 1942 by J. Glover, assisted by Mrs. L. Goldstucker, with a view to producing these from local materials. Preliminary work showed that satisfactory unglazed ware could be made from local clays and that African labor could be taught to use the potter's wheel. Prolonged investigation in the laboratory was necessary before suitable glazes were found to fit these local clays and those submitted by pottery workers in Kenya.

Chenopodium oil. This was produced on a laboratory scale by J. Glover from *Chenopodium ambrosioides* var. *anthelminticum* grown at Amani. The sample complied with the United States Pharmacopoeia standards but not with those of the British Pharmacopoeia.

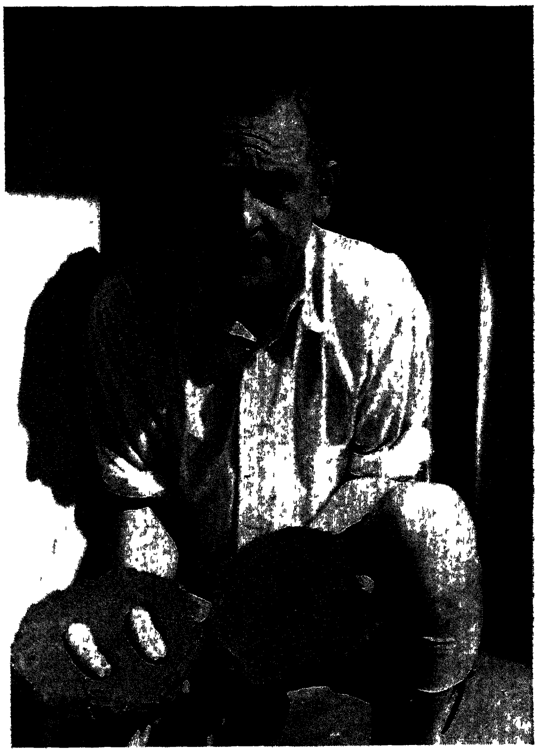
Creosote. R. R. Worsley experimented with the production of creosote oil from producer-gas tar, previously considered a waste product. He produced a disinfectant of the Jayes' type, the formula for which was supplied to a Nairobi firm for manufacture and supply to His Majesty's Forces. Medicinal creosote was also prepared. Crude creosote from local wood tar was shown by J. Glover to give satisfactory protection to wood against two common species of termites.

Dehydrated castor oil. A lengthy investigation was undertaken by J. Glover to discover a substitute for linseed oil and similar drying oils used in paint manufacture. A method was evolved for preparing dehydrated castor oil, using East African kaolin as a catalyst. It was shown that a range of satisfactory drying oils could be prepared by this method, details of which were published in *Technical Pamphlet No. 3* of the East African Industries Technical Advisory Committee.

Petrol from oil seeds. R. R. Worsley spent a considerable amount of his time during 1942 in perfecting a process for the manufacture of high-quality petrol from oil seeds. The process was patented in the United Kingdom.

Glycerin, fatty acids, and stearine. R. R. Worsley experimented with a specially constructed autoclave and demonstrated that coconut and cottonseed oil can be split satisfactorily, on a pilot-plant scale, to produce good quality crude glycerin and fatty acids suitable for soapmaking. The stearine obtained from the splitting of cottonseed oil made good candles. The hydrogenation of cottonseed oil free fatty acids, prepared by the autoclave process, was successfully carried out on a laboratory scale, and soap made from the hydrogenated product; this soap was hard enough for normal household use.

In the course of time Amani has proved to be too picturesquely—and not conveniently enough—placed for such an important government service. Arrangements had been nearly completed when



Dr. Harris, entomologist at Kawanda Agricultural Research Station, shows two vast termite queens in the kind of mud cells in which they live, each pouring out eggs for her own skyscraper termite city. The termite king is a midget by comparison, and the workers and fighters are the size of large ants.

we were there whereby the Amani staff will be absorbed into a wider group, the East Africa Agricultural and Forestry Research Organization. Amani will be continued as a substation.

OTHER RESEARCH

The Rockefeller Foundation has several research groups in Africa working on the tropical diseases malaria and yellow fever. For years the British government has been studying the devastating tsetse fly. We did not happen to see any of these flies, but occasionally our car was required to stop and be searched for them, mainly under the chassis. Once in Uganda, it had to be driven into a snug shed and fumigated.

The Yellow Fever Research Institute has its main laboratories at Entebbe in Uganda on Lake Victoria. A. J. Haddow is the entomologist in charge. Two hundred miles away, east of the "Mountains of the Moon," or the Ruwenzories, where the Belgian Congo begins, we found the other part of this Institute of the Rockefeller Foundation. We came upon it at Bundibugyo in the Bwamba Forest, where the Africans are extremely primitive. As we cruised through the village, we found no indication of the laboratories, except a somewhat tidier wall of elephant grass surrounding a compound, rather larger than most and having unusually snug and thick thatch on its houses. Inside the houses, however, nothing was primitive. Dr. Lumson, the entomologist on duty, showed us excellent laboratories.

Local monkeys and other wild creatures carry the "jungle yellow fever," but are immune to it themselves, thus serving as vectors. (Two pockets of "jungle yellow fever"—in the Belgian Congo and the Amazon River jungles—continue to harbor this disease.) Monkeys from India not being immune, they are tethered on long platforms in the treetops of the Bwamba Forest, where they keep company with the yellow-fever mosquitoes and the local monkeys. After becoming infected, they are brought to the laboratory to live comfortably while blood samples are studied and other observations made.

The East Africa Agricultural Research Station at Amani. Administration Building in background, with a hill

MISSION ACCOMPLISHED

Returning to the coast, Dr. Williams worked from mid-March to late June in the Mombasa region inland from Diani Beach. Here the snails were abundant—the exception rather than the rule in the part of Africa we saw, for the big snails generally were scarce. By great good luck, a well-kept little seaside hotel was near by, allowing us to rough it quite smoothly. The long rainy season, due in March, had scarcely begun in earnest by the end of May. Thus neither the snails nor their enemies were at their full activity. The snail enemies Dr. Williams had expected to study were carabid (*Tefflus*) and drilid beetles. During December in Mombasa, he had found drilid moult skins inside empty *Achatina* shells, suggesting the identity of the killer. He had also found a live drilid larva and an adult carabid in the act of eating a live *Achatina*. Each had its head so deeply pushed into the snail shell and was so absorbed in eating that no amount of har.dling disturbed it.

The unexpected snail enemies at Diani Beach —the carnivorous snails *Gonaxis* and *Eduntulina* —were more conveniently studied in the lightly rainy weather. Dr. Williams' investigations, in the laboratory and afield, convinced him that these are largely responsible for keeping down the numbers of *Achatina* in Africa—at least in the districts he studied. Dr. Joseph C. Bequaert, of the Museum of Comparative Zoology at Harvard, the specialist in land mollusks of Africa who was consultant to this Navy mission, supports this conclusion, arrived at independently.

ACKNOWLEDGMENTS

Since any significant piece of work is always the product of the combined efforts and skills of many and varied persons, it seems fitting to make suitable acknowledgments.

To the staff, and achievements, and facilities of the Experiment Station, Hawaiian Sugar Planters' Association in Honolulu, for their pioneer and still leading work in biological control of tropical insect pests.

To the United States Navy for its intelligent, farseeing concern for the welfare of primitive peoples it is called upon to govern.

To Mr. Harold J. Coolidge, Executive Secretary of the Pacific Science Board of the National Research Council, whose office was home base for this Navy request mission.

To the Insect Control Committee for Micronesia, of the Pacific Science Board, Mr. Cyril E. Pemberton of the Experiment Station, Hawaiian Sugar Planters' Association, chairman.

To Mr. J. William Henry, American Vice Consul in Mombasa when we arrived there, and to Mr. Robert M. Taylor, American Consul in Nairobi.

To the many helpful and hospitable scientists and officials of British East Africa, especially Dr. E. B. Worthington, Scientific Secretary, Office of the East Africa High Commission.

To the many delightful little country hotels and to the Manor and Carlton Hotels in Mombasa, which together enabled us to travel so lightly equipped and so comfortably.

And to the South African Airways and the British Overseas Airways Corporation, which made such special arrangements for our flying home with a live insect and snail menagerie. In London we even adopted an unaccompanied shipment of live chameleons on their way to the Chicago Natural History Museum.

SCIENCE ON THE MARCH

A REVIEW OF PROGRESS IN TROPICAL MEDICINE

A RECENT article in "Science on the March" by Dr. Eugene H. Payne (October 1948) recalled a number of notable advances in tropical medicine that have been made during the past few years. It is the purpose of this report to supplement those remarks with a description of still further progress in this branch of the medical sciences. The particular tropical diseases to be considered are plague, yellow fever, amebiasis, and hookworm disease.

Plague: Plague is an acute, infectious, highly fatal disease characterized by fever, blood-stream infection, and hemorrhages into the skin, subcutaneous tissues, and internal organs. It is caused by an organism called *Pasteurella pestis,* and is little known by most people living under the protection of the United States. If we think of it at all, it is usually as an Oriental pestilence that periodically scourges the underprivileged peoples of the Far East, or as the Black Death, which ravaged Europe from the fifteenth to the seventeenth centuries. But plague is still an important disease today; in our modern times it is frequently rampant in India, China, Manchuria, Burma, Africa, Madagascar, and parts of South America. Furthermore, it is definitely not a disease entirely unknown to us in the United States. Although the epidemic that broke out in San Francisco in 1900 appeared to die out subsequently, the disease has assumed increasing significance, particularly during the past fifteen years. It will be of interest to look a little more closely into this situation in our own country.

Plague is primarily a disease of rats and other rodents and is transmitted from them to man by various fleas. During epidemics, it is principally the rat (brown sewer rat, or *Rattus norvegicus,* and the black house rat, or *R. rattus rattus*) that is involved. As a reservoir of plague infection it is more and more apparent, however, that certain wild rodents—in the United States especially the ground squirrel (*Citellus beecheyi*)—play an important part. All the recent human cases of plague in North America have resulted from association with such rodents, not from contact with rats. Fortunately, these deaths have been of only sporadic, but there are potentialities for an increase. The known area of infected wild rodents includes fifteen Western states. Surveys in these states have incriminated nearly forty rodents, including some fifteen species of ground squirrels, flying squirrels, wood rats, kangaroo rats, field rats, prairie dogs, chipmunks, marmots, and cottontail rabbits. The western badger, a mammal, has also been found infected with *Pasteurella pestis.* The demonstration that certain fleas that can transmit the disease are permanently established in Iowa, Ohio, Nebraska, Michigan, Washington, D. C., and other localities raises the possibility of wild rodent infection in these places too.

There are two principal factors by which plague is perpetuated in wild rodents. One is the occurrence of a low-grade, latent or hidden infection in the rodents which may last for as long as two months, during all of which time fleas can become infected. The other factor is the long period for which such infected fleas can remain infective for other rodents and for humans. Investigators have demonstrated that under certain conditions fleas can remain infective for periods up to five months and, in one experiment, for 396 days! Because of these facts, sporadic cases of human plague from contact with wild rodents and their fleas will undoubtedly always occur in the United States in spite of all efforts to eradicate them. With modern methods of sanitation in effect, however, there is little danger of a widespread epidemic.

Sulfadiazine has been found effective in treating human plague The new antibiotic drug streptomycin is perhaps even more efficacious, but it is still too expensive for routine use. The present treatment of choice, until streptomycin becomes more generally available, is a combination of sulfadiazine and a serum prepared from rabbits by inoculating them with living but not virulent plague organisms. The serum has been developed by Dr. Karl Meyer at the University of California's Hooper Foundation.

Yellow fever: Yellow fever is another tropical disease that is little known to residents of the United States. It is of importance in Brazil, the Amazon basin, Colombia, Venezuela, and in its original African home—the Anglo-Egyptian

Sudan and Congo River Valley. It is an acute infectious disease caused by a filtrable virus and transmitted from person to person (or from animal to animal) by mosquitoes of the genera *Aëdes* and *Haemagogus*. There have been no cases of yellow fever in the United States since the early 1900s, owing to the control of the mosquito *Aëdes aegypti*.

In Brazil, widespread vaccination and mosquito-elimination campaigns carried on at first by the Rockefeller Foundation and then by the Brazilian National Yellow Fever Service have caused the disease to disappear from populated areas. But Brazil still has a yellow-fever problem in her vast jungle areas. In these locations, even when *Aëdes aegypti* is not present, persons whose occupation carries them into the forested jungle frequently acquire the disease. This is important because such infected individuals, by visiting a community where the mosquito *Aëdes aegypti* is present, can be the source of new outbreaks. This, as a matter of fact, has happened in Brazil and other South American countries along the northern coast. In our modern age of swift air travel, it is entirely possible that yellow fever, once so started, could be carried to the United States and other distant communities.

The exact nature or origin of this "jungle yellow fever" has long been debated, but evidence has accumulated which demonstrates the existence of the infection in other vertebrates and in arthropods. Recently, for example, it was conclusively shown in the Bahia Province of Brazil that four different species of marmosets, as well as other monkeys, have the yellow-fever virus. Jungle mosquitoes such as *Haemagogus spegazzini* presumably transmit the disease from monkey to monkey and, if the opportunity presents itself, to man.

As a result of such findings, there are some scientists who now believe that yellow fever may be essentially a disease of lower animals, with periodic outbreaks in man. If this should be true, the situation would be rather analogous to that just described for plague.

Vaccination continues to be a highly effective preventive measure in yellow fever. The so-called American "17-D" strain continues to be the vaccine of choice. During the course of recent surveys in Colombia, it was discovered that five years after vaccination with strain "17-D" 93 percent of the people still showed evidence of the effectiveness of the vaccine.

Amebiasis: The recent tropical medicine review by Dr. Payne remarked that, although some 10,000,000 people in the United States harbor the protozoan parasite *Endamoeba histolytica*, very few develop amebic dysentery or other forms of amebiasis. Speculation as to why some persons develop amebiasis and others do not is intriguing, but so far most of the questions remain unanswered. It is not known, for instance, why approximately 87 percent of reported cases are males and only 13 percent females. The role of alcohol consumption is not understood, either, but various surveys report 52–75 percent of patients to have had a more than average alcohol consumption over a period of time.

Nutrition is doubtless important as a factor in developing amebiasis, because E. C. Faust several years ago demonstrated that in general carbohydrates provide opportunity for the amebae to multiply, but animal proteins reduce this capacity. He found that in fresh raw liver, particularly, there is an unknown factor that tends to make the disease subside. In his experiments it was noted that amebiasis was controlled equally well whether the liver was given by mouth or ground up and administered in an enema solution. In spite of this work, physicians have not generally considered nutrition seriously when treating amebiasis with the more specific drugs available.

Another theory has been advanced that there is a close connection between certain bacteria and growth of amebae. For example, the presence of hemolytic streptococci seems to favor growth of the amebae, whereas *Lactobacillus acidophilus* impedes such growth. It is believed that the lactic acid of *Lactobacillus acidophilus* is the inhibiting substance. In one clinical trial of this theory, patients were given an anhydrous form of lactic acid, "Trilactic," as their only treatment. There was only moderate success, but sufficient to demonstrate that this was a likely factor in growth of amebae. The work is important because experiments such as this point the way for further investigation. Eventually we shall arrive at a treatment that will replace the not-too-effective drugs now in use. These drugs—emetine, diodoquin, chiniofon, and carbarsone—each have an important place in the treatment of the disease, but no one maintains that they have by themselves solved the problem of treating amebiasis.

One recent report regarding the transmission of amebiasis is of interest because it suggests that other means than "food, fingers, flies, fomites and water" may be important. Researchers in Talara, Peru, made cultures of the legs and intestinal contents of 100 cockroaches. No amebae were found in specimens from the legs of cockroaches; they were found, however, in 7 percent of the cultures from the intestinal contents.

food and utensils could therefore be contaminated by the amebae from the feces of cockroaches and that the common cockroach can be incriminated as a carrier of the parasite (at least in northern Peru). The high incidence of amebiasis in other tropical areas may also be partly due to heavy cockroach infestation, but it is not likely that this is an important factor.

Hookworm disease: Hookworm infection remains a serious public-health problem in the United States, as well as in the tropical portions of the world. It does appear, though, that the incidence is gradually decreasing as a result of mass treatment and repeated programs of health education. For example, in Florida, the first survey taken under the auspices of the Rockefeller Foundation in 1910 showed that 58.1 percent of of 6,155 persons examined had hookworms. In a recent study of 8,017 white school children, 40 percent had hookworm infection, but only one tenth of these had moderate or heavy intensity, Figures such as this are still much too high for a civilized country and indicate the need for an even more comprehensive program of better health along with hookworm control per se.

Hookworms live in the human body for five to eight years. Each worm sucks almost 1 cubic centimeter of blood from its human host each day. If only 10–25 worms are present, there is a loss of only 10–25 cubic centimeters of blood daily. If, however, 500 worms are present, there is too much blood loss to compensate for, and the individual rapidly develops an anemia. It is therefore of value for the physician to know just how many hookworms his patient may have when he first sees him for he will be better able to judge the seriousness of the infestation. This is done by counting the eggs passed in the stool of the individual. It has been ingeniously determined that each female hookworm produces 50 eggs per cubic centimeter of stool per day. By using this figure and applying it to equally ingenious short cuts in the egg-counting, the physician can tell approximately how many worms the patient is harboring. By repeating the counting of worm eggs after treatment has been administered, the exact effect of the treatment can be determined.

It should be noted that, although this article touches on a few high lights of the progress made recently in the field of "tropical" medicine, the diseases are not at all restricted to tropical areas. Amebiasis and hookworm disease are cosmopolitan in their distribution. Their incidence in the United States is high. Human plague, as pointed out, is rare in this country, but there is a huge wild rodent reservoir that keeps the disease going in an area of ever-increasing size in the United States. Yellow fever fortunately does not occur in this country now; however, it has flourished in the past in areas high up on the Atlantic coast and in the Mississippi Valley, and if there should be a serious break in our health control program, it is possible that yellow fever would again become a problem. In short, these diseases should really not be called "tropical" diseases at all; they are for the most part diseases that also occur in temperate climates but not to the same degree. There is a strong correlation between the occurrence of these diseases and the general hygienic conditions and state of nutrition of the people. If sanitation in the tropics were on a par with sanitation in the United States, such diseases would necessarily decline greatly in frequency. Progress is being made—real progress —sometimes with huge strides. But much remains to be done.

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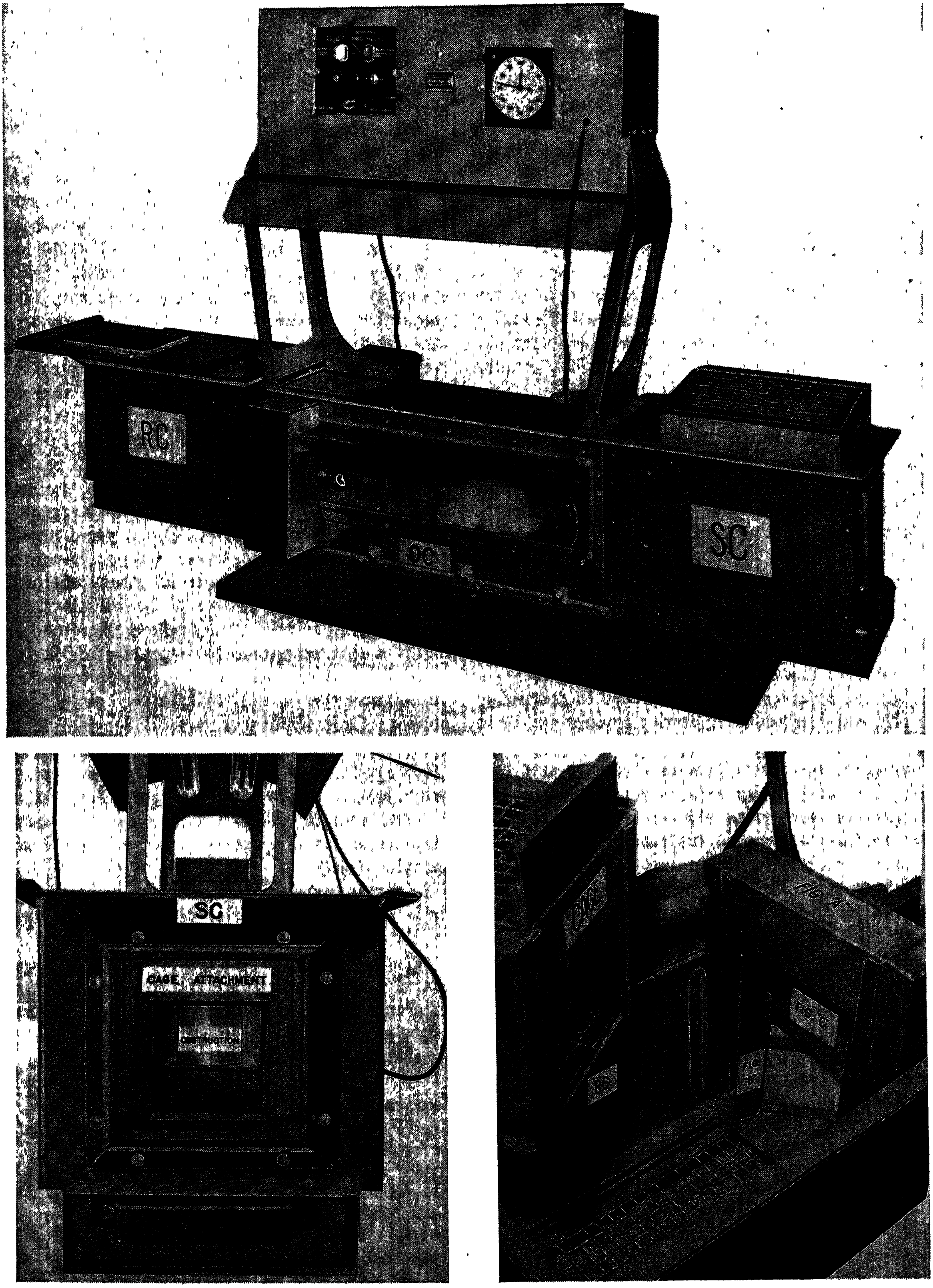
TESTING REPELLENTS FOR RODENT CONTROL

CAN rats be used to aid man in his war against the rat? This is the question raised by the Office of the Quartermaster General. Of considerable concern to the armed forces during the war and to civilian economy as well is the tremendous destruction of food and materials by rodents. It was estimated that rodent destruction cost about $189,000,000 per year in the United States prior to the war. It has also been reported recently by the U. S. Fish and Wildlife Service that the rodent population in the United States is growing at a more rapid rate than the human population. These are impressive facts, and they indicate the urgency of methods for rodent control. To date no completely adequate method of rodent control has been developed. There are many rodenticides and repellents available to the desperate purchaser; there is no way of knowing which repellents are effective, however, although there is little difficulty in determining the effectiveness of rodenticides.

In many situations rodenticides or toxic baits



The Pittsburgh Obstruction Unit

are desirable; on the other hand, there are many situations in which it is not feasible to employ traps or poisons. Repellents can be used in conjunction with toxicants. The repellent may drive rats from otherwise inaccessible places to more convenient ones, where poisons could be used with little risk or where the animals might be trapped.

In June 1946, a research program, sponsored by the Office of the Quartermaster General under a research and development contract, was started at the University of Pittsburgh by the Department of Psychology. The purpose was to develop "standard laboratory methods for evaluating the effectiveness of proposed rodent repellent substances furnished by the QMC." The project has resulted in the development of (1) a testing instrument, the Pittsburgh Obstruction Unit; (2) a method of motivation control for the rat; (3) a set of standard training and testing procedures for use in preparing and in selecting rats for repellency and barrier tests; and (4) a scaling technique adapted to evaluating tested materials.

The Pittsburgh Obstruction Unit has been described in the June 1948 issue of *The Journal of Comparative and Physiological Psychology*. The unit provides for the mounting of a physical barrier of any construction; in addition, it is possible to seal it for tests of vaporous materials or liquids. So far the research has been devoted to the testing of physical deterrents. A barrier when mounted in the unit blocks the rat's path to food. The barrier is mounted on a metal frame in such a way that two sides and an intervening angle project into the middle section of the unit facing the rat. The top and bottom of this passageway are contiguous with the barrier so that the animal cannot attack these surfaces.

Animals are prepared for training by restricting their caloric intake in such a way that a chronic hunger, with inanition, is produced. A standard set of barriers of progressive difficulty is presented to each animal over of period of twelve days. Following this, the rat is tested with a standard barrier for twelve more days. Both trapped wild gray and laboratory-raised albino rats have been used in equal numbers. Both males and females were used, and grays and albinos were matched for weight so that comparisons of the groups could be made.

The obstruction unit contains an electronic circuit with a photoelectric-cell switch and a chronoscope. This circuit makes possible the measurement of the animal's attack time, and it is these records that are used in evaluating the rat's performance on the standard barriers and the rodent resistance of candidate materials. The recording of time is terminated when the animal makes a hole large enough to permit it to get through to the food.

Following the twelve days of testing on the standard barrier, those animals that were above the average in rate of penetration were given additional training and then employed in tests of treated materials. In all tests of treated materials a standard untreated barrier was also used as a control. Most of the treated barriers have been supplied to the project by the U. S. Fish and Wildlife Service; some, however, have been prepared by the project personnel from materials suggested by the Wildlife Service as a result of preliminary screening tests. The method appears to work well, and no basic changes have been necessary in the approach. In all cases none of the materials tested has proved to be sufficiently rodent-resistant to prevent a single well-trained and highly motivated animal from penetrating it.

Some of the materials that have been tested are: dithiobiuret; *n*-phenyl maleimide; pentachlorophenor; Onitrochlorobenzene; dodecylamine hydrochloride; Vinylseal (A-70); Piccoumaron (452-S); latex; bis (5-chloro-2-hydroxyphenyl sulphide); alpha-picoline picrate; diethylamine picrate; sodium silicate; duco lacquer (white); copper napthenate; processed tung oil; and Lucite (acrylic resin).

Of the many wild gray and albino rats used in these studies, comparisons of mean attack times on the standard barrier indicate that there is a very small difference between the wild and domesticated rats and that there is also a negligible sex difference. For these reasons it appears that any hardy and vigorous strain of rats can be adopted for this type of bioassay.

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BOOK REVIEWS

EXPERIMENTAL PLANT SCIENCE

Vernalization and Photoperiodism. A Symposium. A. E. Murneek and R. O. Whyte, with others. vii + 196 pp. Illus. $4.50. Chronica Botanica. Waltham, Mass. Stechert-Hafner. New York.

THIS symposium consists of fourteen papers by several authors who have been active contributors in the fields represented. In general, the various topics treated are summarized, sometimes with attempts at critical evaluation of the data, and sometimes not. Much of the evidence presented still leaves any interpretation of it on a controversial basis. This is especially true of the chapter that deals with vernalization.

The approach in many of the papers is from the viewpoint of experimental ecology. Few detailed data which deal with an analysis of the problem of photoperiodic response from the viewpoint of cellular physiology or biochemistry are presented. A brief presentation of some such data are given in the paper on thermoperiodism. In some cases the few data which are given are dismissed as irrelevant or are mutually contradictory.

Nearly all the papers postulate the presence of a hormone which, although not yet isolated, is presumed to account in some fashion for many of the results recorded. The evidence adduced is of interest and may yet lead to the isolation of some specific substance or substances, but, since present-day knowledge on the physiological cellular response to hormones is meager indeed, any explanation on a hormonal basis results principally in an attempt to present a basic explanation of one unknown in terms of another.

Calling attention to the responses of plants to alternating or nonconstant conditions rather than to a constant environment is of great service to those who attempt a more detailed comprehension of plant behavior. Many more studies and observations of that type are needed. Such periodic response may well be found eventually also in relation to variations in moisture supply, nutrient supply, and various other environmental factors to which successful horticulturists and plantsmen have given serious attention for many generations.

The symposium aggregates many observations of value to those who deal with plants on an economic basis, but offers less to the physiologist interested in plant functions or processes, especially from the standpoint of cellular physiology or biochemistry. Although changes in photoperiod may be used as an ecological factor to bring about reproductive phases in some plants, it offers no universal explanation; there are too many plants that are day-neutral or nonsensitive. Instead of casting aside as irrelevant the few cases in which a nutrient or internal relationship has been experimented upon, and the vast amount of field experience which bears on this point, it would be more profitable to carry on many more detailed experiments relating to such possible relationships under various ecological conditions, and with plants sensitive in varying degrees to such imposed variable environmental conditions. There seems to be no basis for any universal generalization as yet that photoperiodism is the sole controlling factor of the vegetative or reproductive states in plants.

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THE HOPKINS

The Story of The Johns Hopkins. Bertram M. Bernheim. xi + 235 pp. Illus. $3.50. Whittlesey House. New York. 1948.

THIS book should be of great interest not only to medical graduates of The Johns Hopkins but also to anyone interested in the progress of medicine and medical education in the past sixty years. Dr. Bernheim is himself a graduate of The Hopkins—Class of '05—and has lived in Baltimore, where he practices surgery, ever since. When he went to medical school the idea of medicine, not as an apprenticeship but as a postgraduate study with especial stress on theory and research, was still quite new in this country, and he had the privilege of working under all four of the great doctors and pioneers in this type of teaching who made the name of The Hopkins Medical School famous all over this country—and indeed over the whole medical world.

He traces the growth and development of the school from its very inception and earliest years down to the present, discusses and evaluates the share the various members of its staff have had in shaping its policy, and relates the new things in medicine that were developed by the research work encouraged by them. He shows how the policy of the surgical department led to the growth of the other famous Baltimore hospitals and traces the history of the Hopkins unit and the influence of Hopkins doctors in World Wars I and II; he does not neglect the various branches, such as the Phipps Clinic, the Brady, the Wilmer Institute, the Welch Institute of the History of Medicine, and the School of Nursing. The last chapter is devoted to a discussion of the future of the institution. As Dr. Bernheim writes in a very vivid, easy, and colloquial style, and points up his narrative with a

wealth of entertaining and pertinent anecdotes, the book is delightful reading as well as being most interesting and informative. Dr. Bernheim's conclusions reflect the serious thought that he has given to his subject.

There is only one criticism that can be made—Dr. Bernheim, as a surgeon, has given full credit to the achievements of the surgeons and internists, but he evidently failed to realize the great influence that Dr. John Howland has had on the development of pediatrics and his role in training the present-day leaders in this field.

WILBURT C. DAVISON

School of Medicine
Duke University

MINERALS AND PLANTS

Mineral Nutrition of Plants and Animals. Frank A. Gilbert. 131 pp. Illus. $2.50. Univ. of Oklahoma Press. Norman.

THE mineral nutrition of plants and animals is dealt with competently and briefly, almost in summary form. A short discussion at the end suggests conclusions about the importance of the elements in soil fertility and human nutrition.

After a very brief historical introduction and a short chapter on the classification of the elements significant in nutrition, the author takes up each element individually. These compact statements make up most of the book. Ten pages are given to phosphorus, eight to calcium, and smaller sections to magnesium, potassium, sulfur, iron, copper, cobalt, manganese, zinc, iodine, boron, molybdenum, aluminum, silicon, sodium and chlorine, fluorine, and arsenic, lead, and selenium.

The book might have had a little wider usefulness if some material had been included on the occurrence and availability of the nutrients in soils, especially as a basis for the final chapter. In this chapter the author may somewhat oversimplify the problem of changing the mineral composition of plants with fertilizer. On some soils the content of certain mineral elements in plants growing on them can be increased by adding ordinary fertilizers containing the elements. But often this cannot be done, at least significantly. The problem of improving the nutrient quality of food crops through fertilization and other management is almost unbelievably complicated. Fortunately, research is going ahead in this field.

For those who want a brief, authoritative summary of the functions of elements in plants and the most significant relationships to animal nutrition, this is an excellent book. It is dignified, yet easy to read. The book contains a valuable bibliography of some 329 references. It is well printed and properly indexed.

CHARLES E. KELLOGG

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OF SHIPS

The Story of the Ship. Charles E. Gibson. xiv + 272 pp. Illus. $4.00. Schuman. New York.

The Merchant Ship: Design Past and Present. G. S. Baker. 159 pp. Illus. 12*s.* 6*d.* Sigma Books. London.

GIBSON has written an outstanding book. It is not only a story, but a history, of how the great vessels of today developed from the first primitive floating log. Mr. Gibson, himself a sailor who loves and understands ships, has collected all the lore and studied all the laws of shipbuilding through the ages, from 4000 B.C. to the present day. He traces with a light touch the gradual improvements upon the plank and sail through the Egyptian and Assyrian eras, with their early navigational instruments, to the merchantmen of Phoenicia, who sailed from Cadiz to Carthage with their rich cargoes, on through the Greek mastery to the rise of Rome and the beginnings of Western ascendancy. Each civilization left its mark upon the ship until in the fifteenth and sixteenth centuries the great navigations of Columbus and the Cabots, of Hawkins, Magellan, and Drake, opened up the vast Atlantic and Pacific to the ship, with guns and gunpowder changing its design again in the sixteenth century.

"Canvas and cannon," as Mr. Gibson puts it, made every vessel into a warship, and during the Middle Ages any merchant ship could and did so function. He shows very clearly when and why the division between merchantmen and warships was made and how competition mothered the necessity for steam until at last it triumphed. All the changes in design involved in this evolution are given accurately enough for the student and mariner and yet are easy enough for the layman to assimilate. Ship design from canoe to *Queen Mary* is described, and warship design from the galley to the aircraft carriers of today. Methods of propulsion from oars to atom are scientifically and professionally explained, and the reader is given an insight into the future by this exact and interesting study of the past and present.

I read the book with delight, for we have all too few books that link the development of the ship with the development of mankind. If you have a speck of salt in your make-up, you will sense the drama of the ship's growth; if you are a student, you will learn about shipbuilding painlessly and profitably; if you are a ship's officer, you will find within these covers, written in your own language, all the romance of the decks you tread; and if you are a scientist, you will approve the painstaking detail with which Gibson has done his research. Every reader will enjoy *The Story of the Ship,* which is beautifully illustrated by twenty-seven excellent plates and contains a good nautical glossary. As a seafarer, I heartily approve it, as an educator heartily recommend it. I sat up half the night to finish it because it was too interesting to lay aside.

In *The Merchant Ship, Design Past and Present,* Dr. Baker has written an excellent book both for the student and the general public on the evolution of ship design from the Neolithic age to the present-day ocean giant. He has written well from his own extensive knowledge, and his information is detailed and accurate enough for the professional ship's officer to con, or for those who stay at home to enjoy. There is a great need for books of this type to satisfy the many questions in the layman's mind as to freeboard and bilges, tonnage and displacement, gunwales and sheer strake, and the like.

Dr. Baker explains it all in layman's language and in 160 pages covers the whole field of past and present ship design in intelligible words and with an eye to the future. A chapter on the ship at sea explains the various motions of the vessel—rolling, pitching, heaving, yawing, etc.—and how a ship is designed to withstand these stresses.

Speed, together with its effect on design and its relation to economy, is well defined and the relative merits of each method of propulsion compared. In the final chapter Dr. Baker tells of modern ship types, how they are constructed, launched, and tested on trials.

Vice President of the British Institute of Naval Architects, Dr. Baker is a world-wide authority on ship design. His book is a splendid contribution to the literature of shipbuilding and, being technical but not tedious, has an appeal to all readers who want to find out what makes the ship tick. I recommend it highly to librarians, to nautical schools, to mariners in all stages of development, to travelers about to take an ocean voyage, to naval architects, and to all men and women who like to go down to the waterfront and watch the ships go by—and to all thoughtful students of human affairs who realize how much our nation's safety and prosperity depend on shipping.

The book abounds in fine illustrations and possesses an excellent glossary of nautical nomenclature. Professionals and amateurs alike will enjoy it and benefit from reading it.

GORDON MCLINTOCK, REAR ADMIRAL, USMS
U. S. Merchant Marine Academy
Kings Point, New York

FLORIDA BIRD LIFE

Flight Into Sunshine. Helen G. Cruickshank. cxxi + 132 pp. $5.00. Macmillan. New York.

HELEN G. CRUICKSHANK and her photographer husband, Allan, have created a monument to themselves as naturalists, and a tribute to the National Audubon Society, protector of birds. In *Flight Into Sunshine* they have abundantly attained their expressed desire, "to pass on a little of the amusement and excitement" of their bird experiences in Florida. They have done more. *Flight Into Sunshine* records hitherto unknown facts concerning the nesting and other habits of many Florida birds. These are smoothly blended with interesting narrative and vivid descriptions of the sights and sounds of Florida's tropical wilderness.

The first chapter concerns the trip by car from New York, the equipment required by naturalist-photographers in the field, and a few historical high lights of the areas to be visited. Each of the remaining eleven chapters treats, in a highly interesting manner, a single species as studied and observed at one or a few specific locations. Typical chapter titles are Florida Cranes on the Kissimmee Prairie and Anhingas at Paradise Key.

In addition to precise information on the nesting habits of Florida birds, *Flight Into Sunshine* contains many interesting accounts of the authors' personal experiences. One of these, with a five foot, eleven inch cottonmouth, solved the mystery of destroyed glossy ibis nests. By dissection it was revealed that the reptile had swallowed five ibis and Louisiana heron eggs, a very large young American egret, and an adult glossy ibis. To the authors themselves "it seemed almost incredible that one cottonmouth moccasin could hold so much." Many naturalists will envy Allan Cruickshank the experience of watching a Florida crane hatch. The behavior of the brooding birds which culminated in their covering the young and the remaining unhatched egg with grass had probably never before been witnessed or recorded.

One hundred and twenty-one superb photographs grouped in one section complete the volume. These, the product of expert skill and endless hours of patient effort, multiply the value of the book manyfold.

E. J. WOOLFOLK
U. S. Forest Service
Washington, D. C.

EXPLORING THE FAR NORTH

To the Arctic! (Rev. ed.) Jeannette Mirsky. xxi + 344 + xviii pp. Illus. $5.00. Knopf. New York.

THE opening sentence of Stefansson's introduction to this book asserts that it "is both fascinating to read and the best history of northern exploration so far written." Both statements are true—indeed, who would doubt them, coming from the supreme authority on Arctic matters—but we wish to qualify each of these assertions. In our opinion, the author dwells unduly upon the romantic, the adventurous aspects of the expeditions she chooses to describe: hence the reader's fascination does not derive from the subject matter itself, the gradual unfolding of the Arctic region; it does not derive from the pleasure experienced when viewing the expansion of our scientific horizons, but rather from the somewhat artificial emphasis on the dangers and hardships experienced by some of the explorers. And if this partial account is in fact the best history of Northern exploration so far written, then the task of writing a first-rate, comprehensive, and authoritative account remains to be done. Such an account would surely

not omit from its index the names of Waldemar Jochelson and Waldemar Bogoras, authors, respectively, of the standard monographs on the Koryak and the Chukchee, and foremost among ethnologists who have ventured into northeastern Asia. It would at least mention the great Finnish explorers of Siberian peoples and languages—Castrén, who studied the Lapps of Scandinavia and the Kola Peninsula from 1841 to 1844, and who visited the Samoyeds throughout Siberia in 1845–49; Ahlqvist, who went to the Voguls and Ostyaks in 1858; and Donner, who carried on where Castrén had left off. An account that aims to be a history of Northern exploration cannot leave out the story of the great Hungarian explorers either: Reguly, Munkácsi, Pápai. All these, and numerous other expeditions, are not even mentioned in this book. The author admits these limitations herself: she deliberately omits the data that really are of scientific interest and relevance—references to the peoples who inhabit the Arctic, "the study of ocean currents, weather conditions, flora and fauna, experimentation with crops and domesticated animals, settlements, transportation routes, mining, lumbering, and so on."

In other words, this is a popular book written for the layman. We do not mean this disparagingly at all, but wish merely to call attention to the fact that such is the "best history" of its kind, and that this condition ought to be remedied in view of the tremendous and ever-increasing military and economic importance the Arctic region has come to bear in the twentieth century. We need an adequate Arctic guide and reference book: perhaps Stefansson himself can be prevailed upon to write it!

THOMAS A. SEBEOK

Department of English
Indiana University

CONSIDER THE HEAVENS

The Stars Are Yours. James Sayre Pickering. x + 264 pp. Illus. $3.95. Macmillan. New York.

SPACE is practically 99 percent mystery," according to Professor Pickering, who then solves, in the simplest possible language, some of that mystery for the layman. He even provides, unwittingly, source material for "science fiction" in such statements as: "Not only did Jupiter destroy the parent planet (of the asteroids), but he is holding captive some of the spoils of his ancient victory." But the purpose of the book is to help us become acquainted with the stars that twinkle so brightly in our sky and differ in aspect from season to season.

It begins with our own star, the sun, and continues out into space with the planets, including earth, the comets, the meteors, the nebulae, and the constellations. The discussions are full of facts such as distance and speed of travel and size, full of the theories as to the origin of the solar system and life on the planets, full of answers as to why the sky is blue and why such stars as Betelgeuse vary in brilliance. There are the fascinating "white dwarfs," the novae, and the dark banks of cosmic dust. Much of the book is taken up by charts of the heavens, in which north is at the bottom of the page because they are to be held over the head for study. These charts are arranged according to "culmination date" or the date when the exhibited constellations are as portrayed. In addition, there are twenty-three beautiful and awe-inspiring photographs of the solar corona, of the rings of Saturn, of the great nebulae. There are lists of the constellations, star tables and a list of star names, and a bibliography.

In fact, there is everything in the book necessary to the beginner in astronomy. It is equivalent to a year's course at college or university. Moreover, the writing is pleasant, with dry humor but not dry scientific data, and enlivened by graphic figures of speech. "There we are—and there are the stars. They are among the most beautiful, the most interesting and the most mysterious objects in our lives. All that we have to do to partake of their beauty and their eternal mystery is to step out of doors" with this book in hand.

MARJORIE B. SNYDER

Washington, D. C.

NATURE'S CHRONOLOGY

Days Without Time. Edwin Way Teale. 283 pp. Illus. $6.00. Dodd, Mead. New York.

THOSE who follow that genre of literature we call nature-writing are coming to regard a new book by Edwin Teale as an event. First of all, he is a good writer; he has something fresh to say about nature and says it directly, simply, and authoritatively; he can be learned without being pedantic, and dramatic without being sensational. Second, he is a photographer par excellence, and his close-up views of his subjects—milkweed, skunk cabbage, trees, grasshoppers, butterflies, insect eggs, birds, cats, etc., etc.—are superb.

The Teale combination of text and photographs is a thing of beauty and great satisfaction. This happy result has been abetted by what seems to us a greatly-to-be-emulated attitude on the part of the publisher, who has seen fit to treat both the text and the illustrations in a generous manner and has exerted obvious effort to see that the final product shows evidence of some of the niceties and potentialities of the printer's craft. The printing of the half tones is especially good. (Perhaps a few petals of this bouquet should fall into the basket of the Vail-Ballou Press, Inc., of Binghamton, New York, who actually printed the book.)

The essays that make up *Days Without Time* are on a miscellany of subjects in the realm of nature—plant and animal, human and nonhuman. A characteristic that adds variety to Mr. Teale's style is his way of drawing not only from his own wide observations and experiences as a naturalist, but also from those of others, evidencing a wide reading in natural-

history literature and a desire to enrich his writing with a laudable eclecticism. As the English essayist C. E. Montague once put it, there are many persons "going up and down this well-read world with literary luggage so meagre that it is hardly worth putting in the rack, not to speak of the van." Edwin Way Teale is not one of these. He has many "friends on the shelf," and he loves to pull them down.

PAUL H. OEHSER

Smithsonian Institution
Washington, D. C.

COMMON DENOMINATOR

Marching with the Grasses. Raymond J. Pool. xii + 210 pp. Illus. $3.50. University of Nebraska Press. Lincoln.

WITHIN the compass of this small book, the author has sketched in outline the origin, distribution, and utilization of the wild and cultivated grasses of the world in their connection with man's social and economic progress to the present state of civilization. About 100 pages are devoted to the discussion of the place of wheat, rice, corn, barley, oats, rye, sorghums, millets, and sugar cane as sources of food for man and animal. In the workaday world, these plants are not commonly thought of as grasses, but from a botanical standpoint they are true grasses.

In the remaining 90-odd pages, numerous tables, maps, lists, and descriptive materials are presented regarding the location and extent of the chief grassland areas of the world, with special mention of the important grass species for meadows, pastures, and ranges in the different countries.

In the history of agriculture, the herder preceded the farmer. Among domesticated animals, herbivorous species (cattle, sheep, goats, horses) were first in importance. Grass was their staff of life, at first the native grasses of the prairies and mountainsides. Gradually man selected the most promising kinds of grasses and began improving them in the direction of more suitable human food, the cereals, till today, when man lives directly, or indirectly through meat products, on grass. So it has come about in the past two decades that the tide in agriculture has set strongly toward grass farming. The author shows how this trend is manifesting itself in all parts of the world. Grass helps, as perhaps nothing else can, in preserving the fertility and proper physical structure of soils, and is also a most effective means of anchoring the soil against the erosive action of wind and water. Grass farming naturally stresses livestock, and that in turn makes necessary an increase of the grass coverage and a decrease in the area of farmland left bare and exposed to erosion.

Long-continued experiments aimed at determining the species of grasses most suitable for hay, pasture, and range, and for various climatic conditions, have provided lists of grasses to be recommended for these different conditions. The author has selected representative examples of grasses in these groups for description of their special qualities and the required conditions for their best development.

A special chapter deals with the use of grasses in soil conservation in different parts of the United States, and on methods of reseeding the Western rangeland where the grass carpet has been worn thin. Nor is the problem of grasses for lawns, parks, and golf courses neglected. Grasses have found their way into an almost endless list of uses· for hedges, ornamental decorations, matting, sandals, hats, baskets, brooms, paper, perfumes, roof thatching, and so on.

Not all grasses are looked upon with high esteem. A few of them are quite generally considered weeds. Crab grass has exhausted the patience and ingenuity of many a lawnmaker; sandbur is a bane to man and beast; foxtail and Johnson grass are not welcome everywhere. But it is well to be reminded from time to time of the transcendent importance of grass, as the author has done in this book.

E. V. WILCOX

Washington, D. C.

EXOTIC CULTURE

Island of Death. Werner Wolff. 228 pp. Illus. $7.00. Augustin. New York.

EASTER ISLAND, for a land of its size, has been the subject of its share of books and articles. Isolated beyond most inhabited islands, and with the added attractiveness of great stone statues and a mysterious writing, it has for many years been the perfect subject for studies serious, romantic, and speculative. This latest addition to the literature of Easter Island is a controversial book. Dr. Wolff, who has an active and inquiring mind, brings his knowledge of psychology to the aid of ethnological science in an attempt to solve some of the baffling problems of Easter Island culture.

The first chapter is devoted to a summary of the knowledge of the island and the culture of the people. Throughout the summary, as throughout the book, there are numerous comparisons, of a speculative nature, with Oriental and Central and South American cultures. Section II describes the various types of stone and wooden statues found on the island and discusses their symbolism and significance, and their relationship to the "death cult" and the "bird cult." The author distinguishes two types of stone "idols" and finds that all statues, both stone and wooden, have in common identification with a being, an object, or a force. In a later section (VI) after relating the various theories attempting to explain how the huge stone statues could have been moved from the workshop in the crater of Rano Raraku to the sites where they stand, Dr. Wolff comes up with the extraordinary theory that they may have been ejected from the crater by a volcanic eruption. He admits, however, that this theory, which fits in with the native explanation of magic transportation, of "giant-statues flying

through the air like ghosts, seems to resemble a fantastic dream."

On the basis of the native Metoro's readings of the tablets to Jaussen, Dr. Wolff has attempted to decipher some of the tablet writing. His method and interpretations are set forth in detail in Section III. Section IV contains a summary of theories about the origin of Easter Island culture. Numerous similarities and parallels between Easter Island on the one hand and Polynesia, Melanesia, North and South America, the Orient, and Egypt on the other are noted. The Egyptian similarities are further pursued in the next chapter, which is devoted to the hieroglyphics. On this subject the author concludes in part (p. 143):

> I found a large number of Easter glyphs and glyph-groups which have notable similarities to certain Egyptian glyphs. Since the meaning of certain glyphs is known and since the graphic similarity accompanies the similarity of meaning, I feel justified in believing that in cases promoting this graphic similarity—where the significance of the Egyptian hieroglyphics is known—the meaning of the island glyphs may be guessed from the corresponding Egyptian sign.

The book concludes with a chapter on the folklore of Easter Island, finishing with a discussion of logical versus symbolical thinking, the latter, according to the author, typifying that of the Easter Islander.

Dr. Wolff has written a stimulating and thought-provoking book containing a number of theories and suggestions with which few specialists on Polynesia will agree. The book is well designed and handsomely printed, and the publisher is to be congratulated on a fine piece of bookmaking—something all too seldom found among modern anthropological publications.

ERNEST S. DODGE
Peabody Museum of Salem
Salem, Massachusetts

HYDROLOGY

Rainfall and Runoff. Edgar E. Foster. xix + 487 pp. Illus. $9.00. Macmillan. New York.

IN *Rainfall and Runoff* the author has provided a signal service by bringing together in one volume the story of the developments that have taken place in the field of hydrology. The work covers a vast area, ranging from meteorology through the utilization of hydrological data. It is valuable in pointing out the developments that have occurred, in indicating where the student and engineer should go for more detailed information on any particular phase of the subject, and in advancing the use of statistical methods in the utilization of hydraulic data.

The limits of water in location and in amount available for use by mankind are unfortunately unpredictable with any degree of accuracy. Records available pertaining to precipitation and runoff are of extremely short duration when measured in terms of even the recorded period of civilization. As the length of records increases, however, it appears that droughts become more severe and storms and flood occur in greater intensity and are of greater duration. Statistical approaches are handicapped by having to consider very uncertain limits and relatively brief records. These factors should not discourage the use of such methods, but the engineer should keep in mind the limitations involved.

The book places a great deal of emphasis on the use of the H. Alden Foster and Allen Hazen approaches in the treatment of hydrologic series for indicated frequency of occurrences. It would have been well also to give some consideration to the approaches used by E. J. Gumble, a recognized leader in the use of the classical theory of the calculus of probability as related to studies of frequency. There also might have been included reference to the proposal made by Marion Clawson with respect to determination of the coefficient of variability-sequence. The author is to be commended for his reservations regarding the station-year method.

It is necessary that all the tools available to the practicing engineer should be utilized in planning the best use and conservation of our water resources, both above and below the ground surface. Mr. Foster's treatise is important in that it indicates nearly all the tools that have been developed and the limitations of use. The practicing engineer working on hydrology, however, needs to deal with more specific and detailed examples than are given in the book. The excellent and comprehensive bibliography makes such opportunity available.

The inclusion in the book of the chapter on ground water is particularly timely. The various geologic definitions in relation to ground water should be useful in directing the attention of engineers to the relationship as between geologic forms, ground-water storages, and replenishments. Additional information and research in this very important subject are needed.

There is, appropriately, a good discussion of evaporation in arid and semiarid areas. Evaporation in humid areas is also important. With respect to evaporation in such areas, the stream flow as recorded below any proposed reservoir has already been reduced by a very considerable loss of moisture from the reservoir area in the form of transpiration and evaporation. Under these conditions, the net loss of useful water from the reservoir is the difference between the gross evaporation from the reservoir surface as determined from pan evaporation and the existing loss from transpiration and evaporation that has already been accounted for in the stream-flow records that are used to determine the quantities of useful water available. Large errors in estimates of reservoir evaporation can be made by not taking account of these predevelopment transpiration and evaporation losses. Future revisions of the book might well include consideration of the later work of C. W. Thornthwaite.

The author has undoubtedly put a great deal of hard work into the preparation of this book, and it is

unfortunate that the price could not have been set at a figure more nearly within the reach of the average student and engineer.

E. Robert de Luccia

Federal Power Commission
Washington, D. C.

LOGIC AND METHODOLOGY OF SCIENCE

The Limits of Science. Leon Chwistek. Trans. from the Polish by H. C. Brodie and A. P. Coleman. lvii + 347 pp. $6.50. Harcourt, Brace. New York.

THE scope of this work is indicated by the subtitle, *Outline of Logic and of the Methodology of the Exact Sciences.* The official title seems unfortunate, suggesting, as it does, just another humanistic diatribe against science. The book is anything but that. Much of it is heavy reading on—to give the main chapter headings—The Limits of Sound Reason; The Development of the Concept of Number; The Elementary Concepts of Semantics; The Calculus of Propositions; The Theory of Classes; Foundations of Rational Metamathematics; The Fundamental Concepts of Mathematical Analysis; Problems of the Methodology of the Exact Sciences; and The Problem of Reality. Quite a program for 347 pages. Chwistek's work on logic, semantics, and the foundations of mathematics and of physical science has long been familiar to specialists in such fields, especially to those who can read Polish. This translation will remove at least the linguistic barrier to an understanding of Chwistek's subtleties.

In the Introducation to the Introduction proper, it is remarked that "Unfortunately . . . Chwistek uses a vocabulary and symbolic apparatus different from that of other philosophers and logicians." This is not peculiar to Chwistek, as may be verified by glancing through recent papers on symbolic logic. Philosophers, of course, have been talking to themselves for centuries; but that is no reason why logicians should attempt to emulate them. If there ever is an international congress of logicians, one item on the agenda might be an effort to agree on a common symbolism for those things that are common to the works of at least three symbolic logicians. The present situation is similar to, but worse than, the Babel in vector analysis before some of the private notations of various writers were swept into limbo about thirty years ago.

For those who do not care to master the symbolism, there is much interesting historical material attractively presented here. There also are occasional asides that no doubt will appeal to the philosophically minded. For example, "At the basis of the problem of time lies the longing for immortality and the fear of death." How does anybody know that?

E. T. Bell

California Institute of Technology

UP TO DATE

Readings in the Physical Sciences. Harlow Shapley, Helen Wright, and Samuel Rapport, Eds. xii + 501 pp. $3.00. Appleton-Century-Crofts. New York.

THIS book contains a number of articles designed to present to the reader the modern scientific picture of the physical world. In selecting these articles the editors have laid emphasis upon contemporary writers because of the rapid advance in the physical sciences in recent years. Early scientific workers have not been entirely neglected, however. We find in the book Galileo's account of the discovery of the satellites of Jupiter with his new telescope, and an extract from the epoch-making article by Copernicus "Concerning the Revolutions of the Heavenly Bodies." We also find extracts from Newton's *Principia,* and letters by Franklin describing his kite experiment and the effects of a Leyden jar discharge on the human body.

The articles contained in the book are classified under six headings: Science and the Scientific Method, Astronomy, Geology, Mathematics, Physics, and Chemistry. In each class there is a general introduction by the editors and a copious bibliography of modern works on the subject. The reviewer was particularly pleased to find that the mathematical bibliography includes E. A. Abbott's *Flatland: a Romance of Many Dimensions,* a book which well deserves being rescued from oblivion.

That the book is quite up to date may be seen from the fact that in the section on physics there is included a long extract from Smyth's official report on *Atomic Energy for Military Purposes,* and that in the section on chemistry there is an article by Gibbons on synthetic rubber—a wartime product that is apparently here to stay.

The reviewer has given particular attention to those articles not in his own specialty, and has found them generally well written and easy to understand; and for this reason he feels justified in recommending the book as successful in the purpose for which its editors designed it.

Paul R. Heyl

Washington, D. C.

CORRESPONDENCE

OLD

I am a member of the AAAS but do not subscribe to THE SCIENTIFIC MONTHLY. However, I happened to see the article by Bruce Old entitled "On the Mathematics of Committees, Boards, and Panels" in the issue of August 1946. . . . This article was most entertaining and edifying, because it reflects much of what goes on at some of the meetings of committees and boards I have attended. If the article is available as a reprint, I should be most appreciative of a copy.

BERNARD H. FOX

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The University of Rochester

NEW

Perhaps you will be interested in an experience I had recently. I loaned to a colleague of mine the December 1948 issue of THE SCIENTIFIC MONTHLY so that he might read Dr. Knudson's article on "Sound Waves and Rhythms," . . . We are both working occasionally in the field of sound. He returned it in two days and asked for another issue because he liked the first one. In a few days he came back for two more, and I am afraid he will want to read all the 1948 issues. He states that it is the best scientific magazine of its type he has come across, and he would be pleased to consider membership. . . .

C. J. KRIEGER

San Diego, California

AID TO BEILSTEIN WORKERS

I appreciate the opportunity of calling to the attention of the members of the American Association for the Advancement of Science a campaign of the Philadelphia Section of the American Chemical Society to raise $2,500 to purchase food packages for the Beilstein Editorial Board. This worthy cause should appeal particularly to chemists who are members of the AAAS.

Although Beilstein, Gmelin, and other chemical compendia are produced in Germany, they are of international character, and their continuation is vitally important to chemists.

Contributions should be sent to Dr. William H. Hugh, treasurer, Philadelphia Textile Institute, Pine and Broad Streets, Philadelphia 2, Pa. Each donor will know exactly to whom a particular package paid for is finally sent. Here is an opportunity for the scientists of the United States, and particularly the chemists, to demonstrate a willingness to assist fellow-chemists in dire need. Without such assistance it is very likely that the work on Beilstein and other chemical compendia will cease—or at least be delayed considerably. For both selfish and unselfish reasons financial assistance is desirable.

WALTER J. MURPHY

Industrial and Engineering Chemistry
Washington, D. C.

LABORATORY MOUSE

Abhorred, damned, and hunted,
Timorous, persistent, prolific,
Symbol of uncleanliness;
Pulsing, sniffing, prying,
Gnawing, befouling, multiplying;
Furry, silent,
Once suspected of spontaneous generation
From rags and corn;
Commiserated by the tender, earthy poet—
Up through the years, mouse, you have come with us,
An unwilling sacrifice on the edge
Of a widening circle of knowledge;
Invaluable to searchers sounding the uncertain dark
To build greater safety for man and all his animals—
Except you!

Here you are now, mouse,
Bred up from nocturnal noxiousness in must and dust
To ordered living in tended rooms,
On airy shelves,
In round glass houses,
In wire apartments;
A quart of milk a day to 150 of you,
Protein pellets in wire mangers,
Bread and cake, good food and bad, and doubtful—
Whatever research requires,
But always in ease and with a gambler's chance at
  security—
For what happens to you may be the long-sought answer.

Your tail is a handy hold to dangle you
While man reads the record from your knife-notched ear,
You are favored more and more as his circle
Widens into the border of the cold unknown.

You are the elite of the rodents—
The squirrel is an irritable chatterer;
The groundhog is shot with a "22" or gassed in his burrow
As a clumsy clover thief;
The prairie dog is poisoned for his industry and prolificacy.
You, mouse, were created convenient, omnivorous, and
  economical
Of food and space and time;
You compass a human lifetime for, perhaps, a dollar;
You eat as man does;
You share his diseases;
Now you face outward with him on the ever-widening
  front.

Little mouse, you're a happy tool,
Why should I say
"Don't be a fool?"

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MORPHOLOGIC ASPECTS OF CANCER RESEARCH

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THE cancer problem concerns all of us. It is estimated that in the United States alone about 180,000 persons die of cancer every year. The chances are that in your family and in mine someone either has cancer now or will have cancer and will die of it. Though cancer has existed as long as the human race, the knowledge of its nature is hardly a century old. Most of the scientific information has been acquired in the past fifty years. Between 1900 and 1930 there appeared more than 12,000 articles dealing with the experimental aspects of cancer.[1] Today oncology, the science of neoplasia, has advanced so far that we now can recognize the limits of our knowledge.

For practical purposes cancer may be defined as an atypical new growth, derived from cells of the individual. It is atypical in that it does not conform to the cellular pattern of the site of its origin and in that it does not serve any useful purpose. Furthermore, it maintains its metabolic activities at the expense of the body household, to which it contributes nothing. Not all new growths, or neoplasms, are cancerous. Those that grow expansively, like a toy balloon, and remain encapsulated, compressing rather than invading the surrounding tissues, are called benign neoplasms. Cancerous growths invade, infiltrate, and replace the surrounding tissues. Because of the rapidity with which the cancer cells multiply and the lag in the formation of supporting stroma, areas of necrosis and hemorrhage are frequent. Cancer cells may become detached and carried by the lymph or by the blood stream to distant parts of the body. Here they set up new cancer foci similar to the cancer at the site of its origin. Cancer cells are endowed with extraordinary powers of multiplication; in fact, the life of cancer cells is geared to one principal activity, namely, to multiply.

I

Depending on the cellular origins and cellular patterns, cancerous growths are either epithelial or nonepithelial. An epithelial cancerous growth is called a carcinoma; a nonepithelial cancerous growth, a sarcoma. There is a fundamental difference between the two kinds of cancer: the surrounding tissues provide the supporting stroma and blood vessels for a carcinoma, whereas the sarcoma evolves its own stroma. In rare instances there is a combination of the two kinds of cancer, a carcinosarcoma, in which a sarcoma forms the supporting stroma for a carcinoma. Such neoplasms have been observed in the mammary glands of mice, rats, and men.[2]

Roughly, there are two lines of investigation of

cancer: one experimental, and the other clinical. Experimental cancer research employs animals—mice, rats, guinea pigs, rabbits, monkeys, birds, etc. It investigates the origin, structure, nature, and behavior of cancer cells. It studies the susceptibility of the animal to certain neoplasms and the genetic factors in the spontaneous occurrence of cancer. For investigative purposes, cancers are produced in experimental animals by physical, chemical, hormonal, viral, or other carcinogenic factors. Further experiments are designed to test the effects of various therapeutic measures. Cancer cells are transplanted from one animal to another of the same or another species and are propagated in tissue cultures. Such experimental studies prove the noncontagious or noninfectious nature of cancer and the origin of cancer cells from cells of the host. They also demonstrate the survivance indefinitely of cancer cells when environment suitable for their multiplication is provided.

Clinical investigation of cancer concerns itself with human cancer. Since the nature of human neoplasia in general and of cancer in particular can be learned from human sources alone, the investigation of human cancer is of paramount importance. Any phase of research in human cancer may be rewarding, and the information gained may prove of immediate—that is, of practical—value in the recognition and treatment of neoplastic disease.

Perhaps the most urgent of fundamental problems is the origin of the neoplastic cell. Morphologic evidence suggests at least two lines of derivation for neoplastic cells. One of these is the anlage origin, another is origin from apparently normal cells. In the first group, cells left over in the process of evolution of the individual start to multiply and form a neoplasm. These are neoplasms of anlage or cell-rest origin. In the other group, an apparently normal cell is stimulated to multiply, and during the process of cell division a new cell, a variant with neoplastic properties, is produced.

Cells left over in the process of evolution of the individual are often observed. Nevi of the skin; islands of thyroid or parathyroid tissues; islands of cortical tissue of the suprarenal glands; aberrant pancreatic tissue in the wall of the stomach, duodenum, or small intestine and elsewhere; aberrant bile ducts in the wall of the gall bladder; epithelial cell nests beneath the mucosa of the gums and about the hypophysis may be cited as examples. These cell rests usually remain dormant. Under circumstances as yet undeterminable, however, they may begin to multiply and become [illegible] may occur in early childhood, during adolescence, or even in adult life. The stimulation initiating the growth is possibly hormonal and related to that governing the growth of the individual. Such tumors of cell-rest or anlage origin reveal themselves by imitating the tissue or organ structure they originally were destined to become. Depending on the stage of development of the individual at the time when the cells became segregated, these growths may be simple neoplasms mimicking a tissue, or they may be mixed tumors mimicking an organ, or teratomas mimicking an individual.

A few examples may illuminate this concept. An adamantinoma is a benign neoplasm mimicking a tissue. It arises from ameloblasts, the cells that form the enamel of the tooth. This is an ectodermal epithelial growth, the supporting stroma of which is provided by the surrounding connective tissue. Such neoplasms usually occur about the jaw and occasionally at the base of the skull in the region of the *sella turcica*. Rarely, an adamantinoma has been observed also at other locations.[3]

Anlage tumors of the salivary glands provide an example of mixed tumors mimicking organs. The anlage tumors of salivary glands most commonly arise in connection with the parotid glands or along the lines of fusion of embryonal facial fissures, and occasionally elsewhere. They are composed of cells which may produce ectodermal and mesodermal structures. The ectoderm is represented by the epithelial elements, and the mesoderm by the presence of embryonal connective tissue, adipose tissue, endothelial lined spaces, young and adult connective tissue, hyaline cartilage, and osseous tissue. In this kind of growth both the epithelial and nonepithelial elements are a part of the neoplasm. Therefore, the cells which form the anlage for this kind of neoplasm obviously became segregated at a stage of development when the cells had not as yet acquired their dominant characters as ectodermal or mesodermal cells; hence, their ability to form either kind with equal facility.[4]

Teratomas are neoplasms containing tissue elements derived from all three of the germ layers. The anlage for such growths, particularly if they are small, must have become segregated near the blastula stage of the embryo. When such growths are large they probably are a part of single-ovum twins where only one of the twins developed fully and enclosed as a teratoma a portion of its brother or sister.[5] Teratomas arising in the ovaries or in a testicle are of different derivation. In these organs there are multipotential cells that can produce structures of all three of the germ layers. These teratomas represent not siblings but descendants of the individual.

The neoplasms of anlage origin just mentioned are usually benign. There are, however, many cancers of anlage origin. Most of them occur in infancy and early childhood. At this age not enough time has elapsed for a carcinogenic agent to become effective. The medulloblastomas arising in the roof of the fourth ventricle, the retinoblastomas, and the neuroblastomas of the suprarenal gland, which are genetically and structurally closely related, belong to this group. Hypernephromas, Wilms' tumors, which are usually carcinosarcomas, and a recently observed case of fibrosarcoma of the epididymis in a child are other examples.[6]

II

A second line of derivation for neoplastic cells is from apparently normal cells. Often normal cells are stimulated to multiply. During the process of cell division under the influence of some undetermined agent a new cell is produced, a variant with neoplastic properties. This appears to be the case particularly when neoplasms arise from the epithelial cells covering the surface of the body or those lining the respiratory, gastrointestinal, urinary, and genital tracts. Here a prolonged chronic inflammatory reaction frequently precedes the appearance of the neoplasm. Obviously, some carcinogenic agent has been active at these sites. At other sites, however, the carcinogenic agent may be at work without producing any detectable initial lesions, and the cancer may be discovered only at a fully developed stage.

There are no means at present of observing the actual transformation of a normal cell into a cancer cell. Although some investigators speak of precancerous stages, obviously such a state is hard to define. Cells fully differentiated for a special function usually lose their ability to divide and thus to multiply; therefore, such cells cannot become cancer cells. Only cells still capable of dividing may become variants with neoplastic properties—that is, cancer cells. It is uncertain whether a single such cell is the parent cell of a cancer or whether several similar cells at a given site become cancerous simultaneously or in succession.[7] The presence of a growth-promoting substance can safely be inferred as existing about the margins of the cancer, since all the cells near by appear large, their nuclei are more deeply stained, and the cells themselves seem less differentiated than those further away.

The nomenclature of the various kinds of cancer has not as yet been unified. The differences, however, are superficial rather than essential.[8] Each cancer usually has its individual cellular pattern, which is faithfully reproduced in every part of the growth and at all the distant sites where the cells have been transported and implanted. It is simpler to name the neoplasm according to the cell that forms the growth rather than according to the structures formed by the cells. The variations in cellular patterns of growths arising at the same site in different individuals can be explained on the basis of dominant and recessive characters of the parent cell of the growth. An ectodermal parent cell, for example, may grow in sheets producing no particular pattern; it may mimic the cell pattern it would normally produce according to its dominant character; or it may produce any other patterns for which it has recessive characters. The genetic method of nomenclature has been successfully applied to neoplasms of the central nervous system by Bailey and Cushing,[9] and more recently to carcinomas of the lung[10] and to growths originating at other sites.

The existence and nature of a neoplasm are established by microscopic examination. Since the cellular structure and pattern of a neoplasm usually provide clues as to its likely cellular origin and to its probable behavior, this information is of paramount importance in determining the method of treatment. Therefore, when patients have areas suspected of neoplastic involvement or lumps suggestive of a new growth, the lumps are removed or the suspected areas biopsied and examined microscopically. The pathologist to whom the tissues are submitted for gross and microscopic examination identifies the growth and determines its position in the oncologic system. The report he renders is usually decisive as to the course of treatment. Thus the scientific clinical investigation and study of human cancer require the understanding cooperation of patients and the coordinated efforts of a team of physicians.

Cancer teams are in operation in practically all well-conducted hospitals and particularly in those connected with university medical schools. These teams collect an immense number of objective clinical data by establishing standard methods of diagnosis, by applying standard and new methods of treatment, and by observing and recording the course of the disease during the patient's life. The tissues submitted for microscopic examination and those secured at post-mortem are preserved as part of a collection for immediate and subsequent study. Thus the laboratory of each hospital that cares for cancer patients is a potential center for cancer research. The record of each patient constitutes a unit of scientific information for subsequent study. From such centers there may come reports on large series of patients who had cancers in various organs or sites [illegible]

made available on cancer of the lung, stomach, pancreas, biliary system, intestines, genital and urinary tracts, brain, skeleton, soft tissues of the body, etc. that cannot be obtained in any other way. There may come reports on neoplasms never seen before,[11] or so rare that only a few are on record. Such reports may aid others in recognizing similar growths or may bring out new principles or shed light on obscure fields. The collection of individual records of cancer patients in hospitals having modern standards, the analysis of such records by statisticians, geneticists, morphologists, and other specialists, and the reporting of the results add to the knowledge of human cancer. Knowledge is gradually accumulating on the race, sex, and age incidence, on the cellular structure of cancer,[12] on sites of its origin, the methods of spread locally and to distant parts of the body, and on the effects of cancer on the host. Such information is being gathered while the fundamental problems are awaiting solution.

III

It must be clear from what has already been said that information is most wanting at the point where the cell of the individual becomes an atypical cell, forming either a relatively orderly growth pattern and becoming a so-called benign neoplasm, or forming a more disorderly growth pattern, a so-called malignant neoplasm, or cancer. In solving this problem experts in practically any one of the natural sciences may be valuable members of an investigative team.

The experimental research worker may choose animals with a short life span and through controlled matings study the behavior of a cancer through many generations. The research worker studying human cancer is at a disadvantage: his life span is the same as that of his subject, and matings remain uncontrolled. This is the reason why progress in the study of human cancer is so slow and painstaking and why it requires the cooperative efforts of many scientists with special knowledge.

Any field of investigation is enriched whenever new theories are evolved and new methods of investigation are discovered. Often the man with the technical skill is not the same one who evolves the theories that tie seemingly unrelated facts into one greater scheme. The discovery of the cell as the smallest unit of life in the tissue or organ, the invention of microscopic techniques, and the perfection of the microscope laid the foundation for morphologic identification of neoplasms in general and cancer in particular. We are still in the process [illegible] tools. When new methods and techniques are evolved, they may permit the invasion of fields yet unknown, and new theories will provide the impetus for such exploration. It has often been said that a theory need not be true —it just needs to be good! That is, it should explain all the facts available at the time. When new facts appear which the theory cannot explain, it is replaced by a new and better working hypothesis. Let us hope that the interest now manifested in cancer research will yield new tools and theories that will eventually solve the cancer problem.

There are, of course, some obvious questions I am sure everyone would like to have answered. The two most urgent are: Is cancer on the increase? and, What can I do to protect myself and my family from getting it? Neither of these questions can be given a final answer. In my experience cancer *is* on the increase, and the reason for it is that people live longer and more people are alive in the sixth and seventh decades. It must be remembered, however, that cancer is quite frequent in children and occurs also during adolescence and in young adults. The second question can be answered only indirectly. According to E. V. Cowdry,[13] who skillfully summarized the work of the Fourth International Cancer Research Congress held in St. Louis last year, the general consensus of the Congress was that complete protection from cancer is impossible of realization. There are so many carcinogens, of both external and internal origin, that they cannot all be avoided. If by constitution and coincidental exposure to carcinogens one happens to develop cancer, one should look upon it as upon any other disease, have it diagnosed as early as possible, and have it treated in the best manner available.

REFERENCES

1. Shimkin, M. B. In Ackerman and Del Regato, *Cancer.* St. Louis: C. V. Mosby, 1947. Pp. 30–59.
2. Halpert, B., and Young, M. O. *Surg.*, 1948. **23**, 289–92.
3. Halpert, B., and Dohn, H. P. *Arch. Path.*, 1947, **43**, 313–17.
4. Halpert, B., and Tool, C. D. *Cancer Res.*, 1947, **7**, 346–50.
5. O'Leary, C. M., and Halpert, B. *Surg.*, 1947, **21**, 194–99.
6. Halpert, B., and Thompson, W. V. *Arch. Path.*, 1947, **43**, 358–63.
7. Brunschwig, A., and Thornton, T. F. *Cancer Res.*, 1944, **4**, 515–18.
8. D'Aunoy, R., Pearson, B., and Halpert, B. *Amer. J. Path.*, 1939. **15**, 567–78.
9. Bailey, P., and Cushing, H. *A Classification of the Tumors of the Glioma Group on a Histogenetic Basis with a Correlated Study of Prognosis.* Philadelphia: Lippincott, 1926.
10. Halpert, B., and Pearson, B. *Amer. J. Cancer*, 1940, **40**, 213–18.
11. Halpert, B., and Patzer, R. *Surg.*, 1947, **22**, 837–41.
12. Halpert, B. *Cancer Res.*, 1941, **1**, 900.
13. Cowdry, E. V. *J. Amer. Med. Ass.*, 1947, **135**, 1,067–72.

LA MONTANA LLORONA*

ARCHIE F. CARR, JR.

Photographs by Margaret Hogaboom and the Author

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TOWARD the end of April the high valleys of southern Honduras, which have lain waterless beneath the fierce tropical sun since November, begin to dry up. One can count the boulders in the once lush *potreros,* and the scant, green carpet has withered under the dusty pines on the mountainsides. The cedars and the stubby-limbed ceibas wait leafless for the rains of May; only the imperturbable mimosas, the *coyol* palms, and the figs and *guanacastes* along the watercourses relieve the brown monotony. The cows go dry; the steers are skinny and listless and seek shade in preference to the tasteless stubble. At night thin red lines snake along the flanks and summits of the mountains, where ground fires scavenge in the crisp remnants of the pine woods understory. The wind that sporadically sweeps up from the Pacific toward morning is hot, like air from Texas prairies or city asphalt.

* As one wanders about the highlands of Honduras and asks the people the name of their highest local peak, the answer comes back, again and again, *"La Llorona;"* that is to say, *La Montaña Llorona,* which means "The Weeping Forest." The people thus allude, with characteristic imagery, to the tearlike fall of water that condenses on the trees of the cloud forest. How the word *montaña* came to mean forest is another story.

Toward the end of April, too, our nerves unravel. We begin to get sick of the *verano,* with its blue haze of smoky dust, and its tough beef, and two canteens per rider per afternoon jaunt. Our spirits droop in the heat and drought, and we wonder pettishly if it will ever again be cool and wet. Our bitterness mounts with the realization that we must wait for the rains of June.

But is June really the only way out? How about the 6,300-foot peak of Uyuca that rises at the valley's edge only two miles or so away? And how about Portillo and Monte Crudo and El Volcán? They are all much nearer than June and are cool little isolated worlds, as abruptly disjunct and unexpected—and as welcome—as a palm-shaded well in the Sahara. Up there where the clouds cruise by on the unhindered trade wind, the pine woods give way and the *montaña,* the cloud forest, sucks water from the eternal mists and mocks the forty-inch rainfall of the valley below.

From various points in the valley around Escuela Agrícola Panamericana ten cloud-forested peaks may be seen. A visit to even the nearest of them means a long, hot climb, but the reward is great and the climb itself is interesting. It takes one out of the valley with its chaparral

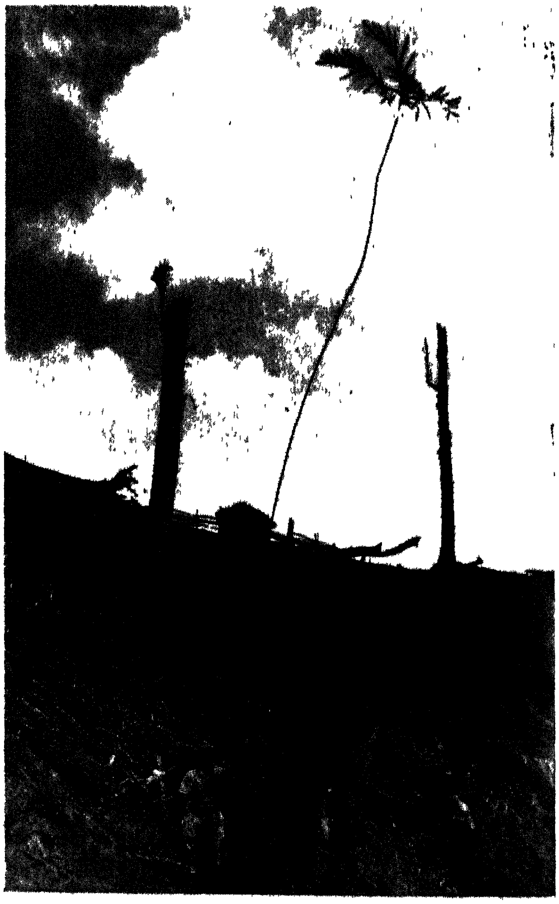
open pine woods of the surrounding hills, with a very different fauna. A couple of thousand feet of this parklike *ocotal,* and the *ocote* pine is replaced by another species, known locally as *pinabete,* often burdened with epiphytes and sometimes mixing or alternating with liquidambar, the familiar, beautiful sweet gum of the southeastern United States. Each of the tran ition areas between these vertical zones is the equivalent of many miles of latitude in the biotic changes it brings, and each tempts the biologist to tarry. But in the drought of April it is better to climb on, emerging from the *pinabetal,* crossing the fringing fields and blackberry tangles, pushing through the second growth *guamil* and passing at last between the outer columnar trunks of the *montaña alta.* Abruptly, midday changes to owl's-light, and the dry breeze behind is damped to a slow drift of air that is 8–10 degrees cooler than that in the valley, and heavy with moisture and the smell of wet plants.

The cloud forest community is primeval and self-perpetuating. As in most natural mesophytic climax forests, the taller trees meet above in a continuous leaf stratum, which opens only here and there to admit splotches of sunlight. Competition for light among the numerous species of gigantic oaks and *aguacates* leaves little for the plants of the dim understory to fight over, but tree ferns and spindly palms thrive, varying in relative abundance from one forest to another and occasionally attaining heights of thirty feet and more. Giant-leafed wigandias and a purple-flowered fuchsia glean light in the shadows. The curious tropical melostomes, with a host of species in the lowland rain forest and a few others in the dry uplands, here show their versatility in a whole new series of forms, which range in size from small shrubs to fair-sized trees. The smaller plants are tender, gloom-loving begonias, aroids and peperonias, pteridophytes and mosses in endless variety, lichens, liverworts, and algae, all of which grow equally well on the ground and on the grotesquely buttressed, deeply fluted, and vine-embraced trunks of the older trees.

It is next to impossible to look up and determine with certainty which leaves belong to a particular tree. The confusion of interlacing branches is complete, and a tree which bears, say, 3 tons of leaves may, according to my reckoning, support 5 tons of epiphytes ranging in bulk from microscopic algae, tiny mosses, and half-inch orchids to enormous thick-leaved, woody parasites, one individual of which may replace a third or even half of the original tree crown. On one 18-inch length of tree-fern trunk I was able to find 24 species of plants, and I am sure that a botanist would in several cases have distinguished between species that I lumped together. On the drenched and windy peak of El Volcán I climbed a tree which was twisted and wind-pruned like a tree on ocean dunes and which had the usual investment of mosses, selaginellas, and filmy ferns concealing every inch of limb surface. Besides this, it bore four different kinds of leaves in approximately equal abundance, and I would have challenged any botanist to determine which belonged to the original host trunk without making a laborious dissection of the tree.

In the high but protected and relatively level coves and glens, such as the superb Plano Aguacatal in the San Juancito range, or the little hidden plateau where the waters of the Santa Clara arise on the slopes of Monserrat, a number of oaks, *aguacates,* and other trees unknown to me, grow to immense size, and the forests surpass in stateliness anything in my experience.

To me, however, the dominant plants of the cloud forest are not the giant trees but rather the



A skinny palm of the genus *Chamaedorea* from a deep cloud forest on El Volcán.

Epiphytic cacti hang from trees in the San Juancito Mountains. In June these pendant ropes bear lovely orange-colored blossoms at the ends.

epiphytes. The most important factor in the development of these woods is water vapor—not precipitation—and the air plants, high and low, respond more directly to this influence than those with roots in the ground. If a tree wins out over its neighbors and rises above them, thousands of unbidden guests seek to share its advantage. They pile up in sodden tons on the trunk and branches and crowd the leaves at the tips of the slenderest twigs. During heavy rains the added burden of water they hold is often too much for the great limbs, and they may crash to the ground, ripping out sections of the trunk as they fall. Apparently, the normal ultimate fate of the big forest trees is to be overcome by epiphytes, insidiously, leaf by leaf, or by catastrophic collapse, or by a combination of the two.

The dominance of the epiphytes is particularly obvious in the exposed levels above 6,500 feet, where the large-leaved trees of the protected places are replaced by heaths, *Podocarpus,* wax myrtle, and other small and hard-leaved species. It is difficult to explain this transition to a dominantly ericaceous flora on these high peaks which receive a maximum of water through condensation of moisture from the nearly continuous winds, unless it be that the stronger winds augment the hazard in a temporary failure of the moisture supply. These scant-leaved, dwarfed, and wind-tortured trees are often little more than framework for the support of masses of air plants, and a tree that at first glance appears to be alive may be nothing but a corpse, completely enshrouded by the flora that killed it.

At 7,500 feet on Peña Blanca the vegetation is a wild, unsorted hodgepodge. There is no distinguishing between limbs, trunks, and roots, for all loop and twist and sprawl about on the steep rock faces beneath a heavy, wet mat of lower plants. Only here and there a plume of leaves projects from the crazy mass to mark the site where a tree is trying to make a living in the face of almost insuperable obstacles.

A feature of the cloud forests almost as striking as the lavishness of their plant life is their relative poverty in animals, a poverty both in species and in individuals, but most markedly in the latter. Expecting to find a luxuriant environment supporting a dense and varied animal population, one enters the woods prepared to marvel at the fauna. It is entirely possible to wander about for hours, however, and even for days, amid this floral splendor and see only a little more in the way of animals than might be found in a well-kept greenhouse.

To illustrate the sort of jolt which this hothouse sterility delivers to one's preconceptions, consider the case of the bromeliaceous air plants. It would be hard to mention a minor environmental

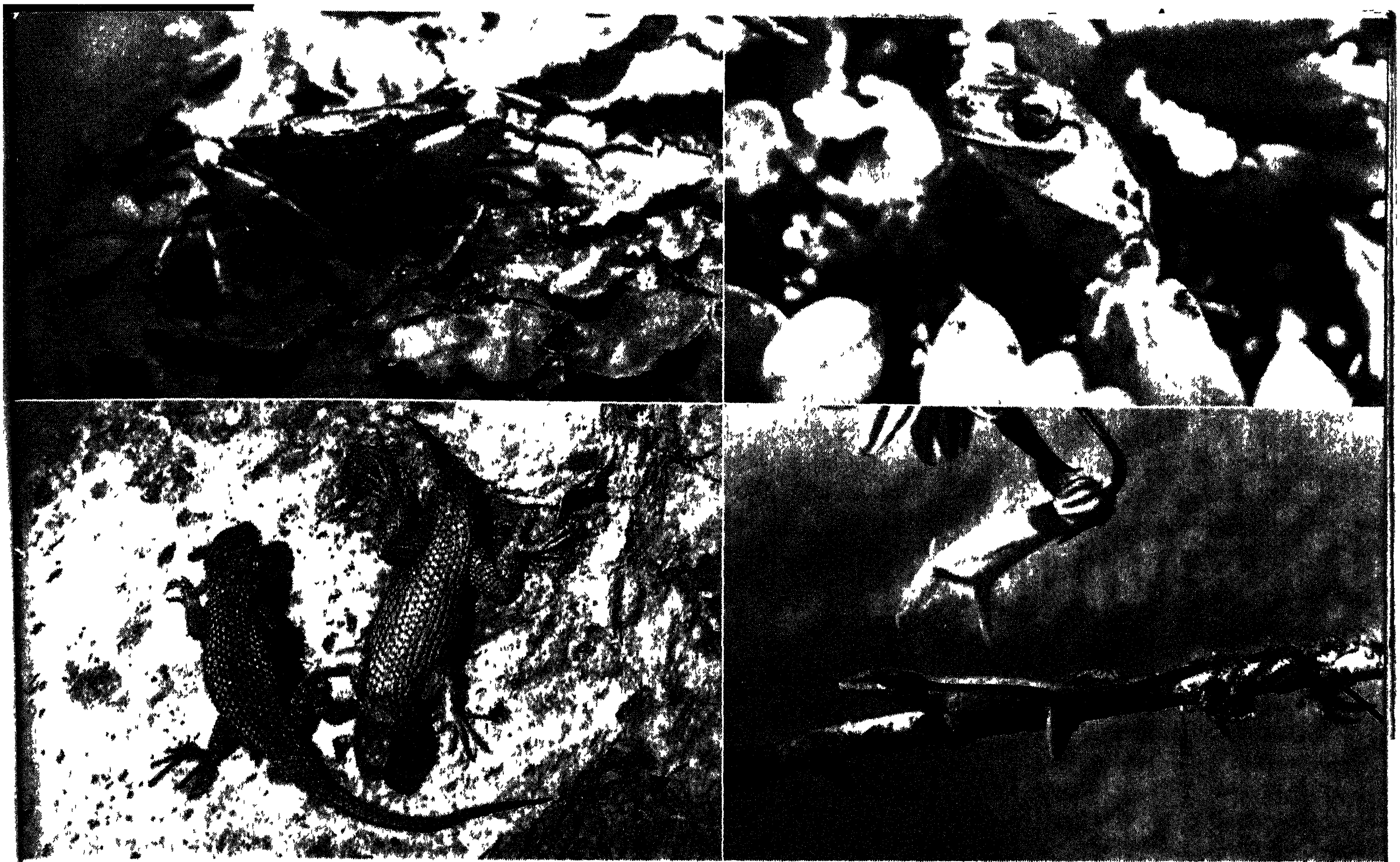
niche which holds, and usually fulfills, more promise for the herpetological collector than these epiphytic, water-storing plants of the pineapple family. In many regions, if a bromeliad can be found, the reward of from one to a dozen or so specimens is almost automatic. In the tropics, humid or semiarid, the smooth, broad-based leaves and nearly permanent axillary pools of cool water offer irresistible quarters for various species of frogs, salamanders, lizards, snakes, insects, and mollusks, and some of these have become drastically adapted in structure to life in air plants. These facts are well and widely known, but to the seasoned collector who, reading this, may find their repetition tedious I suggest that he go with me to Portillo de los Arados. Here the forest climbs a 60-degree slope from a fern-shaded spring and rill among the rocks to the tip of a 6,000-foot peak, and the trees bear more bromeliads than I ever saw before. Or, rather, I should say *bore* more bromeliads, because I think we clawed down half of them in the excitement of our conviction that here, at last, we would find salamanders on the Pacific slope of Honduras. We hauled them down, one after another, dumping upon our shivering persons the quart or gallon of cold water that each contained and finding not one single vertebrate animal. Of invertebrates there were only some sow bugs, an occasional centipede, several scorpions (one of which stung me), and swarms of ants (nearly all of which stung me).

After that, we sulked in the valley for nearly two weeks and did not go near the high woods.

But this was a mistake. When finally we returned it was to spend the night in a tiny milpa deep in the forest of El Volcán. We slept on the ground and were awakened in the vaporous dawn by the ethereal songs of scores of *jilgueros,* the incomparable notes of which express so precisely in fluid sound the spirit of the high forest. We lay under our tarpaulin watching the heavy white mist drift over us and listening to the ecstatic notes of the unseen birds, singing in a cloud, and forgot for good the fiasco of the bromeliads.

Much later, on this same mountain, I had the supreme reward of seeing my first male quetzal. It was 5 : 30 in the afternoon of a day spent in fruitless search of quetzals. We had scaled the dripping peak of El Volcán, descending it on the opposite side and laboriously working our way back around the base to the homeward trail. As the sun was setting we crossed a little clearing bounded by the towering silvery trunks of the primeval forest. I sat down to spend the few remaining minutes of daylight

buttressed base of a giant *aguacate* at Rancho Quemado.



Upper left: This little crab is one of the very few inhabitants of any size of the icy brooks and springs of the highest forests. *Right:* A cloud-forest tree frog peers from a cluster of diminutive orchids. *Lower left:* The coloration of this spiny-scaled mountain fence lizard varies from bright green to sooty black. It lives on dead logs in clearings in the cloud forest. *Right:* A female anoli from an elevation of 7,000 feet at Rancho Quemado, San Juancito Mountains, type locality of the subspecies.

watching the forest border for anything that might emerge while my companion went to fetch the horses. The brief sunset spread flame through the clouds behind the western ranges, and desultory shreds of mist began to spiral down into the milpa. For once there was almost no wind, and the only sounds were occasional incredibly sweet passages from the *jilgueros* and the low, duotone chant of the Mexican trogon in the depths of the forest. Suddenly a harsh cackling call came from a tall tree at the edge of the woods. I rose and walked toward the tree while the call continued. As I approached, several green toucanets emerged from the tree and flew off over the milpa, pushing their banana-like beaks before them. The raucous cackle continued. Then three quetzals, a female and two gorgeous males, rose above the crown of the tree. One of the males and the female flew directly into the woods, but the other flew and hopped from one tree to another, often making vertical sallies into the air above and descending again in a wholly uncalled-for series of swoops and dips and pirouettes to display his crimson breast, the blue-green fire of his wings, and the grace of his yard-long tail. This was reward enough, and indeed if the forest grew for no other end than to support this bird it were no waste.

There is a handful of animals which a quiet observer may see on nearly any given visit to any of the local cloud forests. These creatures are for the most part species endemic to montane communities and have wide but necessarily discontinuous distribution throughout Central America. Like most habitues of deep forests, they know how to live inconspicuously in the background of their environment.

If you want to see them you will have to sit on a log and wait. If your log lies at the edge of a clearing you will almost immediately see on a near-by stump a big green or sooty-black fence lizard of the genus *Sceloporus.* If the time is early morning or late afternoon, or if the clouds are drifting through the forest on a gentle wind, you will not wait long for the songs of the *jilguero* or nightingale thrush and the black robin, known here as *sinzontle.* Both are skilled ventriloquists, and, though they may render their flutelike arpeggios from no more than a few yards away, they are very hard to see. Long before you locate a singer a flock of clorospingas will pass by in the lower trees, squeaking as they hop from [illegible]

ing twig to another. If you continue to give no offense a slim brown wood hewer will swoop down to a near-by tree and stay to forage a bit.

A slender anoli will scurry along a vine and stop to spread his orange throat fan; or to nap for a moment in a splash of hot sunlight, with arms tucked in and legs pressed back against his tail; or perhaps to creep up with horrid stealth and seize a simple-minded cranefly. A small, short-eared squirrel that has been looking at you in silence for a long time will suddenly materialize, often under your very nose, and try to scold without dropping the avocado that it holds in its mouth. These little forest squirrels are common and unafraid and ask only that you keep the peace.

Sooner or later you will become aware of a tiny, intricate song, sung in an excited whisper somewhere close by. This is Rehn's mountain wren, through some ornithologist's whimsey, and when you finally locate the red-brown dwarf it will have been within reach of your hand all the while.

The robin chirp of the dusky *zorzal* is never long still. Usually the woods echo also with one or both of two other bird calls, one the monotonous and incessant single note of the white-faced quail dove and the other the brooding chant of the Mexican trogon.

Among the few actually conspicuous inhabitants of the cloud forests are the hummingbirds when they are in season. They are of a dozen or more species, and to the eye accustomed only to the common ruby-throat of the United States their variety in size and coloration is striking. They range from tiny mites noticeably smaller than the ruby-throat up to a glossy dark species with a body the size of a man's big finger. During September and October, when the wild *aguacates* bloom and the melostomes and vacciniums strew the ground with shed corollas, the hummingbirds leave the fields and *guamil* and blackberry tangles and move into the depths of the forest in hordes. They suck the high blossoms and make their perfect little nests of live moss covered with lichens and lined with the silken fiber of the tree fern. Their voices are as various as their shapes and sizes, and the males scold and squabble incessantly. One misanthropic species marks the course of any intruder who walks through the woods by repeatedly taking stations on twigs just in front of him and uttering raucous cheeps in outraged monotone, once a second without ceasing until the trespasser has crossed the bounds of what the bird regards as his rightful freehold. Another species vents its seemingly chronic spleen by zigzagging angrily among the trees around the human visitor, often diving at his head at breakneck speed, and all the while rattling out a querulous complaint for all the world like a midget kingfisher. Among themselves they fight bitterly and often, and their quarrels are both intra- and interspecific without bias. If one lies on one's back and stares upward at the level where the treetops interlace high above, one may see pairs of buglike hummingbirds zipping over and under the green canopy like dogfighting planes diving into and out of cloud banks. One such pair may suddenly plunge downward in a breathless spiral, the hind bird following in machinelike detail every intricacy of the mad course of the pursued. They may collide in mid-air in front of the observer's face with what ought to be crushing force and then fly separately away to sit on twigs and preen and await the fine new surge of anger that will tune their incredible little muscles for more joyous combat.

Hummingbirds are birds apart. They are miracles of mechanical design and physiologic efficiency. Their initiative and courage are beyond reproach, and in many species the splendor of their coloring cannot be matched in the biologic world. But few who know them will go much farther in their praise. For the rest, they are peevish and restless and generally ornery and are no comfort either to themselves or to anyone who lives with them.

Of invertebrate animals the most noteworthy—in its abundance as well as in the incongruity of its occurrence on a mountain peak—is a crab. To me, a crab has no business in fresh water and much less in a cloud forest, and I still feel an irrational sort of skepticism every time I see one. The small, black mountain species lives in the beds and borders of foaming creeks and rills and around the near-by springs, where it digs caves under rocks where salamanders should be and heaps up mud pellets like crayfish should do.

Perhaps of just as much consequence in the forest community as any animal that I have mentioned are the several kinds of mice and at least two species of shrews that live underground in the root mazes, but they are almost never seen unless trapped. It is also easy to miss the glistening, yellow-speckled plated lizard that hunts sow bugs and wolf spiders and spindly-legged crickets in the leaf mold.

Besides the few common and characteristic denizens that one can count on meeting on almost any trip into the *montaña,* there is a long list of creatures which are seen only sporadically, or very locally, or perhaps not at all and which the forest holds in reserve to shock the observer out of

any growing smugness over the authority of his lore. Thus, only one time, after hundreds of hours of observation, did we encounter an anteater, and on the same day we collected our first and only penelope—the big burro-voiced *pava* —when we walked up under a pair eating berries in a tree at Rancho Quemado. We had hunted deer and frogs on Uyuca so many nights that when finally our lights struck red fire from eyes in the trail ahead we only stared stupidly as a puma loped slowly up into the darkness and *guamil* above These animals of rare occurrence not only lend the promise of drama to every collecting trip, but the very eccentricity of their distribution may present problems of irresistible interest to the zoologist.

For example, consider the faunal vagary to be seen in the colony of giant pocket gophers on a mountain in El Paraiso known as Portillo de los Arados. Here the forest has been skinned back on all the slopes up to the sheer crest, which is crowned by a *faja de montaña*, a long narrow strip of well-developed cloud forest. In the margins of this woods and in the milpas which skirt it, the ground is honeycombed by the burrows of an enormous pocket gopher, which we have seen on two occasions but have not succeeded in collecting. Neither the soil nor the vegetation of this mountain shows any notable difference from those of other mountains that we have visited. The only place in Honduras where pocket gophers are known is Alto Cantoral, north of Tegucigalpa, which is the type locality and only known range of the Honduran pocket gopher, a subspecies of the giant pocket gopher, and probably the same animal which occurs on Portillo. It seems very unlikely that we have merely missed them elsewhere, since their excavations and mounds are very conspicuous and would be next to impossible to overlook. Whether Portillo furnishes the gophers with something vital which is wanting on the other peaks, or whether their restricted distribution refers instead to some event in the history of the region or of the race, is an intriguing question and one which almost certainly would repay a systematic study.

Hummingbird's nest from the windy peak of Uyuca. Constructed of live mosses, it was camouflaged with lichens and lined with the silky fiber of the tree fern.

Another case of interest is that of the quetzal. This incomparable trogon is a characteristic inhabitant of cloud forests from southern Mexico to Panama. Because of its superb plumage it has been persecuted unmercifully in many places, and its scarcity in suitable areas around the larger towns is probably due chiefly to the work of hunters. But in the rural, south-central part of Honduras, where

Tall tree ferns from interior of the forest of Uyuca at an elevation of 6,000 feet. Vegetation is characteristic of the more protected glens and ravines.

our series of cloud forests is located, guns are scarce and where found are usually muzzle-loaded with black powder and a quarter-inch cube of bar lead. They are not often used for shooting at birds and even less frequently for killing them.

It seems probable that here the infrequent occurrence of quetzals is due more to the restricted extent of the individual forests than to the killing of birds. At San Juancito there is far more hunting than in any of the other local forests, and yet the quetzals hold on within sound of blasting at the Rosario mine, because the woods there is almost unbroken over thousands of acres. Likewise, we found quetzals in the Yuscaran Range around El Volcán even though the 15–20 square miles of cloud forest there are spotted with *ranchos* and dissected by cultivation. In the smaller forests, however, such as Montañuela, Portillo, and Monte Crudo, although the physiognomy of the vegetation is identical, quetzals are entirely unknown.

In this case the limiting factor would appear to be food. Quetzal existence apparently depends upon a continuous abundance of small fruits. There is evidently an areal threshold below which a given forest is too small to yield these fruits in perennial supply. The birds venture from their primeval habitat far enough to augment their diet with the luscious blackberries that abound in the marginal tangles (as do also the peccaries and, where found, the mountain tapir), but they evidently never migrate and are actually prisoners in their cloud-swept jungles. If the fruit crop fails for a single week they must starve.

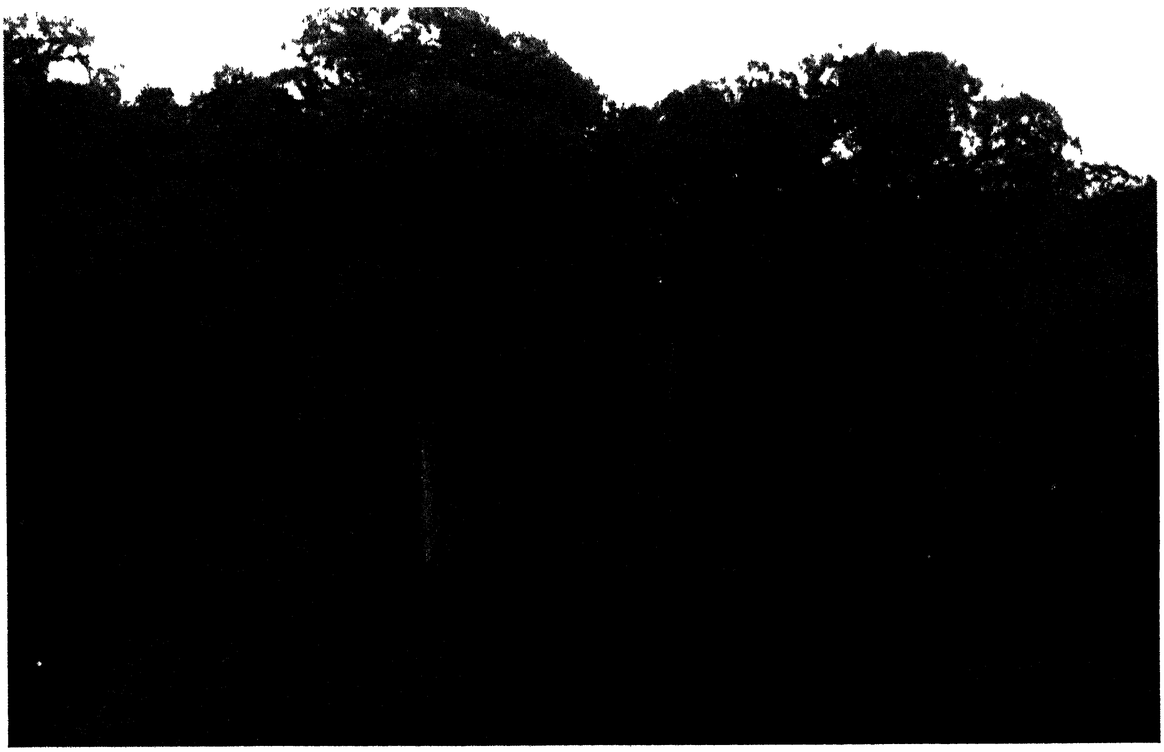
Of equal interest, zoologically if not aesthetically, is the *tamagas* of San Juancito. The *tamagas* is a short, fat, irritable viper of the notorious genus *Bothrops* (*B. godmani*), which includes the *barba amarilla* (fer-de-lance), the horrendous bushmaster, and a host of smaller poisonous snakes. The curious thing about the *tamagas* is that the only ones we have ever seen were at Rancho Quemado in the San Juancito mountains, where they are quite common. In the places where the sun filters down through a break in the tall canopy overhead, they lie around on the leaf mold, their brown and gray color tones and rough scales making them as inconspicuous as mounds of leaves.

We have visited Rancho Quemado eight times and have only twice failed to see *tamagas*. Al Chable found four in one morning, and fifteen minutes after I had warned Margaret Hogaboom to watch where she put her feet she stumbled over a *tamagas* in the trail. I have collected in Honduras for three years, and the *tamagas* of this colony are the only snakes in the central part of the republic that I should call common. Only here, for instance, would I dare predict that on a given afternoon a snake-collecting jaunt would yield something.

The question of what attraction Rancho Quemado holds for *tamagas* is no great mystery. It gives them deer mice in abundance and an endless system of galleries and intricate moss-lined catacombs beneath the prop and buttress roots of the forest trees. Here they can prowl and forage in comfort when the blood-thickening fog is down and the icy drip turns *tamagas* bodies to lumps of helpless clay. The mice and the catacombs make for what would appear to be an ideal viper environment, and the *tamagas* respond with enthusiasm. Why, then, have we looked in vain for snakes of any kind in identically tunneled and mouse-filled areas in all the other forests?

I believe I know why the *tamagas* do not occur on the other peaks. I think peccaries make life impossible for them. The collared peccaries are ubiq-

uitous despoilers of the cloud forests. We have found their tracks or their dung or the wreckage they leave behind in nearly all the forests of the area. They live in the dense *guamiles,* preferably in the ghastly *morales,* or nearly pure stands of giant blackberry bushes, which fringe all the high woods; here the pigs are safe from molestation by man or beast. Part of the time the bands stay in the margins and eat blackberries, but often they come out to make forays into the milpas below or into the forest above. They are keen foragers and are apparently omnivorious, and I imagine that very little that is conceivably edible escapes their jaws. I would not give two cents for the chances of a fat and succulent viper, stupid from overeating or torpid from cold.



Heavy cloud forest above La Tigra, San Juancito.

Since the pigs do not dwell in the depths of the woods, but rather in the blackberry tangles at the edges, the snake population of a large unbroken tract of forest such as San Juancito is practically insulated from the ravages of the peccaries. On all the other mountains, however, the ratio of *guamil* to *montaña* is so much higher that no section of the woods escapes the sporadic patrols, and snakes cannot survive.

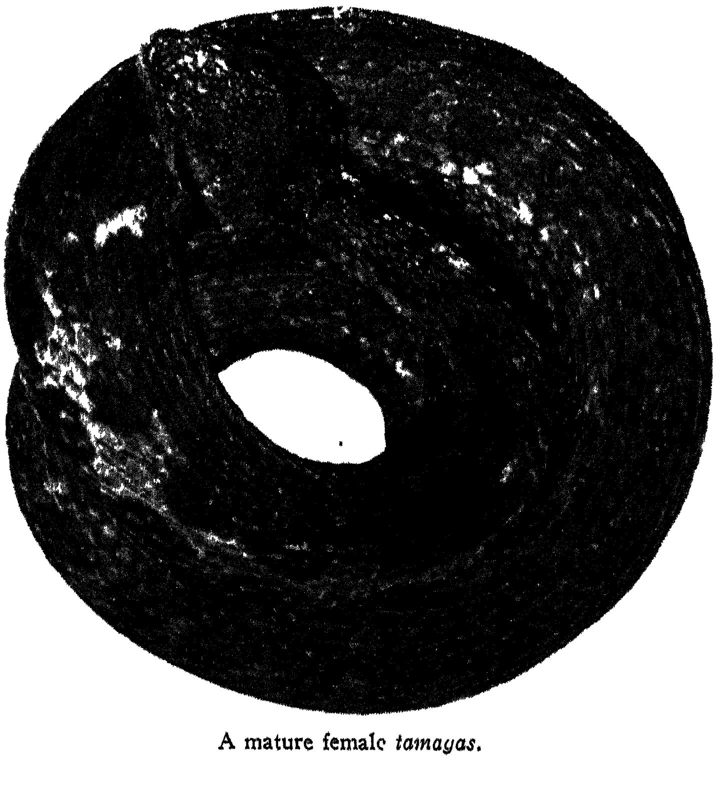
To me, the most curious anomaly of all is the mountain tapir. It outrages my sense of fitness. The mammalogists mostly recognize only one species of tapir in Honduras and so would have us believe that it is one and the same beast that wallows in the creeks of the coastal jungles and scampers about the crags of Chile Mountain. This may be so, but if it is I know of no more impressive example of ecologic tolerance and the broad, flexible outlook. I lived for a month on intimate terms with tapirs in the Nicaraguan rain forest and feel that I know something of their character and aspirations. I cannot believe that the tapir there is the animal that makes the tracks and trails on sheer cliffs of 6,500-foot Pico de Navaja, or ambles about the subalpine *morales* of Batea, picking blackberries, or that ran 6 miles along the knife-edged *fileta* of Cerro Brujo to elude completely a gang of mountain-bred dogs. How could the semiaquatic monster of the Huahuashan swamps slake his chronic thirst or cool his hide on the streamless ridges of Bromadero? I don't believe he does; I think it is a different creature and shall cite one minor aspect of jungle tapir personality to support my stand.

A lowland tapir descends the slopes of ravine-side or river bluff in a manner wholly his own and

altogether irresponsible. If he arrives at such a declivity at all pressed for time he merely lets go and falls down. His legs move and usually manage to keep him right side up, but they in no way retard the acceleration of gravity. I have seen and heard this phenomenon several times and each time I marveled when the tapir failed to kill himself. On Chile Mountain he *would* kill himself. We climbed tapir trails there which required more from the arms than from the legs, and which would demand caution from a mountain goat. Any tapir trying to save time on these trails by falling down them would wind up in the vicinity of Cantarranas, some 4,000 feet below and 8 miles to the westward.

A comparison of adequate series of specimens of tapirs from the mountains and lowlands seems to me very much in order, and I strongly suspect that when made it will show at least racial differences between the two populations.

As a biotic environment, the Honduran cloud forests offer the biologist a challenge and promise of reward that would be difficult to duplicate. But, more than this, they hold an aesthetic appeal that is as deep as the mystery of the dim, green woods and as varied as their many changing aspects.



A mature female *tamagas.*

IS ECONOMICS NECESSARY?

KENNETH E. BOULDING

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BEING an economist, I can hardly be expected to answer "No" to the question that forms the title of this paper. My thesis is, however, that economics is necessary, not merely for the support of economists, but for the development and perhaps even for the survival of science in general and the civilization that supports it. I propose to consider particularly what justification there is for a separate discipline of economics, and what contribution this discipline makes to the general advancement of knowledge.

The social sciences are reputed, at least in popular imagination, to be less "successful" than the physical sciences. The "success" of a science is judged mainly by its ability to predict or to control future events in its field. For the common man, as for the operational philosopher, knowledge is identified with power, and knowing with know-how. By this standard even economics, which has a certain reputation as the most successful of the social sciences, makes a poor showing compared with the prediction of eclipses, the certainties of chemistry, and the miracles of genetics. This, we hasten to explain, is a result of the difficulties of the science, not of the inadequacies of the scientist. We sympathize with the wayward universe of the meteorolgist even as we chafe at the waywardness of his predictions, and, if the predictions of the economist are even more wayward, it is because of the complex and unstable nature of the universe with which he deals. Moreover, the social scientist faces a problem which normally does not bother the nonsocial scientist, in that he is himself part of the field of his investigation. If the heavenly bodies were themselves moved by astronomers, or even if they were moved by temperamental angels who guided their behavior by the astronomers' predictions, the astronomers would find themselves in just as bad a fix as the economists. The bacteriologist who must stain his bacteria in order to see them would be in even worse trouble if his bacteria blushed when they were observed. Of course not even the astronomer seems to be exempt from observer trouble in these days of relativity, but in the case of the social sciences the trouble develops long before we approach the speed of light. Nowhere is the positivistic fiction of a dispassionate, objective observer wholly removed from the field of his observation more absurd than in the social sciences. The difference between the social and the other sciences, however, is merely one of degree, and as the nonsocial sciences run increasingly into observer trouble it may be that not merely the results but the methods also of the social sciences may be of interest to other scientists.

Economics has a certain reputation—not, I think, wholly undeserved—for being the most scientific of the social sciences. It does possess, I think, a larger body of analytical propositions that are widely accepted by competent persons than either sociology or political science. It also exhibits the marks of the history of a true science, in that it exhibits an orderly development toward greater and greater generality. The older theories —i.e., of the classical economists—can easily be formulated as special cases of the more general modern theory. This very internal consistency and success, however, has developed in some economists a certain spiritual pride which has injured the development of social science as a whole, and I think the profession is coming to realize more and more the necessity for trade among the various disciplines if further specialization is to be fruitful. We are reaching out on all sides today toward a unified social science—a regional federation, as it were, which must be accomplished before we can proceed to that great federation of all knowledge that is the ultimate task of the inquiring spirit. All the social sciences have much to learn from one another, and the same might be said of sciences of any kind.

I

Economics, like any other science, has two closely related parts—the pure science and the empirical science. Pure economics is a branch of logic or of mathematics (in these days there does not seem to be much distinction between them). It attempts to construct systems of hypothetical

propositions, mainly of a qualitative nature (if A rises, B falls) relating certain "economic quantities" such as prices, wages, outputs, interest rates, etc. Such a system is called a "model," and the construction of such models is, of course, the characteristic activity of the "pure" part of any science. The nature of the models themselves, however, is determined mainly by the empirical content of the subject matter of the science. Thus, even though the model is an abstraction, not depending for any correspondence with empirical reality for its self-consistency, yet the act of model-building—except perhaps in pure mathematics—is not unrelated to the empirical interests of the model builder, and the usefulness of a model depends on the degree to which it helps in interpreting the complexities of the empirical world. The Keplerian theory of a single planet revolving around a sun is a good example of an "astronomical" model. It has no exact counterpart in reality, at least in our solar system, yet it derives interest and significance from the fact that it helps to interpret (by being capable of extension and generalization) the movements of the actual solar system. Similarly in economics the marginal analysis of the individual economic unit (planet!) or the Walrasian system of equations of general equilibrium of the price system under perfect markets (which corresponds somewhat to the Laplacian system in astronomy) is a "model" which derives interest from the light it throws on the workings of the intolerable complex of social relationships. Models which do not apparently abstract from an empirical universe may be called "non-Euclidian" models from the analogy with non-Euclidian geometry. Thus it would no doubt be possible to construct models of planetary systems assuming different laws of gravitational attraction, of momentum, etc. than those which seem to prevail in our system; indeed, I have no doubt this has been done. Nor are these non-Euclidian models mere idle exercises of an overactive mind; they may turn out to have more than an aesthetic value, as witness the significance of non-Euclidian geometries in modern physics. In economics also there is something to be said for model-building for its own sake, and there is no need whatever to stick to the assumptions of the elementary textbook. Economics is in no way bound to such assumptions as profit maximization: there never has been an economic man even in economics, except as a very first approximation, and by means of the indifference curve analysis economics has increasingly liberated itself from any narrowness of assumption. The *methods* of economic analysis would apply just as well to a Franciscan economy as to a Benthamite! Nevertheless, the *interests* of the model builder are likely to be determined to a considerable extent by the empirical world in which he lives, and even by the practical problems he faces. It is no accident, for instance, that the depression of the thirties was the scene of a great deal of theoretical activity centering around the problem of unemployment. Similarly, in the elementary theory of the firm, the assumption is made that the firm selects that position of the variables under its control which results in the maximization of some measure of money profits. As a first approximation, this assumption yields useful results. But it is quite possible, and indeed necessary, to go beyond it, and to take account of more complex motivations, such as the desire to be important, or to be well regarded, or to obey the dictates of conscience, or even to be liquid.

It is not generally realized, I think, how far economics has gone in the direction of becoming a generalized theory of choice. Economics begins as an attempt to explain the magnitudes and movements of certain quantities, such as prices, wages, outputs, sales, and so on. Very early in its development it became clear that these quantities cannot be treated as an independent world of their own, for they are thrown up as a result of the whole complex of human choices operating within the strait jacket of a niggardly natural environment. Thus even in Adam Smith we find the explanation of wage differences in terms of what might be called the nonmonetary advantages of the various occupations; and little more than a hundred years later we find Wicksteed illustrating the principles of value theory with reference to the problem of how high a cliff one would dive off to save a mother-in-law, or how much family prayers should be shortened to speed a parting guest to the train—problems that are a long way from what is usually thought of as economic. Nevertheless, it is an inevitable logic that has turned the study of prices into a theory of value, for the price system is simply one reflection of the general problem of "scarcity," and the choice between nuts and apples differs only in its simplicity from the choice between income and leisure, between freedom and security, between love and power, between color and form, or between better and worse. Value, in the sense of what we have to give up of one thing in order to get a unit of another—i.e., as a "transformation ratio"—is a phenomenon we meet in every conceivable branch

of human activity, for wherever there is limitation, wherever there is choice, wherever we cannot have our cake and eat it, there the value phenomenon pops up. The novelist balancing up his chapters, the painter balancing his picture, the general apportioning his troops, the preacher arranging his service, the professor preparing his course, the cook planning a menu, the government formulating a policy, are all of them facing essentially the same "economic" problem as in the apportionment of time or the spending of money. Wherever resources are limited, choice is necessary and value raises its earthy head. It may be, as Wordsworth says, that "High Heaven rejects the lore of nicely calculated less or more" (i e., economics), but, even if this is the case (and Wordsworth's authority is by no means unimpeachable), it is merely because High Heaven is presumably possessed of unlimited resources. In some fields the "less or more" may be less nicely calculated than in the market place, though one sometimes wonders after studying the exotic behavior of banks, corporations, and labor unions whether these phenomena could not be profitably studied with the techniques of the cultural anthropologist. Custom, habit, tradition, and ritual play an important part in the day-to-day activity of the most solemnly economic and ostensibly money-making institution. On the other hand, the balancing of advantage against disadvantage which is the mark of the "economizing" process is found among the most primitive tribes, the most careless bohemians and the most otherworldly saints. Indeed, it may well be that the saint—who knows what spiritual goods he wants and who goes after them regardless of how many norms of conventional behavior he shatters—is closer to the pattern of economic man than is the frock-coated banker whose watchword is respectability (a thoroughly primitive, anthropological concept) and whose walk of life is hedged about with innumerable barriers of established custom.

Economics is significant, then, not merely because it investigates an important slice of life in the market place, but because the phenomena which emerge in a relatively clear and quantitative form in the market place are also found in virtually all other human activities. Hence, economic life itself, in the narrow sense of that part of human activity that is concerned with buying, selling, producing, and consuming, is a "model" of the whole vast complex of human activity and experience, and the principles which are discovered in a clear and quantitative form in the market may be applied to the understanding of apparently quite unrelated phenomena in biology, art, religion, morals, politics, and the whole complex structure of human relationships. I do not mean, of course, that economic principles are *sufficient* to the understanding of the complex universe of reality; but they are, I believe, a necessary implement in the inquirer's tool chest.

It is also true, of course, that principles which come to the clearest expression in the study of other subject matters are of great importance in the interpretation of so-called "economic" phenomena. The concept of an ecological system, which was developed first in the biological sciences —i.e., of a system of populations of various things, in which the equilibrium size and the movement of each population are dependent on the size of other populations—is an interpretive principle of the utmost value in the social sciences. Just as a pond develops an equilibrium population of frogs, fishes, bacteria, algae, and the like, all in subtle competitive and cooperative relationships with one another, so society is a great pond, developing equilibrium populations of Baptist churches, post offices, gas stations, families, counties, states, wheat farmers, chickens, and so on, which also exhibit complex cooperative and competitive relations one with another. The concept of mechanical equilibrium, both static and dynamic, has also had an immense impact—indeed, too great an impact—on the social sciences. Wherever we find a potential difference producing a current or flow by overcoming a resistance, we find something like Ohm's law, exhibited in its purest form in the study of electricity, but valuable as an interpretive principle when we study the flow of goods or of resources in response to price differences (economic potential) against the resistance imposed by costs of transport. In the theory of electrical circuits we may find clues to some baffling phenomena connected with the circuit flow of money.

Within the social sciences themselves, concepts which have been developed in anthropology, such as systems of ritualistic and customary behavior, and concepts which have been developed in sociology and social psychology, such as the crisis-adjustment patterns in family relationships, are all applicable to the subject matter of economics.

Indeed, I see the great empire of human knowledge, not as a conglomeration of independent and perhaps even warring kingdoms, each cultivating its own little field of subject matter by its own methods and each living wholly on its own produce, but as a great Republic of the mind, comprised, it is true, of subdivisions such as

Physics, Chemistry, Economics, Botany, and the like, the boundaries of which are, like the boundaries of political states, partly the result of historic accident and partly the result of the lay of the land, but all uniting and cooperating in a common task of producing and exchanging the most precious of all commodities, and, indeed, exchanging not only the results of their labors, but exchanging also the tools which the special requirements of each field have perfected.

II

I propose to devote the remainder of this article, therefore, to a brief discussion of the contribution which the methods of economics may be able to make to other fields.

Recent developments in economics in the theory of oligopoly have an important bearing on problems of political science. It is perhaps significant that there was no representative from political science in this symposium. In a day when civilization itself is threatened by our inability to solve an essentially political problem (the abolition of war), it is tragic that so little fundamental thinking is being done in political science. Even the World Federalists—the only group who seem to be intellectually active in this field at all—seem to have got little further than the eighteenth century. It may well be that a significant revival in political thought will come out of the economics of oligopoly, where we are concerned essentially with problems of *strategy*—i.e., situations in which the choices of each person or organization involved depend upon their expectations regarding the choices of the others. It may be that the present bankruptcy of the national state, which can provide us with neither security, justice, peace, nor honor, is closely associated with the duopolistic character of international rivalry. There are marked similarities between the power struggles of oligopolistic firms and the power struggles of states: price wars and sales wars exhibit in a simplified form many of the essential problems of that most detestable of sciences, military science. There is no more striking contrast than between the resourcefulness and inventiveness which is shown in dealing with the "war" problem in the business world, with its multitudinous forms of agreement and federation, and the sterility and ritualistic rigidity of the political world.

Economics can also make an important contribution to those sciences in which general equilibria or a great multiplicity of interconnected relationships are characteristic of the subject matter. In economics, as in astronomy, the experimental method is almost impossible. We cannot simplify our universe, as the chemist or the physicist does, by the artificial creation of conditions in which virtually all factors but the ones we are investigating are excluded. We cannot take a businessman or a household and expose them first to one set of prices and then to another set to see what happens. Our subject matter is presented to us in a manner that is for the most part not within our control: there is no recipe for unscrambling in fact the magnificent omelette of social experience. We are always faced with an overwhelming and baffling multiplicity, and because of the very dominance of the problem we have been forced to devise methods for handling it.

These methods fall into three groups. There is first the *ceteris paribus* approach, identified mainly with the name of Marshall, which is in a sense a method of intellectual experiment, involving the isolation of a single problem by the assumption that all variables other than those investigated are held constant. This method has yielded valuable results in a limited sphere, and is a necessary prerequisite to the solution of more difficult problems. Nevertheless, it also has its dangers, especially the danger of overgeneralization from the particular to the general case. Thus the fact that a fall in the wages of carpenters is likely to lead to a rise in the amount of employment offered to them by no means implies that the remedy for general unemployment is general wage reduction. It is easy to fall into fallacies of composition when using this method, but in spite of its dangers it remains a necessary implement in the economist's, and indeed in any scientist's, tool bag.

The second method is one that is familiar in the physical sciences, the method of simultaneous equations. In economics this is associated chiefly with the name of Leon Walras and the Lausanne school. It is based on the proposition that any system of n variables, each of which can be written as a function of all the others, yields n of these equations which may be capable of solution to yield values of the variables each of which is consistent with every other. The difficulty of the method is that unless we know a good deal about the form of the assumed functional relationships we cannot be positive that the system has a "real" solution, or that it does not have many real solutions. It may even have solutions that are mathematically correct but economically meaningless, such as negative prices. Consequently, if we except the pioneering work of Leontieff at Harvard, the

method has not been particularly fruitful of results in economics, in spite of its superior elegance and generality.

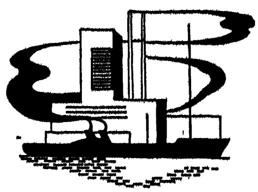
The third method is that associated with the name of Keynes, now frequently called that of "macroeconomics" This consists essentially in using large aggregates of economic variables as the basic parameters of simplified models, the exact properties of which can be fairly easily determined. In a sense this combines the simplicity and fruitfulness of the Marshallian approach with the generality of the Walrasian. Marshall's method is admirable in discussing the forces that determine the price and output of, say, limburger cheese, but it cannot deal with the problems of the system as a whole. Walras deals with the system as a whole, but at such a level of generality and abstraction that practically nothing can be said about it except that it exists. Keynes, by taking the system as a whole, but ruthlessly lumping it into large aggregates, the relationships of which he explores, effects in a sense a combination of the virtues of both the other methods. The macroeconomic models are simple enough to be handled, and yet cover the whole system. Not that the macroeconomic method is without its own dangers. Aggregates like "the national income," or "the level of employment," or "the price level" are all heterogeneous conglomerations, and there is danger, particularly for the more mathematical and less philosophical users of the technique, of neglecting the *structure* of these aggregates. It is fatally easy to write "Let the National Income be Y and the Price Level be P" and straightway to get so deliciously involved in the manipulation of our Y s and P s that we forget that they are not simple aggregates but have a complex structure which may well be relevant to the problem in hand. This "fallacy of aggregation" is a common one; it is at the root of most of the fallacies of Marxism, with its assumption of homogeneous classes; of Nationalism, with its assumption of homogeneous nations; and it even accounts for the spectacular lack of prophetic success among the brighter young economists. Nevertheless, for all its dangers, the macroeconomic method has led to a revolution in economic thought, the end of which is by no means visible, and it creates a discipline and habit of mind which might easily create revolutions in other sciences as well. I suspect that the natural scientists are also subject to both the fallacy of composition and the fallacy of aggregation; that they are much too uncritical of their basic taxonomic systems, much too prone to generalize on the basis of particular experience, and too little sensitive to the abominable interrelatedness of things! It would be a valuable experience for any scientist to familiarize himself thoroughly with what may be called the "macroeconomic paradoxes"—the propositions which are true in individual experience but which are quite untrue for society as a whole. Thus an individual can increase his money stock by "hoarding"—i.e., spending less money than he receives: but the attempt on the part of all individuals to hoard does not result in general "hoarding;" it merely decreases the total volume of money payments. An individual can get rid of money by spending it: a society cannot. For an individual, expenditure and receipts are two very different things: for society they are exactly the same thing, every expenditure being another person's receipt. An individual can "save"—i.e., increase his net worth—by not consuming as much as his income. If everyone tries to do this the result may not be an increase in society's capital but a decline in income and employment. In distribution these paradoxes abound. A trade union may raise the wages of its members: it is very doubtful whether trade unions as a whole can raise wages as a share of the total income. Profits are determined by the level of investment, not by the wage bargain; the more business distributes in dividends, the greater will be the profits out of which dividends can be paid. The macroeconomic world is a Wonderland full of widow's cruses and Danaïd jars, where nothing is what it seems, where things do not add up, where the collective result of individual decisions is something totally different from the sum of these decisions. Moreover, this is the real world: yet it cannot be understood by any generalization from individual experience; it can only be understood through the kind of intellectual discipline which economics provides. Moreover, it is not only in economics that this topsy-turviness prevails. In politics prohibition leads to drunkenness, the quest for national security leads to national destruction, the more literate we make people the less educated they become, and the conquest of nature by the physical sciences leads to ever-increasing misery, fear, and degradation.

III

I have not attempted in this paper to defend economics by reference to the importance of its subject matter, as that can hardly be a matter of question. Was Marx right in supposing that capitalism has an inherent contradiction in it? What

is the necessary minimum of governmental intervention into economic life? Can inflations and depressions be remedied? How far can the distribution of income be equalized without destroying the roots of economic progress? These and like questions cannot possibly be answered without serious study, and the name of this serious study is economics. One needs no more reminder of its necessity. It is trite, but frighteningly true, to say that the survival of this present civilization depends, not on the further development of natural science, but on the solution of certain serious intellectual problems in the social sciences.

In conclusion, I should like to urge the necessity for the study of economics not only for its conclusions and methods, but also for the state of mind it produces. In the old Cambridge tripos, economics—or, to give it its grander title, political economy—was listed as a Moral Science. For all the attempts of our positivists to dehumanize the sciences of man, a moral science it remains. Its central problem is the problem of value: and value is but a step from virtue. Every science, like every craft, imposes certain of its marks on its practitioners. I would hesitate to suggest, especially to members of the AAAS, that geologists grow like their rocks, chemists like their smells, or even astronomers like their heavens. I cannot forbear, however, from quoting from Professor Robbins, of the London School of Economics: "It is not an exaggeration to say that, at the present day, one of the main dangers to civilisation arises from the inability of minds trained in the natural sciences to perceive the difference between the economic and the technical." In the lurid twilight of science in which we live, when it has gained the whole world and lost its own soul, when it is everywhere prostituted to special interests, whether of the dairy farmers, the steel industry, or the national state, when the search for truth is subordinated to the lust for power, it is not altogether an accident that it is in the social science departments that the occasional voice crying in the wilderness is most likely to be heard. In a world of technicians, it is the economist who raises the cry that the technically most efficient in not necessarily, or even usually, the socially most efficient; that the best cow is not the one that gives the most milk; the best business is not the one that makes the most profits; the best army is not the one that creates the most havoc; and, above all, that the best training is not the best education. In a day when self-interest, nationalism, totalitarianism, militarism, and a dreadful pride threaten our very existence, economics points always toward the general interest, looks toward a free-trading world society, claims that the business of living even in a complex society can be accomplished with a small minimum of police coercion, urges that plenty is the source of power and war the greatest enemy of plenty, and by its very failures induces that humility for lack of which we perish.



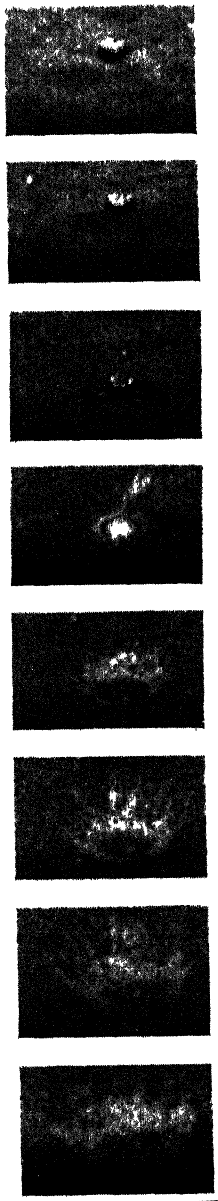
PROTECTING THE LAND AGAINST THE RAINDROP'S BLAST

W. D. ELLISON

Mr. Ellison is a graduate of the School of Engineering, Montana State College, and has been engaged in hydrologic and soils research for more than twenty years, mostly with the USDA. While director of the North Appalachian Hydrologic Research Station he developed a new type of water-cycle lysimeter that would weigh a 120,000-block of field soil with such accuracy that one could detect the collection of a heavy evening dew on its surface, and the water given off by plant transpiration during the day. Mr. Ellison is now Soil Conservationist in the U S. Navy.

EACH falling raindrop that strikes the ground acts as a miniature bomb and splashes soil into the air at its point of impact. Violent rainstorms have great capacity to splash the soil and to damage the land by this "blasting" action. Measurements made by new methods have disclosed wide differences in the destructive action of raindrops that fall in different types of storms. They have also divulged some new facts about the soil and have shown that different soils are destroyed in different ways, and at different rates, by the impact and splash of falling raindrops. These new data indicate that the amount of vegetal cover protection needed to conserve the soil will vary with these differences in rainfall and soil characteristics. Rainfall characteristics are known to vary from one climatic region to another, and from season to season within each region; and soils may vary from one field to another, and even from one acre to another on a single field.

Such variations point to an important need for measuring impact characteristics of storms, and resistance characteristics (stability factors) of soils, before attempting to fit a cropping practice to the land. Some of the wide differences in the factors of rainfall, soil, and protective cover are indicated by a limited number of measurements made during the past year. The results show that differences in drop impacts, from one storm to another, may cause the soil splash on a bare field to vary from less than 1 ton per acre to more than 100 tons per acre; that differences in soil characteristics may vary the splash by more than 450 percent; and that differences in the protective values of vegetal covers may cause the soil splash to vary by more than 10,000 percent. Such latitude in the controlling factors provides wide opportunity for setting up some new cropping practices; practices that are tailor-made to protect each field according to the soil's protective requirements.



Falling raindrop coming in contact with soil. Shots taken 1/1000 second apart show its splashing effects

Soil splash by raindrop impact represents a form of soil erosion that may be called splash erosion, and it represents the very beginning of erosion on land surfaces outside the gullies and channels. It sets in motion a chain of destructive events that are of outstanding importance because of their harmful effects on the land. Unfortunately, we have not been charging splash damage to the effects of raindrops. The splashes are much too fast for the unaided eye to detect, and, being unaware of their presence, we have generally attributed their harmful results to the erosive action of flowing surface water. It is true that surface flow often plays a very important role in this whole chain of destruction; but the surface flow, when acting alone, without the antecedent effects of raindrop splashes would be relatively ineffective on the fields where the flow is not channelized.

There have also been attempts to indict the moldboard plow for the damage caused by splashing raindrops. Obviously, the use of the

been so badly mismanaged that it has accelerated splash erosion, but many other things have also been mismanaged in ways that multiplied splash damage. For example, milch cows have been allowed to overgraze pastures and meadows to an extent that splash erosion became active and caused the soils to decline to less than 10 percent of full productivity. Solutions to the problems posed should be found in a study of the erosion process and in the development of management practices for the control of splash hazards. In the case of overgrazing, of course, the grazing cows need not be eliminated, and the same reasoning applies in the case of the moldboard plow. Perhaps the plow, too, should stay with us, but under improved management practices that provide for the control of splash erosion hazards.

Many types of damage to the land and soil are associated with, and proportional to, these hazards. Such damages are so numerous and complex that nothing more than a brief description of the principal types can be given here.

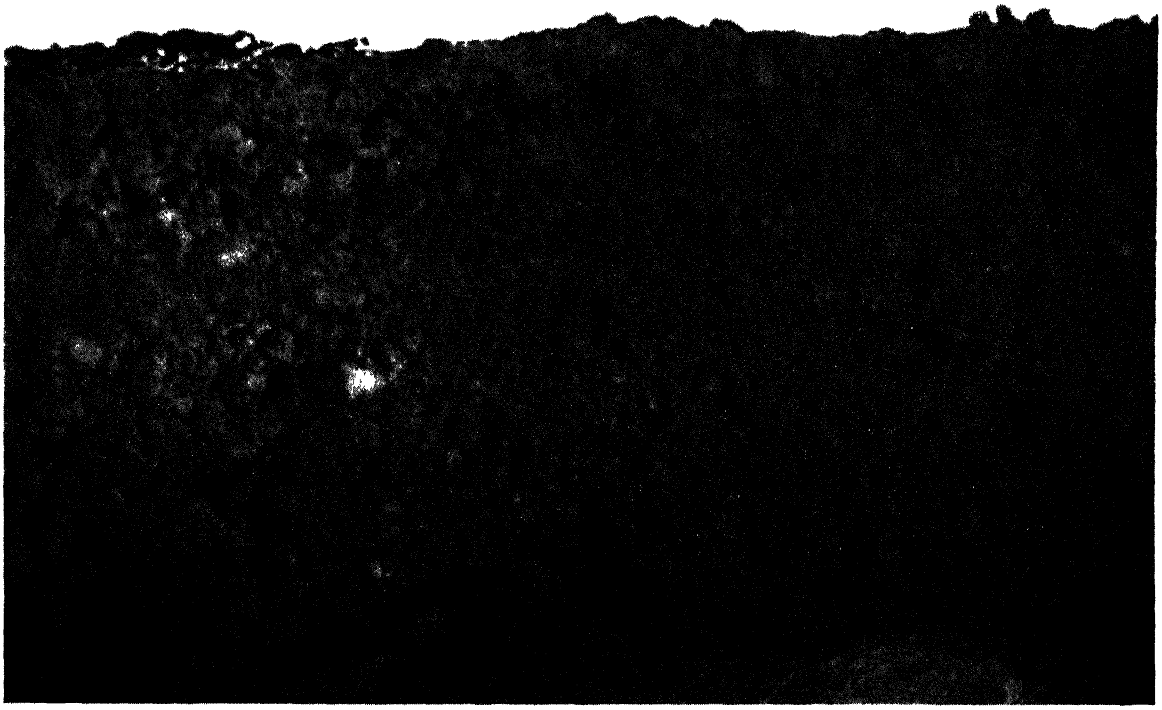
One harmful effect of splashes is to accelerate the loss of topsoil over the entire surface of a field. The splash process sets topsoil in motion and also places it in suspension in the surface water. This water is kept in a turbulent state (such that it continues to suspend much of the splashed soil) by subsequent drop impacts. If these subsequent impacts were eliminated, on field surfaces outside the gullies, the soil that is first splashed into suspension would soon be deposited out of the surface water. Thus, the effects of drop impacts are largely responsible for erosion outside the gullies; erosion that scalps crests of slopes of all their topsoils and often piles and buries much of this topsoil low on a hillside.

Even a considerable part of the gulley carving that is caused by flowing surface water should be charged to splash erosion. Active splash erosion may greatly increase the runoff and also tend to make the runoff highly abrasive. This increased flow containing highly abrasive materials will carve many gullies where gullies would not otherwise be formed.

The splash erosion process puddles surface soils and causes surface seals to be formed. Often these seals will practically waterproof the land and cause most of the rainfall to run off. This increased runoff tends to increase both frequency and intensity of floods and droughts. The high temperatures associated with droughty soils in midsummer may cause baking and cracking in the clays. Surface sealing may also cause poor soil aeration; it may destroy worm life and interfere with microbial action within a soil; and may impair the land's productive capacity in other ways.

Splash erosion may result in elutriation, or a washing out and floating away of the light organic

Soil set in motion high on the slopes is piled and buried low on the hillside. This hillside is not gullied, but all the topsoil has been moved downslope and piled six feet deep near the base of the slope.



Very permeable soil with a high infiltration capacity before bare surface was exposed to 20 minutes of rainfall. Note that surface has been practically waterproofed.

materials and the fine clay factions. If the soil contains many coarse sand grains, these may be deposited out of the flow and left on the surface of the field. When this happens the runoff carries with it the very life of the land, leaving behind a topsoil that contains only sterile sand. This process tends to deplete both the fertility and the water-holding capacity of sandy fields.

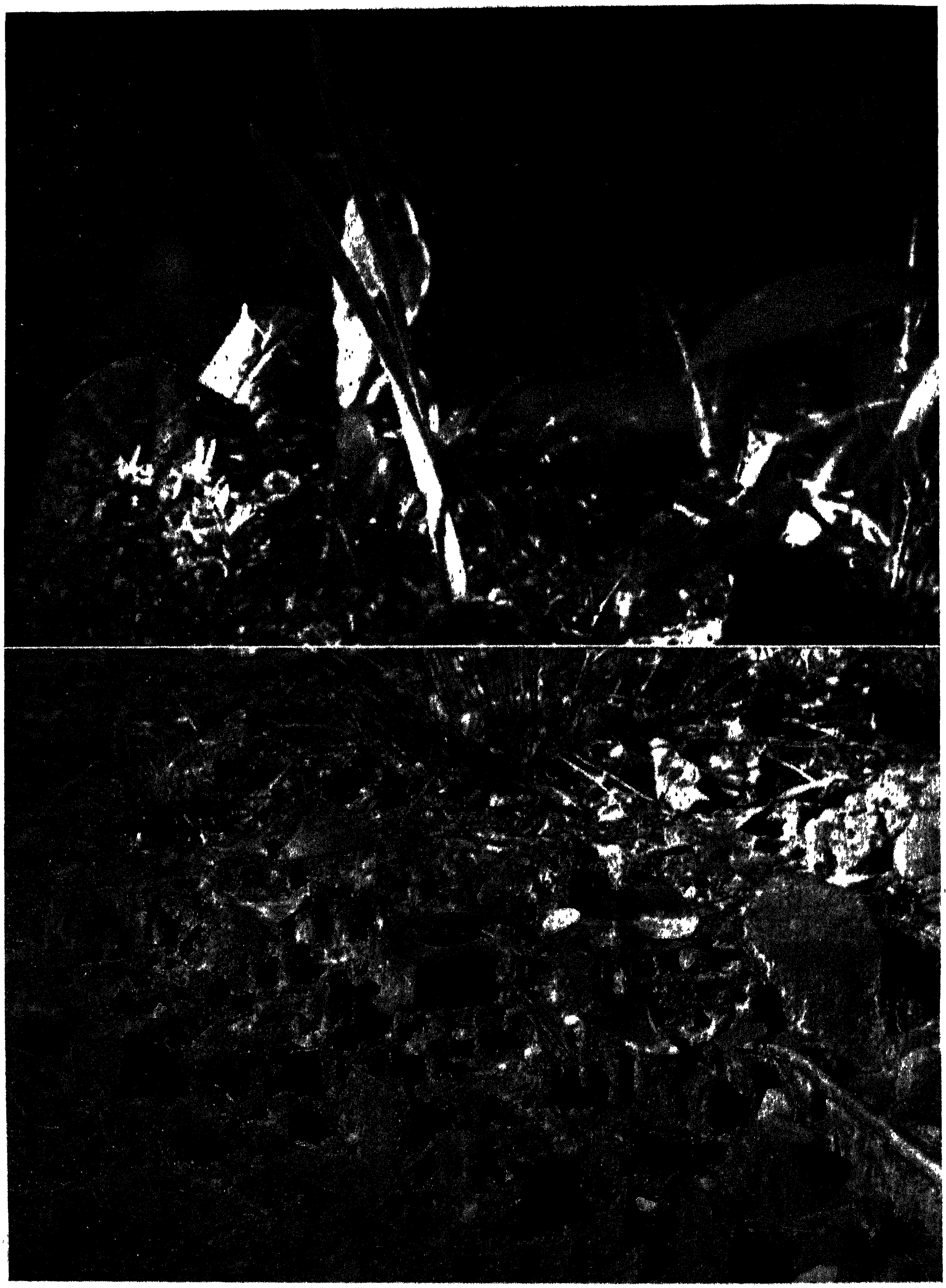
All the various types of damage described above will tend to produce soil structure decline. When the broken aggregates and the puddled surface materials are incorporated into the plow layer, they make this part of the profile less permeable. Loss of organic materials in the elutriation process may also impair a soil's structural properties. Each type of damage should serve as a warning that splash erosion is active on the land.

Soil pedestals on the surface are made by the splash process, and they, too, serve to warn us of actual splash erosion on a field. These are formed under small stones, and where stems of plants, leaves, or other nonerodible objects lie fixed on the surface. These objects protect the material under them from splash by raindrop impact, whereas soil all about them may be splashed and transported away. These pedestals have been formed by splash erosion in many experimental tests, but never by surface flow. This fact is of especial interest because of published statements to the contrary, statements that have developed some popular misconceptions.

In experiments conducted by the Navy, splash action that erodes the soil was photographed with Navy cameras, using a single drop of 5.7-mm diameter. This drop was released from a height of 32 feet, and at the point of impact its velocity was approximately 32 feet per second. The capacity of this drop to cause soil splash is only slightly less than that of a raindrop of like size that falls on an open field during a quiet spring shower, but it may be less than one fourth as effective as a raindrop of like size that is driven into the soil by the high winds of a beating rainstorm.

Splash erosion science is a very new development, but already it has divulged facts that give us leads for improving many of our erosion-control techniques. Results have also shown up deficiencies in some of our experimental findings, especially those findings in which there has been no accounting for differences in the splash factors: the factors of drop impact, soil capacity to withstand such impact, and the protective value of the vegetal cover. Without measuring these basic factors, a comprehensive study of important cause-and-effect relationships in erosional damage has been impossible. Perhaps our failure to account for differences in these primary factors is the underlying reason why many of the present-day conservation practices still have to be based more on trial-and-error developments in actual field operations than on experimental findings in research programs.

The procedures and techniques employed in this new science have been developed by the author over the past twelve years, but they were first



Above: Splashing raindrops with about 70 percent vegetal cover. *Below*: Denuded area where cap rocks protect small pedestals of soil against raindrop action.

tried out on practical field problems in 1948. Sponsored by the regional office of the Soil Conservation Service, Ft. Worth, Texas, the tests were made on rangelands at the Amarillo Experiment Station. The object of these tests on rangelands was to determine how much of the range cover can be removed by grazing at each season of the year without impairing the soil- and water-conserving functions. Results of this work were so highly promising that plans have since been completed for continuing this operation and extending it over the entire state. These same types of measurement are needed to determine minimum cover requirements on the cornfields of Iowa, the wheat fields of Kansas, the potato fields of Maine, the citrus orchards of California, and the cottonfields of Mississippi. Such determinations are basic requirements for developing the soil-protection phases of farm and range management programs everywhere.

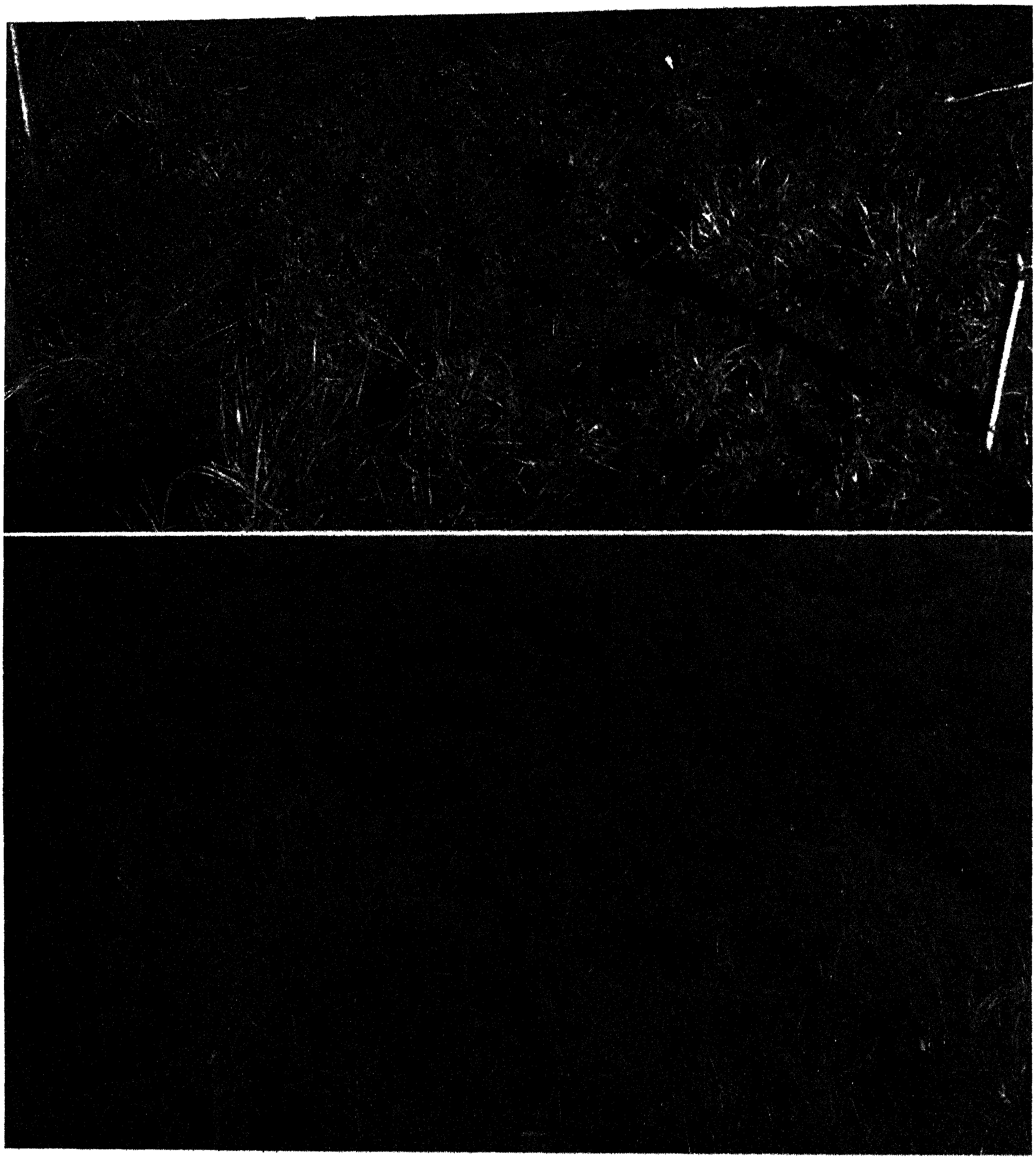
For the purposes of this discussion soil erosion will be defined as a process of detachment and transportation of soil materials by erosive agents. This definition describes the erosion process as being comprised of two sequential happenings: detachment—a tearing loose of transportable particles from their moorings in the surface of a soil mass; and transportation of these detached materials. This definition provides no specification as to the destination of materials that are transported It does not preclude the possibility of soil being transported to and fro on the field, and of its being finally deposited in the vicinity of its initial location In that case there would be soil erosion without any appreciable net loss of soil from any point. A considerable amount of this type of erosion often takes place, especially by the splash process, on gentle slopes. But even though the tonnage loss of soil is inappreciable, the erosional damage can be great. The soil can lose important clay fractions, fertility elements, and organic materials, and its physical properties may be impaired in the process of being eroded. The extent of these erosional damages will depend upon the two primary erosion hazards—detachment and transportation. Where there is no appreciable soil loss, the detachment process, not the transportation, will be most destructive of the soil.

Most of the soil damage previously described as structure decline, surface sealing, elutriation, and to some extent the loss of topsoil from field slopes—and even the increases in abrasive characteristics of surface flow which cause the runoff to carve gullies—may have its beginning in soil detachment by raindrop impact. It is important, therefore, that we measure this detachment and determine its sensitivity to variations in the detaching capacity of falling raindrops, the detachability of the soil, and the protective value of the vegetal cover. After this has been accomplished we should be well prepared to match an effective cover management program to the protective requirements of a soil.

Surface of a bare sandy soil after a fairly heavy rain Organic materials and clay fractions have been splashed and transported from the field. The less transportable sand fractions remain and will eventually be incorporated into the plow layer.

The detaching capacity of falling raindrops may be designated by the symbol D_3. It is measured by exposing a small standardized dish of a standard sand to the impact and splash of falling raindrops. As the drops fall and splash the sand from the dish, the detachability is assumed to be 1.0 and one unit of detaching capacity will splash one unit weight (1 gram) of sand from the dish. To determine the weight loss, the dish of sand is ovendried and weighed both before and after exposure to rainfall. The D_3 factor is then equal to the total grams of standard sand splashed out of the standardized dish during the period of rainfall.

If a rainfall having a detaching capacity of 10 (i.e., it will splash 10 grams of standard sand out of a splash dish) splashes only 6 grams of a particular soil, then the detachability factor D_2—the



Above: Poor range cover, with protective value of only 2.1. *Below*: Good range cover gives a protective value of 131.

amount of soil splashed divided by the detaching capacity of the falling raindrops—is equal to 6/10 = 0.60.

It becomes obvious that the total soil loss from a splash dish is equal to the product of soil detachability and raindrop detaching capacity. This quantity we designate by the symbol D_1. This is the detachment hazard, and it can be determined for a condition of bare soil by the formula $D_1 = D_2 \times D_3$. In this case the D_1 factor (the detachment hazard) will be expressed in terms of grams of soil splashed from the standardized dish. The dish is so small that it is assumed that all the soil detached is splashed into the air and lost from the dish. Since one gram of soil on the surface of the dish is equivalent to 0.71 tons of soil on one acre of land, the D_1 factor can be converted to tons per acre by multiplication $(D_2 \times D_3 \times 0.71)$.

A vegetal cover reduces the detachment hazard by checking the impact energy of the falling raindrops and thereby reducing the effectiveness of the D_3 factor. The protective value of the cover may be designated by the symbol R, and it may be thought of as an impact-conditioning factor. If a cover reduces the effectiveness of drop impact from 100 to 50, the value of R is then 2; if the reduction is from 100 to 10, then the value of R is 10. The value of R is equal to the amount of splash that

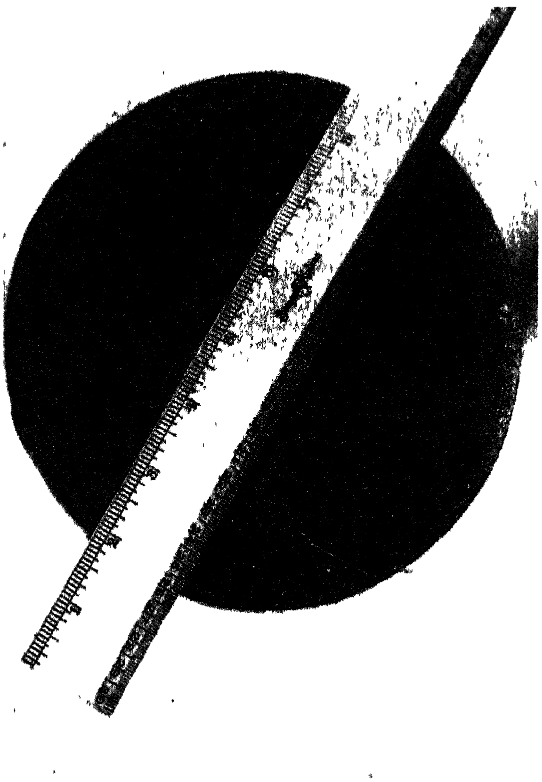
occurs without a cover, divided by the amount of splash that occurs with the cover on the soil. The formula applying to the soil with vegetal cover is $D_1 = D_2 \times D_3/R$ (multiplied by 0.71 to convert to tons per acre).

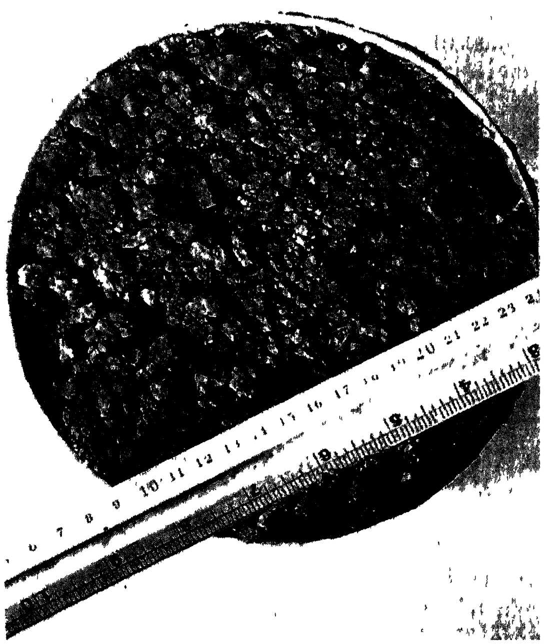
It is interesting to explore some practical applications of these techniques using results obtained in experimental studies. Detaching capacities have been measured in excess of 100 for natural rainfall, and detachability of different soils has varied from a low of 0.2 to a high of 0.9. (A greater spread in this factor is expected, perhaps reaching to a low of less than 0 1.) Protective values of cover in the range work varied from a low of 2.1 to a high of 250. To explore the effects of these variations in soils and covers, let us use a D_3 value of 100.

On soil having a D_2 value of 0.2, the detachment hazard $D_1 = 0.2 \times 100 \times 0.71 = 14.2$ tons per acre for a bare field. The R value of 2.1 was measured on a very poor range having only 704 pounds of forage and litter per acre. With this type of range the detachment hazard would be $D_1 = 0.2 \times \frac{100}{2.1} \times 0.71 = 6.8$ tons per acre. With an R value of 250 (measured on a very excellent range having 5,715 pounds of forage and litter per acre) $D_1 = 0\,2 \times \frac{100}{250} \times 0.71 = 0.067$ tons per acre.

On the soil having a D_2 value of 0.9 for a bare field, the detachment hazard $D_1 = 63.9$ tons per acre. With an R value of 2.1, this hazard would be 30.4 tons per acre, and where $R = 250$, D_1 would be 0.25 ton per acre.

The magnitude of the detachment hazard that can be tolerated without causing excessive damage on the different fields and ranges will vary widely from one area to another. Controlling considerations are the types of soil damage caused; for example, where the slopes are steep, or where the soils are highly transportable, high rates of detachment are certain to result in considerable soil loss. Where the soils are sandy, high rates of detachment will cause rapid soil decline through high rates of elutriation. If the soils have relatively unstable aggregates and contain considerable clay, surface sealing may reduce water intake and lead to excessive droughtiness and to high rates of flood





Two pictures of the same soil. That on the left is from a point in the field where splash erosion has been active for a long period of years. It will be droughty, will produce high flood runoff, will be poorly aerated and difficult to farm. It will not be inhabited by sufficient worm life and will not be fully

Splash dish with wick leading into ordinary fruit jar

runoff. Combinations of several types of damage are certain to lead to decline of soil structure within the upper root zone. But where the land is fairly level, and the soil is neither excessively sandy nor clayey, and where the aggregates are fairly stable against breakdown by raindrops, the detachment process may not be particularly destructive. In these gently sloping areas of fairly stable soils, the detachment hazard may be permitted to increase above those tolerated on areas more subject to splash damage.

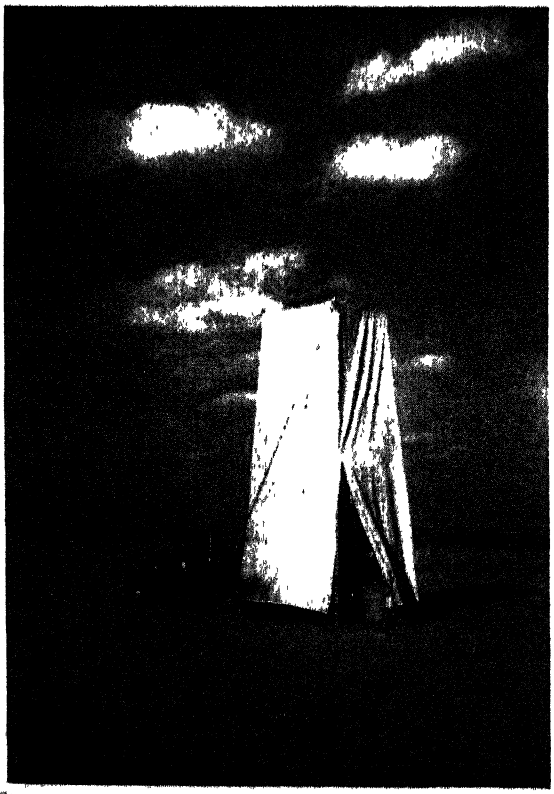
Sealing of the surface soil to an extent that soil moisture and ground-water supplies become deficient, and flood runoff becomes destructive, represents a major erosional damage that occurs on practically all soils. On one of the soils that had an infiltration rate of 0.36 inch per hour when utterly bare, the infiltration was 0.96 inch per hour under a protective cover $R = 2.1$, and it was 9.36 inches per hour under a cover $R = 250$.

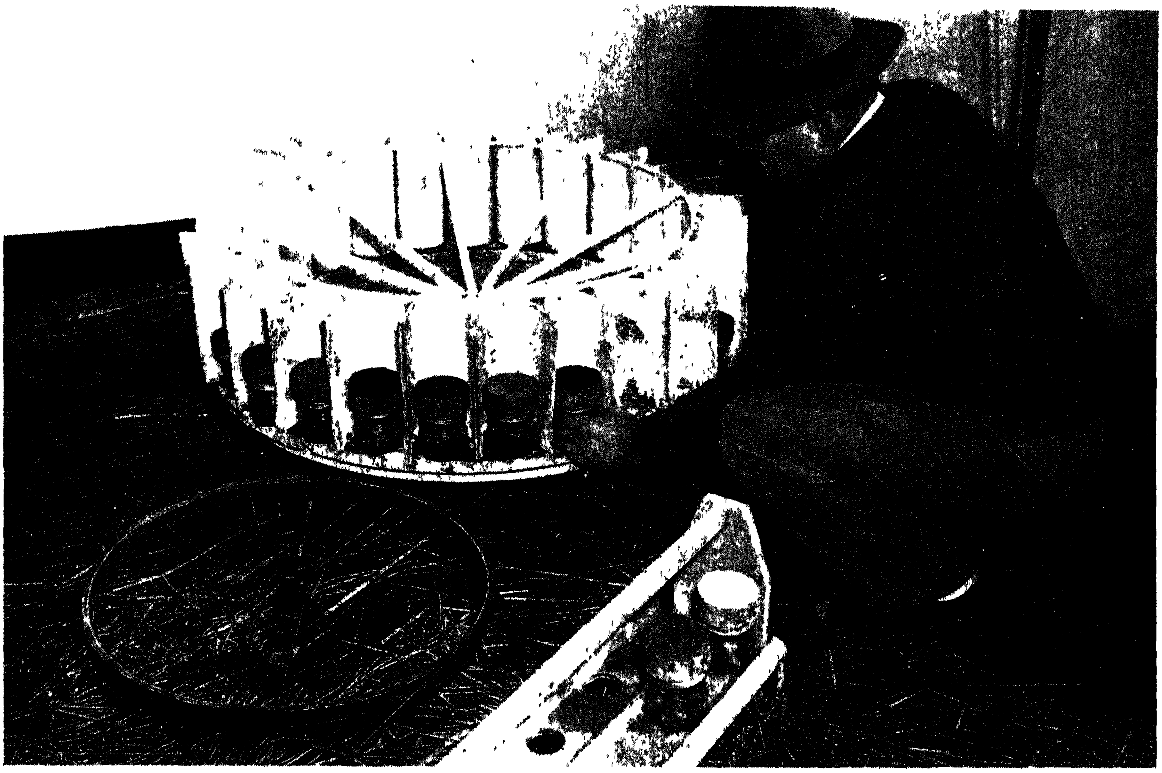
These figures on water intake may be more readily understood if they are expressed in terms of gallons per acre that the soil is capable of taking through the surface layer during one hour of rainfall. The bare soil just referred to would take in only about 9,500 gallons per acre; the soil protected by a cover $R = 2.1$ would take in about 26,000 gallons per acre; whereas the soil protected by a cover $R = 250$ would take in 255,000 gallons per acre in one hour. To those dependent on ground water for industrial supplies, these figures hold great significance. To those dependent on the rain that falls (where there is no irrigation) to produce a crop, these facts should be of more than casual interest. To those who would introduce agricultural prac-

these facts must serve as guides for their flood-reduction programs. They indicate that a water-conservation practice for open fields should be effective before the raindrops puddle the soil and muddy the water. This puddling compacts the surface materials, and the inflow of muddy water further plugs the surface pores in about the same way that a dirty fluid might plug an ordinary kitchen strainer. When this happens the surface of the land is almost waterproofed. This curtails the benefits of rain and at the same time multiplies the damage caused by flash runoff.

The foregoing presents a brief over-all picture of splash erosion phenomena, the damage it produces, procedures and techniques employed to measure the separate factors involved, and some of the results obtained through these measuring techniques. Additional information on procedures, equipment, and techniques may be of interest.

The splash dish is 3.5 inches in diameter, 1.5 inches deep along the edges, and has a funnel-shaped bottom about 0.75 inches deep. A cloth wick prevents soil from falling through the hole in the bottom of the dish, and it overcomes surface tension of the water that percolates through the soil, and leads it down into a glass jar. When this dish is filled with standard sand and set in the open





Turntable being loaded with splash dishes full of soil to be tested.

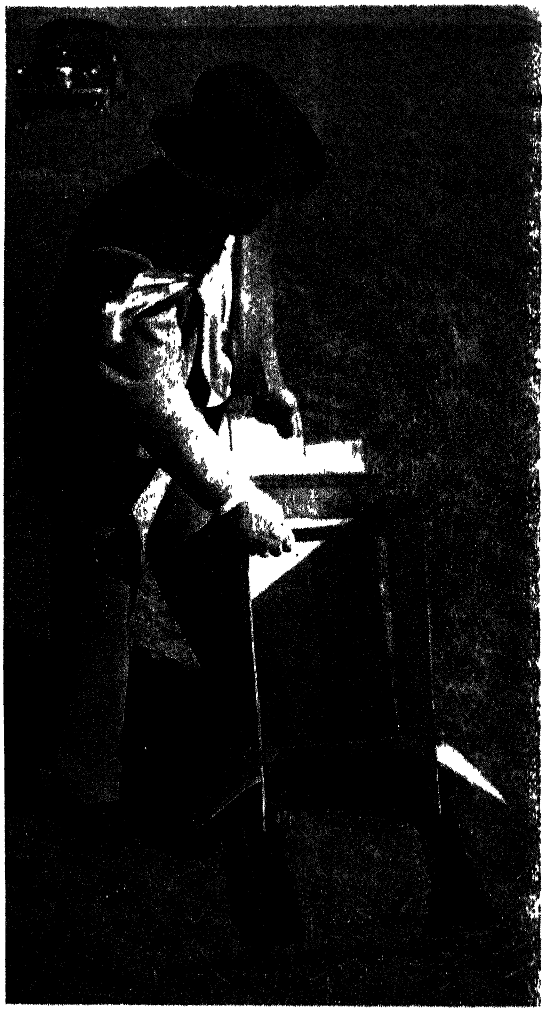
to measure the detaching capacity of natural raindrops, the glass jar is partially filled with water so that the wick carries enough moisture up into the sand to keep it wet and prevent its being blown away between periods of rainfall. The standard sand is a washed laboratory sand of 60–70-mesh size, having rounded edges.

The major piece of equipment constructed for this work is the rainfall applicator, mounted on a pickup truck to make it readily transportable. This apparatus controls each of the three variables of rainfall that affect detaching capacities. These are drop size, drop velocity, and rainfall intensity. By having control over each of these, it is an easy matter to maintain uniform rainfall with constant detaching capacity throughout a series of tests.

The water supply is carried in a pressure tank, and air pressure from a spark-plug pump drives this water up through a garden hose to a set of four spray nozzles. Suspended from the frame that holds these nozzles is a chicken-wire screen, covered with muslin. This muslin is depressed in each opening of the wire, and a yarn string about 2 inches long is run through the muslin, knotted on top, and permitted to hang from the center of each depression. The nozzles spray the water upward over this screen, and it falls back onto the muslin. Drops form as the water runs down these strings and drips off the lower ends—large drops from large strings and small drops from small strings. Only one size of yarn is used in each screen, but different screens are made up to form the different drop sizes. A 20-foot extension ladder is mounted on the back of the pickup truck so that the screen can be hoisted to any desired height. Drop velocities are controlled by the height to which the screen is hoisted and the drops are released.

Rainfall intensities are controlled by the number of nozzles used above the screen and by the amount of pressure that is developed in the pressure tank. Either two or four nozzles may be used, and operating pressures may be varied from about 20 to 30 pounds. The falling drops are protected against wind currents by a canvas curtain suspended from the top of the ladder. This equipment is used for applying raindrops when testing either the soil's detachability or the protective value of the cover on open fields and ranges.

The detachability factor should be determined for at least two different conditions of a soil. One of these will be a pulverized condition, and the other will be for a soil in field condition. For the pulverized condition, all large aggregates are removed, so that those remaining are highly transportable by the splash process (maximum sizes about 2 mm). The aggregates remaining should also be stable against breakdown by drop impact, so that for all practical purposes they act as separate particles. Under these conditions, the detachability of the soil material is controlled largely by interparticle phenomena (with aggregates acting as particles). If the particles and small aggregates are angular, so that they interlock and resist separation, the detachability tends



Splash frame. One end has been removed to show inside of apparatus.

to be lower than if they are rounded. Properties and amounts of the clays present are also important considerations.

The above-described detachability test is made with a splash dish of pulverized soil placed on a rotating table. A number of samples are set around the outer edge of the table, and it is rotated at about 5 r.p.m. As the table is turned, all the samples rotate through a common path, and this tends to smooth out and equalize the drop impacts applied to each sample. It was seldom that the amount of detachment (D_1) varied more than 5 percent between different samples of the same soil.

The test of soil in field condition can be made in either of two ways. One method is to use undisturbed cores of soil in splash dishes set on the the rotating table. When this is done the funnel-shaped bottom portion of the splash dish is filled with standard sand, and the soil core rests on top of this sand base. The surface must be free of all vegetable cover for this test. Intra-aggregate phenomena, as well as interparticle phenomena will affect the detachability of a soil in field condition

A second method for testing detachability of a soil in the field employs a splash-frame type of sampler. This frame is set in the field so that it encloses a small plot of undisturbed, bare soil. When rainfall is applied, the splash inside this frame strikes the side walls of the sampler and flows down into small troughs, where it is preserved. The soil carried by these splashes, measured in grams, is the D_1 factor. A new D_3 factor is needed for this larger sampler. It may be obtained by setting the splash frame on a plot of the standard sand and applying rainfall. The amount of sand splashed, measured in grams, represents the D_3 value. After D_1 and D_3 have both been measured by these methods, the D_2 factor is computed from the formula $D_1 = D_2 \times D_3$.

The splash-frame type of sampler is also used to measure the protective value (R) of vegetal covers. In one method of measuring this value, standard sand is spread beneath the vegetal cover, over the surface of the soil, but inside the splash frame. In this test the amount of sand splashed by those drops not intercepted by the cover is the D_1 factor. Since the detachability of the standard sand is one (unity), the equation $D_1 = D_2 \times D_3/R$ now becomes $R = D_3/D_1$.

The other method for measuring R is based on first determining the detachability of the soil in field condition, as it is found under the vegetation. The D_1 factor is measured in a test with the splash frame, on a plot where D_2 is known. The protective value of the vegetation is then computed from the formula $R = D_2 \times D_3/D_1$.

Different sizes of frame samplers are used, but a sampler about 1.5 feet long and 0.5 foot wide will suffice for most conditions. Each time the dimensions of this frame are changed, a new D_3 value must be obtained. Each change in samplers also requires a new set of conversion factors for converting detachment hazards to units of tons per acre. All the splash within the frame is not caught with the sample, and so the sample cannot be proportioned by area factors alone. Since it is assumed that all splash on the splash dish is splashed out and can be represented by the measure of soil loss, we use this dish as a basic unit for purposes of conversion. The first step is to find what portion of the total splash in the frame is represented by the splash sample. The formula is

$$\frac{\text{(Area of frame sampler)}}{\text{(Area of dish sampler)}}$$

$$= K \frac{(\text{Splash sample caught in frame—gm})}{(\text{soil lost from splash dish—gm})}$$

The second step consists of proportioning the area of the splash frame and its splash sample multiplied by K to one acre. This formula is

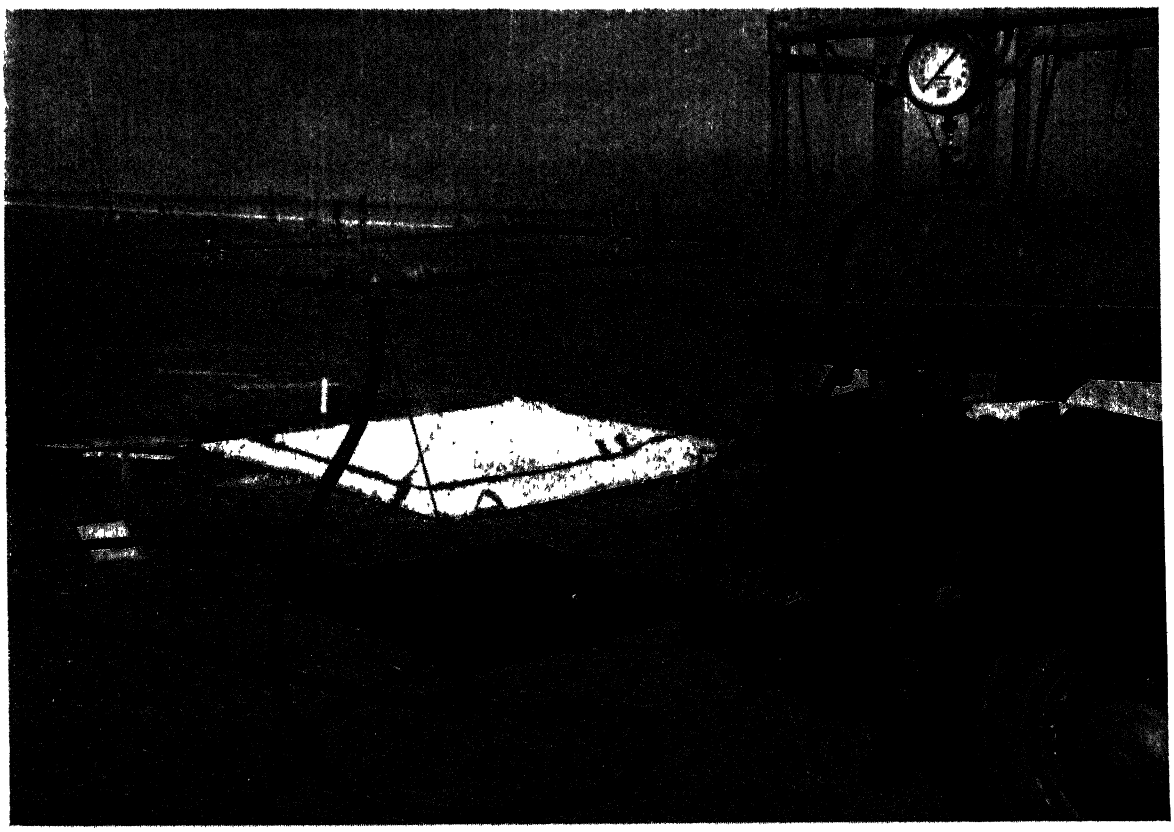
$$\frac{(43{,}560 \text{ sq. ft. in one acre})}{(\text{sq. ft. area of frame sampler})} \times \frac{(\text{splash caught in frame sampler—gm})}{453.6} \times (K) = \text{splash in pounds per acre.}$$

This work represents a new attack on problems of erosion and protection of the soil against erosional damage. It is based on a new definition of erosion, and it recognizes the impact of the raindrop as a destructive, erosive agent that must be checked before effective soil and water conservation can be achieved. So long as we blamed flowing surface water for all the erosional damage to our fields, our erosion-control practices were top-heavy with contouring operations that control only surface flow. We have talked about cover crops, but not in terms of their controlling splash erosion. Most of the cover crops used have been winter covers, which are useful in controlling leaching losses, but in matters of splash control they are usually most effective during the seasons when splash erosion is the very least. All too often the fields will be found in an unprotected condition at times when rainfall can be most destructive.

This new attack on the soil erosion problem should lead to a broad, popular recognition and appraisal of the three important factors that are so basic to the job of protecting a soil's resources. Such an appraisal is needed before we can achieve soil stability on our fields and ranges, and it may lead to the next important development in soil- and water-conservation practices.

Raindrops have always been represented to us in the role of a friend. Perhaps this fact alone has made it difficult for us to believe that they would destroy our lands. But history records many failures of entire nations whose fields were terraced but whose farmers were unaware of the invisible action of the splashing raindrops. Their terraces delayed, but they could not prevent, the inevitable destruction of their uncovered fields. Even where these terraces were constructed of massive stone, they could not possibly provide the protection their soils needed—protection against the raindrop's blast.

Photographs by the U. S. Soil Conservation Service and the U. S Navy.



Truck with pressure tank and extension ladder mounted

GROWTH POTENTIALS OF THE HUMAN INFANT

ARNOLD GESELL

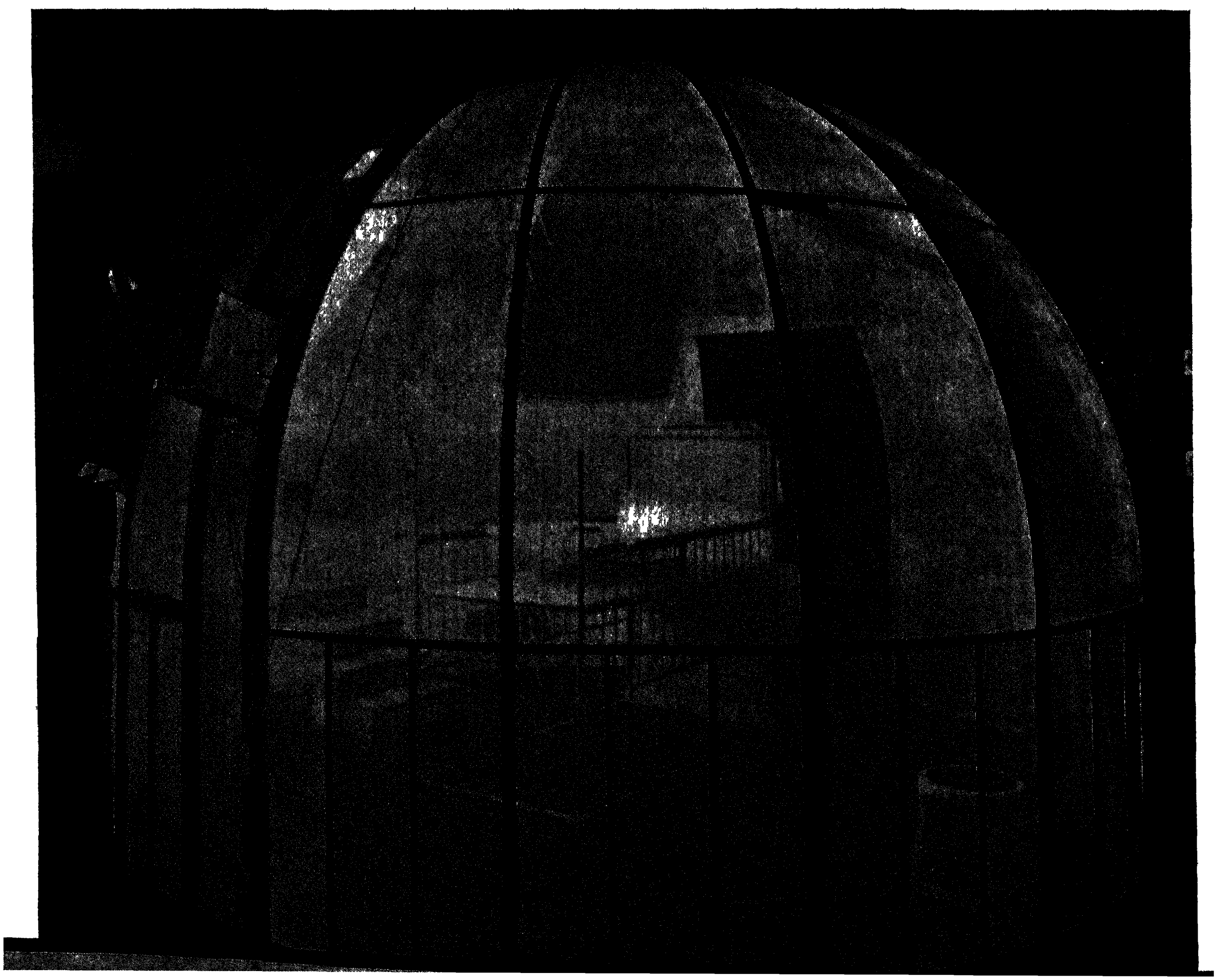
Well known for his work in developmental pediatrics, Dr. Gesell (Ph.D., Clark, 1906; M.D., Yale, 1915) is the founder of the former Clinic of Child Development at Yale University. His article is based on the address he gave in the symposium on "Educability" during the Centennial Celebration of the AAAS.

ALL educability is dependent upon innate capacities of growth. This intrinsic growth is a gift of nature. It can be guided, but it cannot be created; nor can it be transcended by any educational agency.

The problem of human educability therefore must reckon with two closely related concepts, namely, *learning* and *growth.* Darwin understood this when he suggested toward the end of his life that more accurate knowledge of the development of infants would probably give a foundation for some improvement in their education.

From the vantage point of post-Darwinian science, we begin to see each child as the focal end product of age-old processes of evolution. Biologically considered, infancy is a period of formative immaturity, which is most prolonged and most intensified in the very species which stands highest

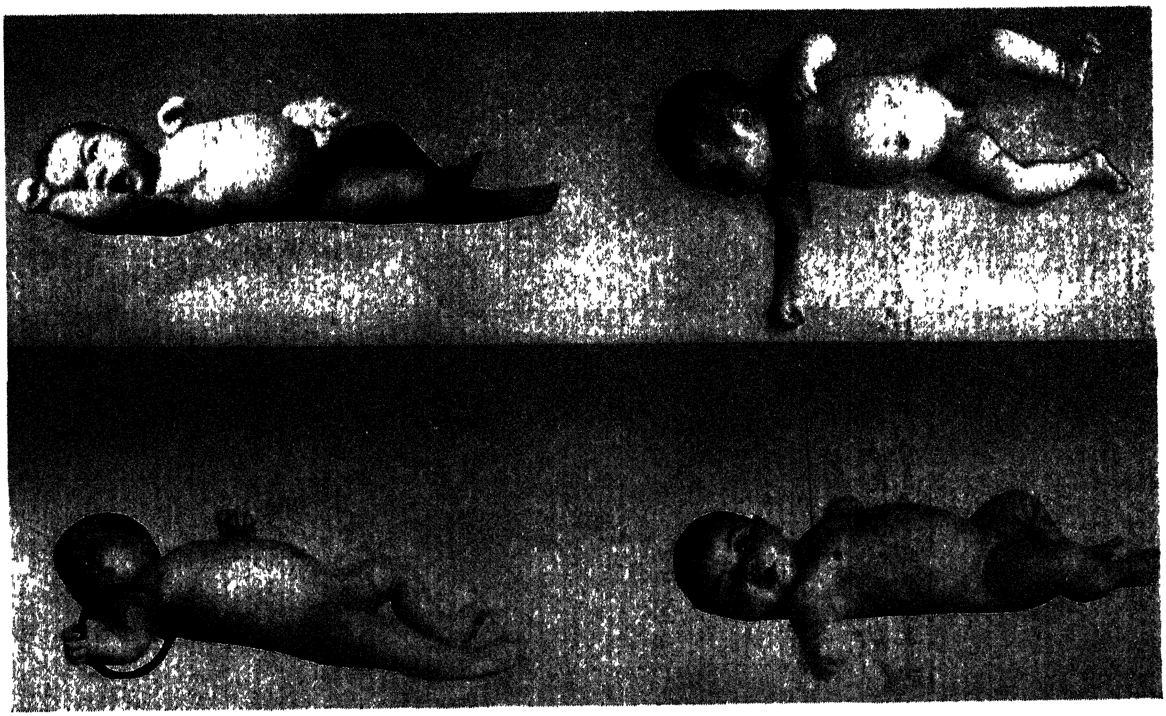
The photographic dome. This structure served both as an observatory and as a device for systematic cinema recording of the growth stages of infant behavior patterns. The interior was illuminated with Cooper-Hewitt and Mazda lamps, and two silent 16-mm cameras were mounted on the quadrants. A clinical examining crib and test materials were used to elicit behavior under standardized conditions. The dome was encased in a one-way vision screen, which effectively concealed observers and operators stationed outside.



in the life scale—presumably in our own! Bernard Shaw rather deplores this circumstance. Accordingly, in his *Metabiological Pentateuch,* he arranges matters otherwise; and on a summer afternoon in the year A.D. 31,920, the Newly Born emerges from a fabulous eggshell (some "filaments of spare albumen clinging to her here and there") —an exquisite creature endowed with speech and the full-fledged intelligence of a seventeen-year-old youth. With such a precocious start and a Back-to-Methuselah life cycle, the race may indeed be in a better position to cope with its cultural problems.

The infant of today, nevertheless, is born with prodigious powers of psychological growth. Note how swiftly and progressively he gains command of his eyes and hands. Very soon after birth he is able to fixate an eye upon an object of interest; first he fixates with one eye, then with the other, later with each in rapid rhythmic alternation, and later conjointly and convergently. At four weeks he gives sustained binocular regard to an object brought into his line of vision; at eight weeks he follows it with head rotation; at twelve weeks he looks regardfully at his own hand; at sixteen weeks, when seated in a supportive chair in front of a test table, he can focus eyes upon a tiny pellet 7 mm in diameter. He takes hold of the world with his eyes before he does so with his hands. But at twenty-four weeks he can pick up a one-inch block on sight. His early manual grasp is pawlike, palmar and ulnar. Soon it becomes digital and radial. At forty weeks he picks up the pellet with fingertips by precise pincer prehension. At fifteen months he releases this same pellet into a bottle; he adaptively superimposes one block upon another. At eighteen months he builds a tower of three blocks; at two years, a wall; at three years, a bridge.

I rehearse this brief tale because it illustrates what the infant himself rehearses by way of his own ontogenetic development. The significant fact about these patterns of eye-hand behavior is that they are not products of formal instruction, training, nor of education in a narrow sense. They are self-taught, self-initiated. In sequence and form they represent a generic ground plan of child development. The ground plan is primarily determined by genes. Environmental factors support, inflect, and modify, but they do not generate, the basic progressions of development. When the infant enters the world, he is already in possession of fundamental growth potentials which are distinctively his own, though phyletic in their origin,



The tonic neck reflex pattern exhibited in a normal infant at one week, six weeks, eight weeks, and twelve weeks of age. Eyes and hands function with increasing coordination after the third month, as shown on page 254.

With the aid of motion-picture cameras at the Yale Clinic, we have documented thousands of behavior patterns and pattern phases at thirty-four progressive age levels, from the period of fetal infancy through the first ten years of life. Growth gradations were charted in four major fields of behavior: motor, language, adaptive, personal-social. These objective records show that, although no two individuals are exactly alike, all normal children tend to follow a general sequence of growth characteristic of the species and of a cultural group. Every child has a unique pattern of growth, but that pattern is a variant of a basic ground plan. The species sequences are part of an established order of nature. Accordingly, the eyes take the lead, the hands follow; palmar grasp comes before digital; creeping before walking; crying before laughing; towers before walls; vertical crayon strokes before horizontal, and horizontal before oblique. First the blade, then the ear.

Growth is a step-by-step process. Each step is made possible by the step that preceded. The mind thus grows by natural stages. Maximum educability is realized only when educational measures are attuned to the maturity status of the organism. We have demonstrated the significance of maturity factors by extensive use of the method of co-twin control. One of a pair of extremely similar, single-egg twins was intensively trained for periods of six to eight weeks in a specific activity; the other twin was reserved as a comparative control. Objective data supported by cinemanalysis were gathered for stair climbing, constructive play with blocks, vocabulary training, digit and object memory, and motor skills in ring tossing and paper cutting. In none of these activities was it possible to confer a permanent advantage of skill upon either twin. After a lapse of a few weeks or months, the performances of the twins on the various tests were as similar to each other as at the beginning of the given experiment. I hasten to say that this does not prove that twins (or singletons) should not be educated. We simply have demonstrated in quantitative terms that the efficacy of training varies enormously with the developmental readiness of the infant and child.

Maturation is the net sum of gene effects operating in a self-limited life cycle. If you are reluctant to acknowledge the educational importance of genes, you may say, "This is all very well for such physical reactions as walking, stair climbing, block building, writing, drawing, and motor skills. But does it apply to emotions, to morals, to personality, and to the spiritual aspects of childhood?"

A lawful growth sequence illustrated by cube behavior patterns. Crude radial prehension at twenty-eight weeks; a wall at two years; a bridge at three years; and a gateway at four years.

Our studies show that the higher psychical manifestations of child life also are profoundly subject to laws of development. From the standpoint of development, body and mind are indivisible. The child comes by his mind as he comes by his body, through the organizing processes of growth. Psychically, he inherits nothing fully formed. Each and every part of his nature has to grow—and his sense of self, his fears, his affections and his curiosities, his feelings toward mother, father, playmates, and sex, his judgments of good and bad, of ugly and beautiful, his respect for truth and property, his sense of humor, his ideas about life and death, crime, war, nature, and deity All his sentiments, concepts, and attitudes are products of growth and experience. For all these diverse areas of behavior it is possible to formulate gradations and gradients of growth which represent the natural maturational stages by which the child assimilates the complex culture into which he is born.

The culture also assimilates him through its "gigantic conditioning apparatus." But the process of acculturation is fundamentally delimited and pervasively patterned by the mechanisms of maturation inherent in the individual.

Educability is delimited and configured by the selfsame mechanisms; for educability does not depend upon a formless kind of plasticity. It depends upon the structured nascencies of the mind as a growing organism. The human mind is a minutely architectured action system which has an embryogenesis and a developmental morphology, manifested in patterns of behavior. The forms and lawful sequences of these patterns can be defined by scientific methods. This is the task of a genetic and clinical science of child development.

More knowledge needs to be applied at the beginnings of the life cycle to reduce the mounting tide of adolescent instability and of adult abnormalities of behavior. Through broadened methods of developmental diagnosis and supervision in infancy, through individualized growth guidance in nursery and elementary schools, we can strengthen the stamina of the child and of the family unit. We can foster basic virtues and discover distinctive gifts and talents—academic and nonacademic—in the early years of life. We cannot make democracy a genuine folkway, unless we bring into the homes of the people a *developmental philosophy*

of child care that is rooted in scientific research.

No one has to teach a baby the elements of growing. He knows all that by heart, for nature drilled it into him through countless ages of evolution. What is more, nature compounded him so ingeniously that no one just like him will ever be born again. He is an individual. Under given environmental conditions, his inborn growth potentials will govern the extent and the modes of his maturing. His growth characteristics constitute the very core of his individuality, and by the same token his educability. To rear him aright, whether at home or at school, we must understand his individuality.

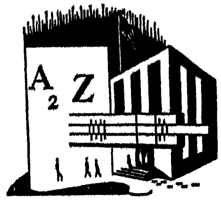
He manifests this individuality from the very beginning in his natural rhythms of feeding, sleep, and self-activity. Given wisely managed opportunity he seems to know when to sleep, when to be hungry, and how much to sleep and eat. His educability is not so bland and undifferentiated that he responds neatly to an iron-clad feeding schedule. Things work out better, if his own self-regulation mechanisms, which are really growth mechanisms, are given a reasonable scope. The discerning physician makes no arbitrary distinctions between physical and mental factors; he gives conjoint consideration to the infant's nutritional status, to his immunities, allergies, and behavior traits. The child grows as a unit.

The task of the culture, likewise, is to watch for signs and symptoms of the child's total well-being with a special concern for psychological health. We must go along with the baby far enough to build up in him a sense of security. Step by step it is possible to build up his self-confidence through strengthening his confidence in his caretakers. Gradually he gains in morale and social insights, not through sheer indulgence, but through perceptive guidance on the part of his elders. And the more these elders know about the processes of growth, the more they will enjoy the truly remarkable progress which normal children make even in the first five years of life.

The intrinsic badness of children has, in my opinion, been vastly exaggerated by distorting interpretations. Well-constituted children with healthy inheritance have an intrinsic charm—a charm which betokens intrinsic goodness. The growth potentials for good far outweigh those for evil, unless the cultural odds are too heavily weighted against the child.

It is too freely said that science is indifferent to human values. I would say in this connection that science by implication is always concerned with values, and the life sciences which deal with the physiology and the pathologies of growth are coming profoundly to grips with the deepmost determiners of human values. The race evolved, the child grows. And we shall not have the requisite self-knowledge to manage our culture until we make a more sedulous effort to understand the ways of all growth and the potentials of child growth, which are the culminating evidences and products of organic evolution.

This evolution has not ceased; and to that degree man still remains educable. He seems to have reached the very acme of mass cruelty, confusion, conflict, and destructiveness. Therein lies a tithe of hope. It would seem that on sheer evolutional grounds of survival, man must and can shift to a higher cerebral plane of attitude and action. Among other things, he surely needs a science of behavior, a systematically prosecuted science, which will not only probe the lingering wickedness of old Adam, but which will explore with unrelenting penetration the rich repository of potentials for good, which are revealed with awesome mystery in the sequences of child development.



WEEDS, FUNGI, AND THE EDUCATION OF BOTANISTS

FREEMAN WEISS

Dr Weiss (Ph D, Cornell, 1923), who is a plant pathologist at the Bureau of Plant Industry, USDA, Beltsville, Maryland, gave the address from which his article is taken as retiring president of the Botanical Society of Washington last December 10.

THIS paper is the result partly of necessity and partly of a challenge. The necessity arises from the tradition of the Botanical Society of Washington that its president, who is effectively muzzled all year by his role of presiding officer during the Society's debates on matters botanical, is at last allowed his say at the end of his term. The challenge was perhaps unintentionally issued by one of our fellow-members and an esteemed professional colleague, L. K. Kephart, at a meeting of the Society in 1935. In the course of a review of the then recently established project on weed control of the U. S. Bureau of Plant Industry, Kephart stated that of all the agents of weed destruction or suppression that had been tested, only plant diseases had failed to be of any help whatever.

As a plant pathologist more or less nurtured in the belief that the prevention or control of plant diseases was an essential factor in our agricultural prosperity, I was amazed at the low estimate placed on their utility in combating weeds. In the meantime there has been opportunity for considerable reflection upon the subject. Granting that Kephart's conclusion was soundly based upon the available evidence in a field in which he is a specialist, the question emerges whether plant diseases are overrated (at least by pathologists) as agents of crop destruction; or are weeds endowed with special properties of disease resistance just as they have developed, through natural selection in a hostile environment, other aggressive and tenacious characters?

The possibility of embarrassment, personal or to my professional colleagues, leads me to evade answering the first part of this question, though stating my conviction that the destructive capacity of crop diseases needs no exaggeration, in a world food shortage, in order to be impressive. Perhaps another aspect of the question is whether plant pathologists have yet devised effective techniques for the mass inoculation of field-grown plants; but this is beside the present point.

There are, of course, many observations on record of damaging infections of weeds by fungus and other diseases, and there have been numerous speculations whether certain rusts and blights could be adapted to the purposeful destruction of plants. To date, however, there is little evidence that weeds, even in dense and fairly pure stands, suffer from diseases to the extent that crop plants often do; and the employment of pathogenic organisms for the extermination of weeds is still largely speculative. Indeed, in view of the rapid progress of recent years in the development of selective herbicides that destroy weeds but spare crops, the question may be of no practical consequence. The basic problem of how weeds escape or resist parasitic diseases to which crop plants, even of related species, often succumb is still vital, however. It is worthy of the serious attention of plant pathologists. Though not at all novel, this question, in both this particular aspect and also the general relations between weeds and crop diseases, has not received the attention it merits. Furthermore, the habits of weeds, as a subject essential to the liberal education of botanists, and especially of the students who will be the botanists of tomorrow, far transcend the the question of how weeds and plant diseases may interact.

The term "weed" is used here in a sense somewhat alien to the traditional concept which is, as everybody knows, "a plant out of place." In the present connection I would define a weed as a plant for which no economic use has as yet been found. It might be called an uneconomic plant. Whether in the traditional sense or the amended one proposed here, we botanists, and mayhap agriculturists as well as everyday gardeners, and also pharmacists, physicians, industrial engineers, and tycoons, have much to learn from, and about, weeds. Let us examine a few cases in point.

I

Most botanists, and also boatmen and drainage engineers who work in tropical streams, would

not question the status of the water hyacinth, *Eichhornia crassipes,* as a weed, and a most aggressive one. It is said that the progeny of a single plant can expand over some 30 acres of water surface in about 3 months. Experiments recently carried on by British technologists in India are reported to show that enough gaseous fuel, which is about 80 percent methane, can be obtained from a 30-acre crop of Eichhornia to provide cooking fuel for a thousand families for 6 months.

There are many plants, not ordinarily regarded as weeds but serving little useful purpose other than ground cover, that occupy vast areas of low-grade land useless now, and perhaps permanently, for ordinary agriculture. Examples can be found in the reeds, rushes, sedges, and grasses of tidal marshes and other areas under shallow water or subject to periodic flooding. There are also moors and uplands of great extent where the soil is too thin or the topography too rugged to permit the cultivation of ordinary crops, yet their vegetation of bracken and heather produces in the aggregate an enormous amount of plant material. Even greater in extent are the dry lands where not even the most drought-tolerant farm crops and range grasses will grow—these support a vast quantity of vegetation that has long challenged the imagination of botanists to find use for. The lush vegetation of tropical swamps and jungles still further swells the amount and variety of plant material that awaits the discovery of feasible means of utilization. If, however, water hyacinths can be economically harvested and converted into usable fuel, the prospect of similarly transforming other waste vegetation into valuable products should not appear quite hopeless.

A few years ago Ernst Berl, of the Carnegie Institute of Technology, published some rather startling claims on the feasibility of converting plant wastes into fuel oil and gasoline by a process of controlled internal combustion and hydrogenation (*Science,* 1944, 99, 309–12). It was stated that a product that is semisolid at ordinary, but liquefiable at slightly higher, temperatures could be obtained. This "protoproduct" contains about 60 percent of the original carbon content of the plant material, has a thermal efficiency of 75 percent or higher as compared with 30 percent efficiency of coal, and by ordinary cracking processes was convertible into gasoline. Its final yield of carbon was about the same as that obtained by the conversion of carbohydrate to alcohol, but being less oxidized, its fuel value was nearly twice as great, and the conversion process was cheaper and more rapid. Berl presented some interesting calculations to show the feasibility of obtaining from ordinary crops and from plant wastes an aggregate quantity of fuel equal to the current consumption of petroleum in all forms. Thus the present sugar-cane production of four countries—Cuba, Hawaii, Puerto Rico, and the Philippine Islands—amounting to 60 million metric tons a year, would suffice, if converted into gasoline by this process, to operate about one third of the motor vehicles in the United States. Other calculations indicated that even at the lower rate of sugar-cane yield of the United States—about half that obtained in Hawaii—it would be possible to produce the amount of fuel currently obtained from petroleum here from only 5 percent of the land area now in cultivation, or from 2.3 percent of our total arable land.

These estimates are of interest in connection with some recent computations by R. M. Salter of the world's potential crop production (*Science,* 1948, **105**, 533–38). With reference to sugar production (from all sugar crops), Salter estimated that the present world total of about 30 million metric tons could be increased to nearly 180 million tons by the full utilization of all areas at present in sugar-crop production, together with the additional cultivation of all adapted tropical areas. This was estimated to be six times the world requirement for sugar provided the entire population received an adequate sugar diet. There would obviously be no purpose in producing sugar crops in such excess for food, but if other uses for sugar cane could be found of the type and on the scale proposed by Berl, the world's productive capacity might be fully utilized, to the incalculable relief of apprehension (and perhaps conflict) over its limited and dwindling petroleum resources.

The impetus thus given to the replacement of petroleum by a renewable source of energy might lead to the utilization of other plant material, less exacting than sugar cane in soil and climatic requirements, and therefore producible over a much larger part of the earth's surface. Even on a hypothetical basis, a meritorious thesis might be written by an enterprising young botanist who would calculate the area, and the productive capacity of plant material convertible into liquid or gaseous fuel, of the vast canebrakes that border many of the streams in the Southern states. The giant cane, *Arundinaria gigantea,* appears to be one of the most rapid-growing and productive of all plants, perhaps equalling sugar cane and corn in its yield of vegetable matter.

II

The foregoing has presented only quantitative aspects of utilizing waste plant material. The possibilities of recovering qualitatively useful products are equally interesting. One need only recall the phenomenal discoveries of new drugs and antibiotics within the past decade to envision possible future developments of equal importance. Some of these materials are derived from plants that previously had very limited utility. For example, rutin, a drug obtained from buckwheat, has proved so valuable in the treatment of capillary fragility in man—a major cause of paralytic stroke—that it is now estimated that some 50,000 acres of buckwheat will be needed to supply the demand for this one product. Dicoumarol, obtained from fermented sweet clover, has provided for the first time an effective remedy for coronary thrombosis. The detection of plant hormones and the study of their properties and applications have been among the most productive phases of research in plant physiology, but the possibilities of further discoveries may only have been touched Plant pollens are among the richest sources of hormones. Corn pollen has thus far provided one of the best sources, but many plants are abundant pollen producers, as hay fever sufferers know only too well. Perhaps some of our pollen-rich but obnoxious weeds can be regenerated into useful members of the plant world when their hormone potentialities are fully explored.

Besides the familiar weeds of fields and gardens there are fungous weeds, or weed fungi, that are at least as numerous. Some of these have already been converted to the service of man. Penicillin is, of course, the best-known product to date of a fungus whose miraculous power was discovered only because it grew as a weed in a bacterial culture; but ever since its discovery the search has been urgent for other products of similar potency, not only among molds and other fungi, but also in the bacteria, algae, seed plants, and even insects. The usefulness of antibiotics in combating bacterial and other parasitic infections appears not to be limited to the diseases of man and his domestic animals. Plant diseases also may be amenable to antibiotic therapy. Penicillin itself was found to possess curative properties with respect to one of the notable diseases of plants, crown gall. Not only did it inhibit the growth of the gall-inciting bacterium in artificial cultures, but it destroyed infected cells in developed galls on plants while being harmless to normal tissues. Further tests of penicillin in relation to bacterial diseases of plants failed to establish any general antibiotic properties, although it was effective against a variety of plant pathogenic bacteria in artificial cultures. When injected into plants, either before or after inoculation with pathogenic bacteria, it exhibited little or no antibiotic effect, perhaps because of its rapid dilution by the plant sap (B. A. Rudolph. *Phytopath.*, 1946, **35**, 717–25). Nevertheless, the initial success with penicillin stimulated the search for other antibiotics that might prove useful in plant therapy. Within the last two years at least four antibiotic products of microbes, active against fungi but either nonbactericidal or only slightly so, have been reported in literature. Two of these, tentatively named "viridin" and "glutinosin" and derived from two common molds, were first described in England. To date their antibiotic properties appear to have been demonstrated only in artificial cultures, but these were exhibited against a variety of other fungi, including some that are pathogenic to plants. In the United States, two products, or perhaps one under two names, have been identified in cultures of certain actinomycetes, which are filamentous organisms showing affinities with both bacteria and fungi. These products have not only shown strong antagonistic effects against a variety of fungi—though not against the bacteria commonly used in antibiotic assays—when grown together in artificial cultures, but have proved effective as protective or curative sprays when applied to plants. The first of these was discovered by workers at the University of Wisconsin, and was reported under the name "antimycin" (C. Leben and G. W. Keitt. *Phytopath.*, 1948, **38**, 899–906). More recently, workers at Michigan State College have announced that a second antibiotic has been obtained from the actinomycete *Streptomyces griseus* (which is the source also of the bactericidal streptomycin) that has remarkable curative properties for certain mildew diseases of plants. This substance, to which the name "actidione" has been given, is said to have cured powdery mildew of beans and several similar diseases when applied as a spray in the very low concentration of 5 parts per million. As the growth of the disease agent in this instance is almost wholly on the surface of the host there is no diluting effect of the plant sap to combat. Although the demonstrated capacity to control a plant disease has thus far been limited to experimental plants grown under glass, this report is notable because of the curative ef-

fect against an already established disease and the low concentration at which the material is effective. At present, plant-disease control is largely aimed at prevention rather than cure, and the standard fungicides are used in much higher concentrations, which are often more or less directly injurious to the plants they are intended to protect.

III

It is not, however, with weeds and weed fungi as direct sources of useful products that I am principally concerned, but with the properties and mechanisms they exemplify in surviving an adverse environment, including man's vigorous efforts to eradicate them.

The familiar lawn weeds, such as plantain, dandelion, chickweed, pennywort, wild garlic, and many others, are seldom visibly affected by the vagaries of weather, and they flourish in the midst of infestations of grubs, nematodes, and other subterranean enemies that make the survival of lawn grasses all but impossible. These weeds even emerge unscathed from an attack of brown patch, caused by the omnivorous fungus parasite *Rhizoctonia*. Not only in neglected lawns, but sometimes in those on which all the science and art of plant culture have been lavished, weeds may be the only survivors. Although the crop plants of fields and gardens at times show a variety of symptoms of nutritive deficiencies, which have now been extensively catalogued, similar defects in weeds are practically unknown. Such adaptations to a rugged existence are not correlated with botanical or, more accurately, taxonomic relationships but appear more or less throughout the plant families. The city dweller who struggles to make grass grow in his yard but reaps a harvest only of weeds might suspect that the grass family is deficient in vegetative capacity and hardihood, but I have seen crab grass flourishing on a pile of builders' sand. The lily specialist who finds it difficult to preserve the health of these fair flowers throughout a single growing season might scoff at the alleged relationship of lilies to wild garlic, which thrives anywhere despite the most hostile intent of gardeners. Clearly the approach to a better understanding of the nature of weeds is not based on taxonomy, nor altogether related to the morphology of roots and seeds, although much botanical study has been expended upon these features of weeds.

The physiological approach has received less attention, yet weeds afford some remarkable examples of physiological efficiency. For example, wild garlic makes most of its growth in the late fall and early spring, when the temperature is low, the light weak, and the days short; it has only a few threadlike leaves, yet it produces an annual crop of bulbs that outweighs the photosynthetic apparatus, and it has phenomenal powers of survival through all kinds of seasons. It has notably few diseases, though it leads a self-centered life, characterized by persistent vegetative propagation and by inbreeding, both of which are said to have debilitating effects. In contrast to its tenacity of life, the suicidal disposition of the glamourized corn plant, which is said to be incapable even of survival in nature, is striking. Yet the corn plant, which thrives only in warm sunshine and long days, is considered a model of photosynthetic efficiency in the conversion of the energy of sunlight into starch. Although no plant can meet the engineer's standard of efficient conversion of energy, little is known of the capacity of weeds in this respect. Gardeners are merely sure that weeds grow faster than any useful plant.

A possible factor to explain the aggressive nature and survival capacity of weeds is their possession of properties inimical to the growth of other plants. This is a phase of plant physiology upon which there has been a great deal of speculation as well as experimental study. Space forbids more than bare mention of the efforts of physiologists to explain the often deleterious (though sometimes beneficial) influence of one kind of plant upon another, though the conclusion that such influences exist is hardly contestable. The earlier theory that ascribed such effects to excretions from the roots is now largely discredited but not wholly dead. In its place the view now prevails that this antagonism of one plant species for another, and even that between individuals of the same species, results from the post-mortem decomposition of roots and other plant parts—a process in which the microflora of the soil plays a dominant role and the rate of oxidation is a controlling factor. Much of the experimental work on this problem was done before the existence of plant hormones, and their capacity to influence plant growth in minimal concentrations, were fully recognized. It is perhaps for this reason that E. De Wildeman, a contemporary student of this phase of plant life (to which the name "allelopathy" or "teletoxy" has been given), has recently reaffirmed almost identically the conclusions stated by Pickering in the early 1900s. He holds that all plants, in whatever substrate they are found, excrete materials of various unknown chemical char-

acteristics that are injurious to their own health as well as to other plants. And he adds that "the teletoxic action of noxious weeds has not been appreciated" (*Acad. Roy. Belgique Bull. Cl. Sciences V,* 1946, 32, 117–26).

The interrelations between weeds and fungi are many and varied. Not only are weeds often the means by which fungi and other disease organisms are disseminated and enabled to survive or increase when their crop-plant hosts are inaccessible; they may also serve as the medium on which new strains of pathogenic organisms originate. The classical example of this is the production of new strains of the cereal stem-rust fungus on the barberry through the mingling of biotypes in sexual reproduction on the latter host. A similar relation doubtless holds in many other rusts that occur on economic plants, of which the diploid phase is initiated on some totally unrelated plant, often an obscure weed. Another type of weed-fungus relationship is exemplified in the part played by *Nicotiana repanda,* and other endemic species of the tobacco genus in the Southwest, in providing a permanent reservoir of inoculum of the downy-mildew fungus, which periodically spreads over the commercial tobacco culture of neighboring states to the east.

Even a casual survey of the fungus flora of the native and introduced species that are botanical relatives of crop plants shows a close parallel between their parasites, as might be expected. Moreover, it discloses many instances where the sexual, or perfect, stage of certain pleomorphic fungi, which are commonly found in only the conidial, or imperfect, stage on crop plants, is frequently encountered on their endemic botanical relatives. To young mycologists with a yen for connecting up the life cycles of pleomorphic fungi, this should be an intriguing challenge, for under the Rules of Nomenclature they can associate their own names with those legitimately applied to the perfect stages of such fungi, to the exclusion of all previous authors.

The importance of weeds, especially perennial ones, as accessory hosts of viruses that are the cause of so many important diseases of crop plants was demonstrated long ago by Doolittle and Wellman for cucumber mosaic, by Kunkel for aster yellows, by Valleau for tobacco and potato mosaic, and by many others A more recently recognized factor that complicates the detection of such relationships, but correspondingly enhances their importance, is the existence of symptomless carriers of viruses among weeds and endemic plants. On the other hand, weeds provide students of plant viruses with some of their most useful test plants, as they seed freely, grow quickly, and sometimes exhibit very distinctive symptoms.

The occurrence of symptomless carriers of pathogenic organisms is found not alone with relation to viruses. It has been shown that certain races of the fungus *Fusarium*—a notorious agent of wilt diseases of many important crops—can be isolated from normal-appearing weeds and some field crops but are strongly pathogenic to cotton, sweet potatoes, and tomatoes. In an earlier demonstration of a similar relationship, the bacterial pathogens of tobacco wildfire and angular leaf spot were found sometimes to be present on the roots of cereals and legumes, apparently in something more than an accidental association. The bearing of these discoveries on the selection and management of crop rotations is manifest.

All these observations add up only to the conclusion that plant life must increasingly be studied as a whole. If, in the necessary initial preoccupation with plants of historic economic importance, the study of the obscure members of the plant world has been neglected, the balance must be redressed, if for no other reason than because it is easier to find out something new about the latter. There is another reason too. It has been well stated by a colleague, Dr. F. P. McWhorter, who, though he works professionally among the finest creations of floriculture, holds the highest admiration for the drab but intensely interesting plants of the desert. "The desert," he says, "is the place where life has learned to live." I would add that perhaps so also is the weed patch, not only in gardens and fields, but the microscopic weeds in the soil, the atmosphere, and even in the debris of our laboratories.

THE EVOLUTION OF THE CONCEPT OF NUMBER

F. D. MURNAGHAN

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IT IS a matter of common observation that many well-educated, cultured people have a definite inferiority complex in regard to mathematics. Such persons will usually say frankly that they were good at, and enjoyed, arithmetic in their elementary schooling but that when they came to negative numbers and algebra they could not understand a word of what was going on, that as soon as they passed the course (by dint of memorizing formulas) they forgot as quickly as they could all about the matter. We feel that this attitude toward mathematics cannot be properly attributed to innate perversity, but that it is due to the fact that our teachers of mathematics do not take the time to explain properly the simple, fundamental ideas underlying the concept of number. This article is an outline of the way these ideas should, in our opinion, be presented, and we shall be amply rewarded if an occasional teacher of elementary arithmetic and algebra is tempted to incorporate some of our ideas into his teaching.

Stage 1

The counting number stage. The first stage in the concept of number is the counting number stage. The essential features of the system of counting numbers are (1) the existence of a leader, 1, and (2) the existence of a successor for each counting number, no two counting numbers having the same successor. Thus 2 is the successor of 1, 3 is the successor of 2, . . . , 9 is the successor of 8, and so on. The *existence* of a leader or beginner 1 and, equally, the *nonexistence* of an ender, or final number, of the counting number system are noteworthy features of the system. Addition of counting numbers is based on the successor concept. Thus $5+8$ is defined to be the successor of $5+7$; $5+7$ is defined to be the successor of $5+6$, and so on till we reach $5+1$, which is *defined* to be the successor of 5, i.e., 6. It is clear that $7+5$ is not, *by definition*, the same as $5+7$; the latter is the successor of $5+6$, whereas the former is the successor of $7+4$. Nevertheless, both sums are the same, and this is a general property of addition of counting numbers; if a and b are any two counting numbers, the sum $a+b$ is the same as the sum $b+a$. In technical language, addition of counting numbers is a commutative operation. Furthermore, addition of counting numbers is associative; i.e., $(a+b)+c=a+(b+c)$, and we denote the common sum by $a+b+c$.

The multiplication of counting numbers is defined in terms of the addition of counting numbers. Denoting "six times nine" by 9×6 (i.e., nine multiplied by six), we define 9×6 by the formula

$$9\times 6=(9\times 5)+9.$$

In words: Six times nine is, by definition, five times nine plus nine. But what is five times nine? It is, again by definition, four times nine plus nine and so on till we reach 9×1, i.e., one times nine. This we define by the statement that the leader 1 of the counting number system is *polite* in multiplication; one times nine is, by definition, nine, and, generally, the product of any counting number a by one is that counting number a. It is clear that all products of counting numbers are implicitly defined by this process, and it is equally clear that 6×9, i.e., nine times six, is not, by definition, the same as 9×6, i.e., six times nine; the latter is $(9\times 5)+9$, whereas the former is $(6\times 8)+6$. Nevertheless, both products are the same, and this is a general property of multiplication of counting numbers; if a and b are any two counting numbers, the product $a\times b$ is the same as the product $b\times a$. In technical language, multiplication of counting numbers is a commutative operation. Furthermore, multiplication of counting numbers is associative; i.e., $(a\times b)\times c=a\times(b\times c)$, and we denote the common product by $a\times b\times c$. If you have understood all this, it will be clear to you that the statement "two times two is four" is not a definition of "four," but rather the statement of a theorem which must be demonstrated, and I leave to you the fun of demonstrating it. If it bores you because it is too easy, substitute the following theorem:

To find the product of any two counting numbers between 5 and 10, add their excesses over 5 and multiply their defects under 10. The former figure is the ten-digit number, the latter, the unit-digit number of the product. For example, if the two numbers are 6 and 8, the excesses over 5 are 1 and 3, and the sum of these is 4; the defects under 10 are 4 and 2, and the product of these is 8 Hence (if the theorem is true) $6 \times 8 = 48$.

Since multiplication of counting numbers is defined in terms of the addition of counting numbers, it is to be expected that the two operations should be simply interrelated. The simplest and most useful expression of the close relationship between the two operations is given by the formula

$$(a+b) \times c = (a \times c) + (b \times c).$$

In words: Instead of first adding and then multiplying, we can first multiply and then add. We express this fact technically by saying that multiplication is distributive with respect to addition. It is the basic fact underlying all "long multiplications" and, to give an instance more suited to the days in which we live, underlying the design of all computing machines.

This is all that space permits me to say about the first, and most important, stage in the evolution of the concept of number. It is the stage which most people understand reasonably well, and it serves as the model for the successive stages. We shall only dignify with the name "numbers" the individual members of a collection of symbols if we are in possession of two rules of operation on these elements, which rules of operation (termed addition and multiplication) possess as many as possible of the simple features that distinguish the rules of addition of counting numbers; namely, associativity, commutativity, and the distributive property of multiplication with respect to addition. I make one remark before leaving our friends the counting numbers, although their leader 1 is polite in multiplication so that $a \times 1$ is *always* the same as a, he is not polite in addition; $a+1$ is *never* the same as a. In fact, there is no counting number which is *ever* polite in addition; $a+b$ is never the same as a.

Stage 2

The relative number stage. Here the thing we shall call a number is an *ordered* pair of counting numbers, and we are interested merely in a relationship of "relative magnitude" of the two counting numbers. The relationship of relative magnitude that 4 bears to 7 is not the same as the relationship of relative magnitude that 7 bears to 4, and this is what we mean by the adjective "ordered." The ordered pair (4,7) is not the same as the ordered pair (7,4). If we say that we start "at" the first of the two counting numbers that we write down and end "at" the second of the two counting numbers that we write down, it is highly important at which of the two counting numbers we start and at which we end.

In the concept of relative magnitude, however, the particular counting number at which we start has no significance; the relative magnitude (5,8) is the same as the relative magnitude (4,7) or the relative magnitude (3,6) and so on. Thus we have many symbols for the *same* relative magnitude. The test by which we recognize whether two symbols (a,b), (c,d) (where a, b, c, d are any four counting numbers) are symbols for the same relative magnitude is very simple: If they check by cross addition—i.e., if $a+d=b+c$—they represent the same relative magnitude; otherwise not Thus (1,6) and (8,13) represent the same relative magnitude because $1+13=6+8$. Before we can call these symbols for relative magnitudes numbers, we must define our rules of addition and multiplication, and these rules should be carefully framed so as to preserve the convenient features which characterize the addition and multiplication of counting numbers.

The rule of addition of relative magnitudes is very simple and may be phrased as follows: *Respect the comma.* Thus the sum of (a,b) and (c,d) is furnished by the formula

$$(a,b) + (c,d) = (a+c, b+d).$$

It is easy to show that the sum so defined is independent of a change in the symbols used to represent either (or both) of the relative magnitudes that are being added. It is also clear (simply because we have respected the comma) that addition of relative magnitudes defined in this way is both commutative and associative. Furthermore, it is clear that the relative magnitude (a,a), in which we start and end at the same counting number, is polite in addition, and that every relative magnitude (a,b) has a unique "close associate" or partner—namely, (b,a)—which is distinguished by the fact that the sum of any relative magnitude (a,b) and its partner (b,a) is the relative magnitude (c,c) which is polite in addition. We term the polite relative magnitude (a,a) ***zero*** and we term the partner (b,a) of (a,b) the ***negative*** of (a,b); the simple fact that the relationship between any relative magnitude and its negative is a partnership makes it clear that the negative of the negative of any relative magnitude is this original relative magnitude. Furthermore, the only

relative magnitude that is its own negative is the relative magnitude zero.

As far, then, as addition of relative magnitudes is concerned everything is satisfactory. The rule of addition is as simple as the rule of addition of counting numbers (amounting, as it does, to a mere repetition of this rule), and it possesses all the convenient features of the addition of counting numbers. We have indeed a gain in the fact of the existence of a polite relative magnitude—namely, zero—and the consequent existence of partnerships between pairs of relative magnitudes (each pair consisting of a relative magnitude and its negative). But we cannot call these relative magnitudes *numbers* until we frame a second rule of combination which plays the same role with respect to the addition of relative magnitudes as does multiplication of counting numbers with respect to the addition of counting numbers. If we were not able to frame such a multiplying rule of combination, we would term our relative magnitudes *vectors* (to emphasize that we start at a counting number and end at, after being *carried* to, a counting number). It is clear that the sum of $(1, a+1)$ and $(1, b+1)$ is $(1, a+b+1)$, and so we can use the symbol a of the counting number a (preceded, to avoid confusion, by the sign $+$) as a shorthand or stenographic symbol for the relative magnitude $(1, a+1)$, and then our knowledge of the addition of counting numbers furnishes us with the sum of two relative magnitudes which are such that in each of them the second counting number is greater than (i.e., follows) the first; in fact, the relation $(+a) + (+b) = +(a+b)$ is valid for the stenographic symbols. When we use this stenographic notation for relative magnitudes, we denote by $-a$ the negative $(a+1, 1)$ of $(1, a+1)$, so that our relative magnitude vectors are denoted by the symbols . . . $-3, -2, -1, 0, +1, +2$. . . . Our relative magnitude vectors have, like the counting numbers, no ender, but, unlike them, they have also no beginner. Any relative magnitude (a,b) has the predecessor $(a+1, b)$ as well as the successor $(a, b+1)$; for example, the predecessor of $+1$ is 0, the predecessor of 0 is -1, and so on. The representation of these relative magnitude vectors by equidistant points on an indefinitely extended line is familiar to everyone, and I shall not dwell on it; I content myself with the remark that we refer to this representation by the statement that relative magnitude vectors are *one-dimensional* vectors, or *vectors on a line*.

The rule of multiplication of relative magnitudes is not as natural or easy to frame as their rule of addition. Our guiding principle is that we should like, for the sake of convenience, the product of $+a$ by $+b$ to be $+ab$; in our original nonstenographic notation, then, we would like to have $(1, a+1) \times (1, b+1) = (1, ab+1)$. The following rule achieves this aim, and we adopt it as our rule of multiplication of relative magnitudes:

$$(a,b) \times (c,d) = (ad + bc, ac + bd).$$

In words: To get the first counting number of the product, cross multiply and add, and to get the second counting number of the product, multiply the corresponding counting numbers of the two given relative magnitudes and add. It is easy to verify that this rule of multiplication possesses all the features that distinguish multiplication of counting numbers; it is commutative, associative, and distributive with respect to addition. Furthermore, the product of (a,b) by $(2,1)$, i.e., -1, is the negative (b,a) of (a,b). This is the basis for the mysterious rule of signs: "Minus times plus is minus and, although plus times plus is plus, minus times minus is not minus but plus." We are now entitled to term our relative magnitudes (which have, so far, been only vectors on a line) numbers, and we refer to them as *relative numbers*. They are also known as the integers (positive, negative, and zero). It is unfortunate that this second stage of the concept of number, the relative number concept, a stage so important for all commercial and social transactions where relative magnitudes are so much more fundamental than absolute magnitudes, is so poorly understood by so many people. A person who has once grasped the concept of relative numbers will never be guilty of the absurdity of trying to count with them; he will not worry himself with such meaningless questions as: Can I have -6 things? Can I have 0 things? He cannot have -6 things, but he can be in a situation where his competitor has six more than he has; he cannot have 0 things, but he can have the same number as his competitor.

Before leaving this second stage of the concept of number I direct your attention to the Dr. Jekyll and Mr. Hyde nature of the *level* relative number (a,a), which we term zero. Zero is polite in addition, but is a murderous bandit in multiplication. The sum of any relative number and zero is that relative number, but the product of any relative number and zero is always zero. This is a natural stumbling block for the poorly instructed student of numbers. How can two zeros be the same as one zero?

Stage 3

The rational (or slope) number concept. In order to introduce the rational (or slope) number

concept let us consider an indefinitely extended line (drawn horizontally on a plane) on which the integers (positive, negative, and zero) are represented; we term such a marked line a *scale*. We now construct a series of vertical scales whose zero points are the marks on the horizontal scale (the positive integers being marked on the upper part of each of the vertical scales) and obtain in this way a checkerboard arrangement of points on the plane. We use the symbol (a,b), where a and b are now integers, i.e., relative numbers and not counting numbers, to denote the position of the integer b on the vertical scale whose zero point is the integer a on the original horizontal scale. Thus $(+1,-3)$ is the point where the integer -3 may be found on the "$+1$ vertical scale," i.e., the vertical scale whose zero point coincides with the position of the integer $+1$ on the original horizontal scale. The various lines from the point $(0,0)$ to the various points (a,b) intersect, if a is not zero, the $+1$ vertical scale in points some of which coincide with the marks on the $+1$ vertical scale. For example, the line through $(2,2)$ will intersect the $+1$ vertical scale at the mark $+1$. Terming the line through (a,b) the "line (a,b)," it is clear that the lines (a,b) and (c,d) will coincide if, and only if, the ordered pairs of integers (a,b) and (c,d) check by cross multiplication; i.e., if, and only if, $ad = bc$. We confine our attention to those lines (a,b) which intersect the $+1$ vertical scale (i.e., we consider all lines (a,b) for which $a \neq 0$), and we say that each of these lines (a,b) defines a *slope number,* which we denote by $\frac{b}{a}$ and which we mark on the $+1$ vertical scale at the point where the line (a,b) intersects the $+1$ vertical scale. Thus, the slope number $\frac{b}{1}$ is marked at the same point on the $+1$ vertical scale as is the integer b, and the slope numbers $\frac{b}{a}$ and $\frac{d}{c}$ are the same if, and only if, they check by cross multiplication; i.e., if, and only if, $bc = ad$. In other words, the lines (a,b) and (c,d) coincide if, and only if, their slope numbers are the same. These slope numbers are familiar to you as fractions, b being the numerator and a the denominator of the fraction $\frac{b}{a}$. The fraction $\frac{1}{2}$ is the slope number $\frac{1}{2}$, and $\frac{1}{2}$ is the same as $\frac{2}{4}$ simply because $4 \times 1 = 2 \times 2$. There is no fraction whose denominator is zero (simply because there is no slope number (a,b) when $a = 0$).

Before we are really entitled to call our slope numbers or fractions *numbers,* we must frame our rules of addition and of multiplication. In contrast to the situation that confronted us when we were dealing with relative numbers or integers, it is the multiplication of slope numbers that is easy and natural, whereas their addition is relatively complicated. The rule of multiplication of slope numbers may be phrased thus: *Respect the bar.* In other words, the product of $\frac{b}{a}$ by $\frac{d}{c}$ is $\frac{bd}{ac}$. Just because we respect the bar we lose none of the simple properties which characterize multiplication of integers; multiplication of slope numbers, i.e., of fractions, is commutative and associative. Whether it is distributive with respect to addition we cannot tell until we have *defined* addition of slope numbers. Our guiding principle in framing a convenient definition of addition of slope numbers is the following: If the two points (a,b) and (c,d) happen to lie on the same vertical scale (other than the zero vertical scale), i.e., if $c = a \neq 0$, we would like the sum of $\frac{b}{a}$ and $\frac{d}{c}$ to be $\frac{b+d}{a}$. This leads to the following rule of addition of slope numbers:

$$\frac{b}{a} + \frac{d}{c} = \frac{bc+ad}{ac}.$$

In words: To obtain the numerator of the sum, cross multiply and add. To obtain the denominator of the sum, multiply the two denominators. It is easy to see that addition of slope numbers, or fractions, defined in this way, is commutative and associative, and that multiplication of slope numbers is distributive with respect to it. It is also easy to see that the *average* of any two slope numbers $\frac{b}{a}$ and $\frac{d}{c}$, i.e., the product of $\left(\frac{b}{a} + \frac{d}{c}\right)$ by $\frac{1}{2}$, is represented on the $+1$ vertical scale by a mark which lies between the marks which represent $\frac{b}{a}$ and $\frac{d}{c}$. In other words, any interval, no matter how small, of our scale contains an endless number of slope numbers.

We leave this third stage in the evolution of the concept of number, the slope number or, as it is usually called, the rational number concept, with the remark that we have now sacrificed the successor property which was such an important feature of Stage 1, the counting number concept, and of Stage 2. the relative number concept. No rational number has an immediate successor or an immediate predecessor. In particular, the rational number $\frac{0}{1}$ (which we term simply the rational

number zero) has no immediate successor and no immediate predecessor. In other words, there is no smallest positive rational number and no greatest negative rational number. Despite the fact that the rational numbers lie so densely on every part of the +1 vertical scale, they can be counted so that, in a certain sense, there are no more of them than there are counting numbers. The counting is done according to the following scheme, where we show the first ten of the rational numbers:

$$\frac{0}{+1}, \frac{+1}{+1}, \frac{-1}{+1}, \frac{+1}{+2}, \frac{-1}{+2}, \frac{+2}{+1}, \frac{-2}{+1}, \frac{+1}{+3}, \frac{-1}{+3}, \frac{+3}{+1}, \ldots .$$

The plan underlying the counting is simple: we place each negative rational number $\frac{-b}{a}$, where a and b are positive integers, immediately after its partner $\frac{b}{a}$ and, after starting with zero, we arrange the positive rational numbers according to the size of the integer $a+b$. The positive rational numbers are all supposed written in their lowest terms, and, if two positive rational numbers $\frac{b}{a}$ and $\frac{b'}{a'}$ are such that $a+b=a'+b'$, we write down first that one of the two numbers which has the smaller numerator.

Stage 4

The real number concept. Despite the fact that the marks which represent rational numbers on the rational number scale lie very dense on every part of the scale, they do not suffice for even very elementary computations. Thus, there is no rational number which measures the diagonal of a square of unit side. What do we mean when we say that the length of this diagonal is $\sqrt{2} = 1.4141 \ldots$? We mean that, although there is no mark on the rational number scale which represents the length of the diagonal of a square of unit side, yet if we laid this diagonal on the rational scale with one end at zero, the other end would lie in each interval of the following sequence of intervals (1,2), (1.4, 1.5), (1.41, 1.42), (1.414, 1.415) . . . , there being, conceptually, no end to this sequence despite the fact that each of the intervals is covered by, and is only one tenth the size of, the one that immediately precedes it. We regard this *nested* sequence as defining the *irrational* (= nonrational) number $\sqrt{2}$, and it is not difficult to define addition and multiplication of irrational numbers in terms of the corresponding operation on the rational end points of the various intervals of the nested sequences which define the irrational numbers. The addition and multiplication of irrational numbers arrived at in this way possess all the simple desirable features that characterize addition and multiplication of counting numbers (Stage 1) or of relative numbers (Stage 2) or of slope numbers (Stage 3); both addition and multiplication are commutative and associative, and multiplication is distributive with respect to addition.

I shall not enter into details, for there remain two stages in the evolution of the concept of number about which I want to say something. But I must add that these irrational numbers, defined by nested sequences of rational intervals, form, together with the rational numbers, the *real numbers*, which underly the subject known as calculus, and it is only the simple truth to say that no one can have any real understanding of calculus who does not know about them. Despite this fact, there seems to be a conspiracy of silence, motivated, no doubt, by the kindly but mischievous thought that the truth is too difficult or too dangerous for beginning students; of the tens of thousands of students who are today supposed to be learning the elements of calculus in our colleges less than one tenth of one percent ever hear of the nested intervals which define the numbers without which their subject would be impossible to develop. I here justify my rapid passage over this fourth stage of the evolution of the concept of number, the real number concept, by a plug. I have written an elementary calculus (published by the Remsen Press, 26 Court St., Brooklyn 2, N. Y.) in which you can find it all explained in terms suitable for a young boy or girl. In this book I have dared to tell the truth, but I have not dared to commit the crime of making the truth dull and uninteresting.

Stage 5

The complex number concept. In introducing the concept of a complex number, we follow exactly the same principle as when we introduced relative numbers. Just as a relative number was an ordered pair of counting numbers, so a complex number is an ordered pair of real numbers. Addition of complex numbers is defined by the same principle as in the case of relative numbers: *Respect the comma.* Thus, the sum of the two complex numbers (a,b) and (c,d) is the complex number $(a+c, b+d)$, and, just because we respect the comma, it is clear that this kind of addition possesses the desirable features of the addition of real numbers; it is com-

mutative and associative. We may mark our complex numbers on a scale, but, since we have to represent for each complex number *two* real numbers, our scale is no longer a line but a plane. Each complex number (a,b) is represented by a point in a plane. Actually we have two real-number scales, one horizontal and the other vertical, on the first of which we mark the real number a and on the second of which we mark the real number b. Thus we merely do *twice* for complex numbers what we have learned to do *once* for real numbers, and this is the only complexity involved. You will have understood from the previous discussion that we are not yet entitled to call our ordered pairs of real numbers complex *numbers,* for we have not yet defined the necessary rule of multiplication. They are, so far, only vectors, but now instead of being one-dimensional vectors, or vectors on a line, as were the relative numbers, they are two-dimensional vectors, or plane vectors. Thus the complex number (a,b) may be regarded as represented by the vector which carries us from the origin $(0,0)$ of our plane scale to the point (a,b) of this plane scale. But our vectors may start at any point, and so the complex number (a,b) may equally be regarded as represented by the vector which carries us from any point (c,d) of our plane scale to the point $(c+a, d+b)$ of this plane scale. The addition of our plane vectors follows the triangle law, in accordance with which we start the second vector where the first one ended, the sum of the two vectors being, then, the vector which carries us from the initial point of the first vector to the terminal point of the second vector. The zero vector $(0,0)$ is polite in addition.

When we try to make numbers out of these plane vectors we meet clearly a situation which was already present, although not so distinctly, in the first, or counting number, stage of the evolution of our concept of number. When you learned to multiply 3 by 5 you must have vaguely felt that there was some difference in the roles played by the two numbers 3 and 5. In forming 5 times 3, 5 was a kind of machine, or operator, the "multiplying-by-5 machine," into which we fed the number 3, the machine returning immediately the desired product. In forming 3 times 5, things are quite different; we have now a different machine, the "multiplying-by-3 machine," into which we feed a different number, 5 rather than 3, and this different machine immediately returns to us the desired product (which happens to be the same as the number returned to us by the other machine). In working with plane vectors, we have to devise a multiplying machine which feeds on vectors to produce vectors, and it turns out that the machines that do this most conveniently are *not* plane vectors. In order to keep in touch with the well-established notation, we write our plane vectors which represent our complex numbers in the form $\begin{pmatrix} a \\ b \end{pmatrix}$ rather than (a,b), and then the machines which feed on these plane vectors are blocks of four numbers like $\begin{pmatrix} p & r \\ q & s \end{pmatrix}$, which we may conveniently regard as built from two *column* vectors $\begin{pmatrix} p \\ q \end{pmatrix}$ and $\begin{pmatrix} r \\ s \end{pmatrix}$ or from two *row* vectors (p,r) and (q,s). If (p,r) is any row vector and $\begin{pmatrix} a \\ b \end{pmatrix}$ any column vector, we term the real number $pa+rb$ the product of the row vector (p,r) by the column vector $\begin{pmatrix} a \\ b \end{pmatrix}$. The result of feeding the column vector $\begin{pmatrix} a \\ b \end{pmatrix}$ into the multiplying machine $\begin{pmatrix} p & r \\ q & s \end{pmatrix}$ is, then, the column vector obtained by multiplying, one after the other, the rows of the machine by the column vector which is being fed into the machine. Thus:

$$\begin{pmatrix} p & r \\ q & s \end{pmatrix}\begin{pmatrix} a \\ b \end{pmatrix} = \begin{pmatrix} pa+rb \\ qa+sb \end{pmatrix}.$$

It is now easy to define the product of one machine by another; we merely feed, one after the other, the column vectors of the second machine into the first machine. Thus:

$$\begin{pmatrix} p & r \\ q & s \end{pmatrix}\begin{pmatrix} a & c \\ b & d \end{pmatrix} = \begin{pmatrix} pa+rb & pc+rd \\ qa+sb & qc+sd \end{pmatrix}.$$

This coupling of two multiplying machines has, as is easily seen, the disagreeable feature that it is noncommutative. It is sensitive to an interchange of the roles played by the two machines. If, however, we restrict our attention to those multiplying machines whose two column vectors are such that the second is obtained by rotating the first through a right angle in the positive sense, this disagreeable feature disappears. These special multiplying machines are of the form

$$\begin{pmatrix} p & -q \\ q & p \end{pmatrix},$$

and the product $\begin{pmatrix} p & -q \\ q & p \end{pmatrix}\begin{pmatrix} a & -b \\ b & a \end{pmatrix}$ of any two of them is of the same form, being

$$\begin{pmatrix} pa-qb & -(qa+pb) \\ qa+pb & pa-qb \end{pmatrix}.$$

It is these special multiplying machines, or two-dimensional square *matrices,* as they are called, that are to be our complex numbers. The plane

vectors $\begin{pmatrix} a \\ b \end{pmatrix}$ are not complex numbers; for, although these may be conveniently added, they may not be conveniently multiplied. A typical complex number is the two-dimensional square matrix $\begin{pmatrix} a & -b \\ b & a \end{pmatrix}$, and the laws of addition and of multiplication of complex numbers are expressed by the formulas:

$$\begin{pmatrix} a & -b \\ b & a \end{pmatrix} + \begin{pmatrix} c & -d \\ d & c \end{pmatrix} = \begin{pmatrix} a+c & -(b+d) \\ b+d & a+c \end{pmatrix};$$

$$\begin{pmatrix} a & -b \\ b & a \end{pmatrix}\begin{pmatrix} c & -d \\ d & c \end{pmatrix} = \begin{pmatrix} ac-bd & -(bc+ad) \\ bc+ad & ac-bd \end{pmatrix}.$$

It is easy to see that these laws possess all the desirable features of addition and multiplication that were so convenient in each of the four preceding stages of the evolution of the number concept; addition and multiplication are each commutative and associative, and multiplication is distributive with respect to addition. Furthermore, the complex number $\begin{pmatrix} 1 & 0 \\ 0 & 1 \end{pmatrix}$ is polite in multiplication, and the complex number $\begin{pmatrix} 0 & 0 \\ 0 & 0 \end{pmatrix}$ is polite in addition. We now introduce a stenographic notation in accordance with which the special complex number $\begin{pmatrix} a & 0 \\ 0 & a \end{pmatrix}$ is denoted by the symbol a, and it is then clear that $b\begin{pmatrix} 0 & -1 \\ 1 & 0 \end{pmatrix} = \begin{pmatrix} 0 & -b \\ b & 0 \end{pmatrix}$. It follows that every complex number $\begin{pmatrix} a & -b \\ b & a \end{pmatrix}$ may be analyzed into the form $a+bi$ where i is a stenographic symbol for the complex number $\begin{pmatrix} 0 & -1 \\ 1 & 0 \end{pmatrix}$. This particular complex number i is such that its square is -1; in other words,

$$\begin{pmatrix} 0 & -1 \\ 1 & 0 \end{pmatrix}\begin{pmatrix} 0 & -1 \\ 1 & 0 \end{pmatrix} = \begin{pmatrix} -1 & 0 \\ 0 & -1 \end{pmatrix}.$$

This is all the mystery that lies in the imaginary unit whose square is -1. If any plane vector $\begin{pmatrix} a \\ b \end{pmatrix}$ is fed into the complex number (or multiplying machine) i, it is returned unchanged in length but rotated through a right angle in the positive sense. The machine, then, which is obtained by squaring the complex number i (i.e., by coupling i with itself) rotates every vector that is fed into it through two right angles. This is the geometrical interpretation of the statement that $i^2 = -1$. Before leaving this fifth stage of the evolution of the concept of number, I point out that associated with any complex number $\begin{pmatrix} a & -b \\ b & a \end{pmatrix} = a+bi$ is another complex number $\begin{pmatrix} a & b \\ -b & a \end{pmatrix} = a-bi$, which we term the *conjugate* of the original complex number; the product of the two conjugate complex numbers $a+bi$ and $a-bi$ is the complex number $\begin{pmatrix} a^2+b^2 & 0 \\ 0 & a^2+b^2 \end{pmatrix} = a^2+b^2$. If we use the single symbol β to denote the complex number $a+bi$ then we denote by $\bar{\beta}$ the conjugate number $a-bi$ to $\beta = a+bi$.

Stage 6

The quaternion number concept. The concept of quaternions is developed from the concept of complex numbers in exactly the same way as the concept of complex numbers was developed from the concept of real numbers. Our new numbers are still two-dimensional square matrices, but now the coordinates of the two column vectors are complex numbers. The second column vector is determined, once the first column vector is given, by the following rule: If the first column vector is $\begin{pmatrix} \beta \\ \delta \end{pmatrix}$, then the second column vector is $\begin{pmatrix} -\bar{\delta} \\ \bar{\beta} \end{pmatrix}$. Thus a typical quaternion is of the form $\begin{pmatrix} \beta & -\bar{\delta} \\ \delta & \bar{\beta} \end{pmatrix}$, where β and δ are any two complex numbers, just as a typical complex number was of the form $\begin{pmatrix} a & -b \\ b & a \end{pmatrix}$ where a and b are any two real numbers. We term, after Hamilton, these new numbers *quaternions*, because four real numbers are necessary to specify any one of them; namely, the two real numbers a and b which serve to define $\beta = a+bi$, and the two real numbers c and d which serve to define $\delta = c+di$. If we define the squared magnitude of any complex two-dimensional vector $\begin{pmatrix} \beta \\ \delta \end{pmatrix}$ to be $\bar{\beta}\beta + \bar{\delta}\delta$, we may say that the two complex column vectors of the typical quaternion $\begin{pmatrix} \beta & -\bar{\delta} \\ \delta & \bar{\beta} \end{pmatrix}$ are equally long and mutually perpendicular, just as the two real column vectors of the typical complex number $\begin{pmatrix} a & -b \\ b & a \end{pmatrix}$ are equally long and mutually perpendicular. The rules of addition and of multiplication of quaternions are given by the formulas:

$$\begin{pmatrix} \beta & -\bar{\delta} \\ \delta & \bar{\beta} \end{pmatrix} + \begin{pmatrix} \lambda & -\bar{\mu} \\ \mu & \bar{\lambda} \end{pmatrix} = \begin{pmatrix} \beta+\lambda & -\overline{(\delta+\mu)} \\ \delta+\mu & \overline{(\beta+\lambda)} \end{pmatrix};$$

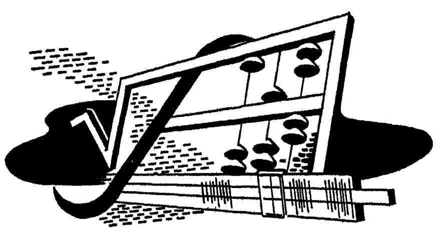
$$\begin{pmatrix} \beta & -\bar{\delta} \\ \delta & \bar{\beta} \end{pmatrix}\begin{pmatrix} \lambda & -\bar{\mu} \\ \mu & \bar{\lambda} \end{pmatrix} = \begin{pmatrix} \beta\lambda-\bar{\delta}\mu & -\beta\bar{\mu}-\bar{\delta}\bar{\lambda} \\ \delta\lambda+\bar{\beta}\mu & \bar{\beta}\bar{\lambda}-\delta\bar{\mu} \end{pmatrix},$$

and it follows that addition is commutative and associative and that multiplication is associative and distributive with respect to addition. We have had, however, to sacrifice in this final stage of the evolution of the concept of number the commutative property of multiplication; multiplication of quaternions is not in general commutative. It is easy to see that if we use the stenographic symbols 1, i, j, k, for the quaternions $\begin{pmatrix}1 & 0\\ 0 & 1\end{pmatrix}$, $\begin{pmatrix}0 & -1\\ 1 & 0\end{pmatrix}$, $\begin{pmatrix}i & 0\\ 0 & -i\end{pmatrix}$, $\begin{pmatrix}0 & i\\ i & 0\end{pmatrix}$, the general quaternion $\begin{pmatrix}\beta & -\bar{\delta}\\ \delta & \bar{\beta}\end{pmatrix}$, where $\beta = a + ib$, $\delta = c + id$ may be analyzed into the form $a + ci + bj + dk$. The quaternion units i, j, k are easily seen to satisfy the relations

$$i^2 = j^2 = k^2 = -1\,;\ jk = -kj = i\,;$$
$$ki = -ik = j\,;\ ij = -ji = k$$

The four matrices $\begin{pmatrix}1 & 0\\ 0 & 1\end{pmatrix}$, $\begin{pmatrix}0 & -1\\ 1 & 0\end{pmatrix}$, $\begin{pmatrix}i & 0\\ 0 & -i\end{pmatrix}$, $\begin{pmatrix}0 & i\\ i & 0\end{pmatrix}$ were used to great effect by Pauli, some twenty-five years ago, in connection with the quantum theory of the spinning electron and are frequently referred to as the Pauli matrices.

This ends, then, my survey of the six stages in the evolution of the concept of number, and I can see the obvious query framing itself. Why, other than for considerations of time and mercy, stop there? What about the seventh, eighth, and so on stages? Well, there is a remarkable theorem, proved some sixty years ago, that this is all the stages there are if you refuse to sacrifice the associativity and commutativity properties of addition, the associativity and distributive property with respect to addition of multiplication, and also demand that there are no divisors of zero; i.e., that if the product of two numbers is zero, one or the other of them must be zero. The only number systems that possess these properties are (1) the counting numbers, (2) the integers, (3) the rational numbers, (4) the real numbers, (5) the complex numbers, and (6) the quaternions. In my discussion I have made it clear how important for the basic fundamental concepts of mathematics are, in my opinion, the concepts of vectors and matrices. If any of you happen to teach mathematics, please break your conspiracy of silence and tell your beginning students about these simple, interesting things. You do not have to go deeper into the subject than two-dimensional, or at most three-dimensional, matrices. After all, the application by Pauli of his two-dimensional matrices to the theory of electron spin was a valuable scientific contribution which gained him a lot of fame. The fact that these Pauli matrices are merely a representation (in the field of complex numbers) of the quaternions discovered more than a century ago by the great Irishman Hamilton is for me a matter of national pride.



CRYSTAL RESEARCH

PAUL H. EGLI

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CRYSTAL research has become fashionable along with the development of the "solid state" as an important branch of science, and in many instances the two fields are synonymous. Early crystal work was necessarily confined to the external features and was conducted largely by mineralogists and a small school of theoretical scientists in the days when the gap between chemistry and physics was first being bridged. With the exciting discovery of X-ray diffraction, real progress on exploring the interior of solids became possible, and an increasing number of the most able physicists became interested in atomic and classical problems in crystal physics. The question of how crystals were formed, however, continued to receive little attention.

Within recent years a widespread recognition of the importance of crystal formation has developed in many fields of science. In metallurgy, for example, it is recognized that many of the problems are concerned with the crystalline texture of the material, and crystal formation is receiving the attention of outstanding physical metallurgists. Similarly, contributions are being made from the viewpoint of problems in ceramics, explosives, luminescent materials, paints, and in many seemingly unrelated fields.

The most active interest has arisen in connection with the development of a variety of applications for single crystals as such, and it is this work with which this discussion is primarily concerned. Most of the applications are connected in a broad sense with optics and communications. The general principles of the use of crystals as windows, lenses, prisms, and filters in optical instruments are well known, but some of the electronic poss'bilities have been less widely described. Telephone and telegraph circuits use crystals in their transmission networks to separate the various messages carried simultaneously on the same cable. Radio and radar use crystals in microphones, in earphones, to control the wave length of the transmission, and for circuit elements to replace vacuum tubes in various ways. Sonar gear uses crystals as both the generator and detector of sound in underwater echo-ranging. Crystals are being used as the source of ultrasonic energy for effecting chemical reactions and physical changes in materials. Several methods of detecting and measuring nuclear radiation are based on various properties of crystals.

The growth of single crystals is achieved by a variety of techniques, but often a particular growth method goes hand in hand with a particular application. For example, crystals for high-frequency oscillator control must have chemical stability, mechanical rigidity, and sufficient asymmetry in structure to permit a moderately strong piezoelectric effect. These properties are associated with materials which have high Debye temperatures and which are generally too insoluble for normal growth from solution. The complex structures lead to instability at the melting temperature or to symmetry transitions during cooling, so that melt growth techniques are eliminated. Thus, crystals of possible use as high-frequency oscillators must almost invariably be grown by hydrothermal (high temperature-pressure solution) techniques. Optical applications normally require physical properties such that the crystals can only be grown by a melt process; and other applications are similarly linked with crystals grown in a particular way.

The familiar type of crystal growth from solution is useful in industry only for the materials used in airborne and underwater sound devices, but the widespread use of phonograph pickups, and the large number of crystals used by the Navy in antisubmarine warfare, have been sufficient to support a sizable industry. Moreover, crystal growth from solution lends itself readily to changing the process variables, so that it is a convenient method by which to study the factors that control the growth mechanism and has accordingly received the most attention from crystal chemists.

Certain general principles, helpful in attacking the synthesis of crystals of a new material, have been established for crystal growth. For the most part, these growth principles have been established by observation, and the theoretical explanations are only partially satisfactory. The thermodynamic conditons necessary for growth were stated in an excellent manner by Gibbs in 1865, but progress

since then toward a usable kinetics for the nucleation processes has been trivial. Crystallization is usually considered in terms of two factors: the diffusion of material from the surrounding supercooled medium to the crystal surface, and the chemical reaction of addition of new material to the surface. This is expressed most simply as a typical rate equation

$$N = C\, e^{\frac{-Q}{KT}}\, e^{\frac{-W}{KT}} \qquad \text{(Equation 1)}$$

where N = number of units added per unit time. The first factor, following the constant C, governs the transport of material to the crystal (Q = the activation energy of the diffusion process), and the second factor governs the reaction at the crystal surface (W = the activation energy required to add a unit to the crystal). In aqueous solution, Q is relatively small and remarkably constant regardless of the salt. Moreover, the role of diffusion in the process can be minimized by proper agitation, so that this step is seldom the controlling factor in solution growth.

The surface reaction, which is thus normally the controlling factor, remains too complex to calculate. A qualitative conception of the process can be visualized by use of a diagram first used by Kossel (Fig. 1). Local energy fluctuations constantly deposit and dissolve material at the surface. New units are more likely to become attached at those places on the surface where they will be most tightly bound to existing crystal surface. The highest activation energy is required to add a new unit at a corner to form a new layer, whereas an appreciably small fluctuation can add new material to a partially completed row. Growth is thus pictured as proceeding in a rhythmic fashion, pausing for a

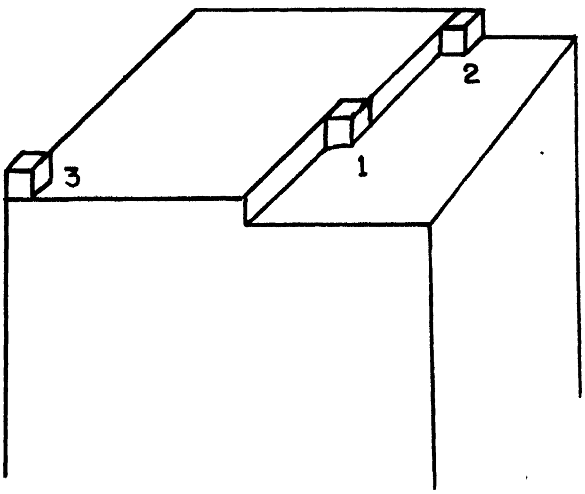


Fig. 1. Diagram illustrating sequence in which new material adds to a crystal surface in an ideal process.

large fluctuation to add material at a corner to start a new layer, proceeding more readily along the edges, and quickly filling in row after row to finish the layer.

Numerous elaborate relations have been developed to describe this process, but the necessary physical constants are not available to make them generally useful. A simple statement of a widely accepted viewpoint was presented by Landau as:

$$\Delta F = 2\beta\sqrt{\eta} + (\mu' - \mu)\,\eta \qquad \text{(Equation 2)}$$

ΔF is the free energy change for adding η ions to a surface
β is a constant for the "edge" energy per particle
η is the number of ions
μ' is the chemical potential per particle in a crystal
μ is the chemical potential per particle in a solution

This relation refers to the process of adding a two-dimensional group to the crystal surface.

An expression can be derived from this equation which states that a certain critical number of ions must be collected before a group on the surface is thermodynamically stable. Thus by differentiating and equating to zero,

$$\eta_0 = \frac{\beta^2}{(\mu - \mu')^2}. \qquad \text{(Equation 3)}$$

where η_0 = the critical size of a stable two-dimensional group. The activation energy W in equation (1) is then synonymous with the free-energy change necessary to form a group larger than the critical size η_0 in equation (3). In terms of the Kossel diagram, the critical size necessary for stability would be smaller for a group added at point (1) than at point (3).

The same arguments used for the growth process can be applied to the problem of nucleation, where similarly energy fluctuations must form an aggregate greater than the critical size of a stable nucleus. Recently considerable publicity has been given to the concept of an intermediate size aggregate (called an "embryo" by Frenkel) which by statistical theory has a finite existence even though it is too small to be thermodynamically stable. A bucket of water, for example, no matter how hot, is conceived as containing several hundred particles of ice. This concept makes it easier to explain how the larger-sized groups necessary for a stable nucleus can be formed. Otherwise, to form a nucleus would require large fluctuations which bring together the whole group more or less simultaneously; this is implausible from the viewpoint of statistics. The question is far from settled, however, as witness the fact that at a recent symposium on crystallization, the best-qualified guesses as to the number of ions or atoms in a nucleus at its critical size varied from 1 to 23,000.

FIG. 2 *Right*: Typical result of an attempt to grow NaCl from pure solution; *left*: result obtained by addition of small amount of lead ion to the solution.

Despite the lack of rigor of the theories of crystallization, the qualitative concepts they imply are helpful in guiding experiment. For example, one of the critical features in growth of large single crystals from solution is preparation of the solution so as to inhibit the formation of unwanted spontaneous nuclei. This involves certain obvious things such as very careful filtering to remove dust and similar small foreign particles, because the high surface energy of such material furnishes preferred spots for nuclei to develop. Less widely recognized is the necessity for vigorously heating the solution for an appreciable period beyond the time when the solute is apparently well dissolved. The long time required to dissolve the last traces of submicroscopic particles that could serve as nuclei is somewhat surprising in view of the well-known fact that small particles become increasingly soluble as size diminishes. This supersolubility effect is explained by the same considerations used to show that nuclei are not stable below a certain critical size, but the rate of the solution process is a more complicated matter. A plausible explanation of the persistence of nuclei in a superheated solution depends on the fact that solution takes place more readily at certain spots on a crystal surface—usually in the reverse order of the ease of growth—and the process becomes slower as the shape of the crystal changes with the loss of the more easily dissolved portions.

As implied from the theory, the two controllable factors which govern the rate of the growth process are the degree of supersaturation and the agitation. Supersaturation, which forces material to precipitate out of solution onto the seed, can be achieved in a variety of ways. On a laboratory scale, evaporation is generally the easiest method to control. On a larger scale, gradual temperature change—lowering for normal salts—is more convenient. In some cases there are advantages to using a system in which the solution is kept in equilibrium with the solute in a separate container, at a temperature slightly supersaturated with respect to the temperature of the growing chamber. By whatever means supersaturation is obtained, the success of the growth process depends on precise control of this factor. The problem is to maintain sufficient supersaturation to force a rapid growth rate without inducing spontaneous nuclei and without depositing material so rapidly that it is misorientated, thus causing flaws. Growth is slow at best; two inches a month is considered good even for crystals that grow with relative ease. The only way to make the process sound fast is to translate this to atomic terms, in which case it is 5,000 atomic planes per minute.

The permissible growth rate depends partly on the composition of the solution, and this can be varied to some extent. The optimum purity of the solution is an important factor in all growth processes, and nothing in the existing theories is very helpful. It is not true—as generally stated—that crystals grow better from very pure solutions. On the contrary, it is virtually impossible to grow large crystals of such a common thing as table salt from solution without the intentional addition of impurities. Figure 2 shows a typical result of an attempt to grow NaCl from pure solution and the result obtained by the addition of a small amount of lead ion to the solution. Of the several hundred different crystals investigated at the Naval Research Laboratory, growth of nearly all was improved by the addition of selected impurities. The type and amount of ion that was most effective proved different in nearly every case, and only the roughest sort of generalizations have been developed to aid in the selection of the proper composition for a new growth problem. Heavy multivalent cations are frequently the most useful additions, generally in concentrations below one part per hundred by weight in the solution. Larger concentrations result in growth that is worse than

from pure solution and frequently modify the habit of the number and relative size of external faces

Habit modification has been the subject of intensive investigation from the time of Robert Boyle in 1690, but it remains about as mysterious as it was then. Much of this work was concerned with the action of dyestuffs, and many colorful effects have been achieved by virtue of the fact that the dyes were absorbed on certain faces and not on others in the same crystal. The information thus acquired has been helpful in interpreting growth rate and face development in some types of compounds, but the general pattern remains vague.

Absorption of the foreign materials into the crystal is not a necessary part of the habit-modifying process, and in the case of the ions used to improve growth, as described above, the amount retained in the finished crystal is often undetectable by common analytical techniques. Virtually

FIG. 3. Completed NaCl crystals, showing manner in which they are mounted for providing agitation.

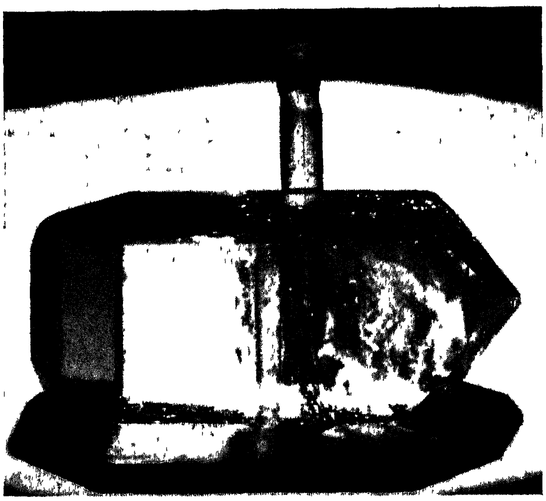


FIG. 4 HIO_3 crystal. Showing flaws induced by growth enclosing the supporting rod.

undetectable amounts of impurities in the crystal may, however, alter the physical properties appreciably. As little as six parts per million of certain contaminants increases the conductivity of ammonium phosphate by a factor of ten. For this reason, it is generally desirable to do the best possible job of removing all impurities from a solution intended for crystal growth and, subsequently, to add the desirable foreign ions.

The other major factor in solution crystal growth is the agitation which removes depleted solution from contact with the crystal surface—decreasing the path of diffusion—and prevents convection currents induced by the heat of crystallization from causing misoriented growth. Slight differences in the amount of agitation often change the growth rate by a factor of two or three, so that the efficiency of the whole process may depend on this variable. In many solutions, apparently depending on the viscosity, a point is reached beyond which additional agitation is of little value, but no ill effects have been observed from increasing agitation up to the point of visible turbulence, which may induce spontaneous nuclei.

A variety of ingenious schemes, many of them patented, have been described for achieving the necessary agitation, but none is a cure-all. The most effective technique in many cases is to mount the growing crystals on a rack, which is rotated in the solution (Fig. 3). In this way the rate is easily controlled and seeds can be mounted so that the rapidly growing faces receive the maximum benefit. An advantage is obtained in some instances by reversing the direction of rotation periodically. The scheme is not universally useful, however, because many crystals crack as growth encloses the supporting rod, regardless of the diameter or material of construction (Fig. 4). In other cases the crystals are sensitive to the tiny vibrations of the stirring motor, or the shape of the product desired may not be convenient for this system. In these instances schemes can usually be employed which involve variations of stirring the solution or rocking the container so that the solution swirls past rigidly mounted seeds.

Another important consideration in growth from solution is selection of the seed on which new growth can deposit. In general, any fragment of previously grown or natural crystal of the desired material can be used, or if none is available, small crystals can be obtained by very slow evaporation of solution in a beaker completely free of agitation. For high efficiency, however, it is necessary to use sections cut in certain crystallographic directions. Normally, the best seed is a section parallel to one of the smaller faces of a completely formed crystal. Crystals grow at different rates in different directions because new material is more readily attached to certain atomic planes; Figure 5 shows how this results in small faces normal to the rapidly growing directions. A seed plate cut in one of the slower growing directions eventually develops the normal faces, but the process is time-consuming. Growth along the smaller faces frequently has sufficient advantage over other directions that those faces project ahead of the main body of a seed until the new faces are completely formed, enclosing a hollow section. This is strikingly demonstrated on ammonium phosphate seeds cut normal to the long direction in the crystal (Fig. 6).

From an engineering viewpoint, the exploitation of crystals grown from solution has been highly successful. Much of the credit belongs to The Brush Development Company, which pioneered in

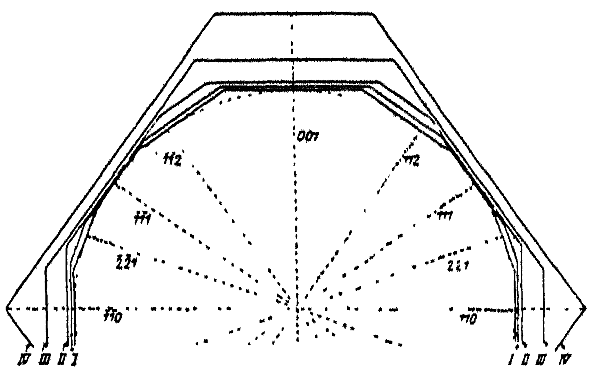


FIG. 5. Diagram illustrating how faces normal to rapidly growing directions diminish in successive stages of growth on a sphere.

FIG 6. Successive stages in capping process in the growth of ammonium dihydrogen phosphate. The seed is a plate normal to the long direction Growth occurs only at the perimeter (center of plate can be cut out without effect), and the enclosed pyramids are hollow.

the production of Rochelle salt for use in both audio devices and underwater sound gear. Rochelle salt units still have no serious competition for high-fidelity reproduction equipment, and high-quality units have long been available at a reasonable price for use in home phonograph pickups. Rochelle salt also made possible the first major improvements in underwater sound gear after the original development of quartz transducers by Langevin in France during the first world war.

More recently, the piezoelectric properties of literally hundreds of crystals have been investigated and several new materials developed for special applications. The value of a crystal as an electroacoustic transducer depends on the amount of piezoelectric response for a particular mode of vibration. Of the thirty-two classes of symmetry, twenty exhibit a piezoelectric effect, and each of these classes is limited by symmetry to electrical response from elastic deformations in certain definite directions. For underwater sound gear a longitudinal response is most convenient, and Rochelle salt remains the most sensitive for this purpose. Early in World War II, a new crystal, ammonium dihydrogen phosphate, was developed —again largely by the Brush Company—which is considerably more rugged, both chemically and electrically, and has sufficient piezoelectric response to be usable. Within eighteen months after the first measurements on tiny crystals, large-scale production of beautiful material was in progress (Fig. 7), and all sound gear constructed during the last part of the war employed the new crystals.

More recently, a new crystal, lithium sulfate monohydrate, has been developed—independently by NRL and the Brush Company—for its strong response to hydrostatic pressure. Its piezoelectric sensitivity is about seven times that of tourmaline, which had previously been employed for absolute sound-intensity measurements and similar applications for which a hydrostatic response is necessary.

For oscillator control, the essential requirement is a combination of piezoelectric and elastic properties which result in a constant resonant frequency over a wide range of temperature. Several new materials, potassium pentaborate, dipotassium tartrate (similar to Rochelle salt), and ethylene diamine tartrate, have been developed which are valuable for low-frequency applications such as telephone circuits. For Swiss radio networks, which operate at lower frequencies than those in America, the Federal Technical School at Zurich has developed potassium dihydrogen phosphate.

Quartz remains the only crystal with a high frequency, shear piezoelectric response, and a small frequency change with temperature suitable for oscillator control of American radio and television frequencies. Intensive efforts are in progress

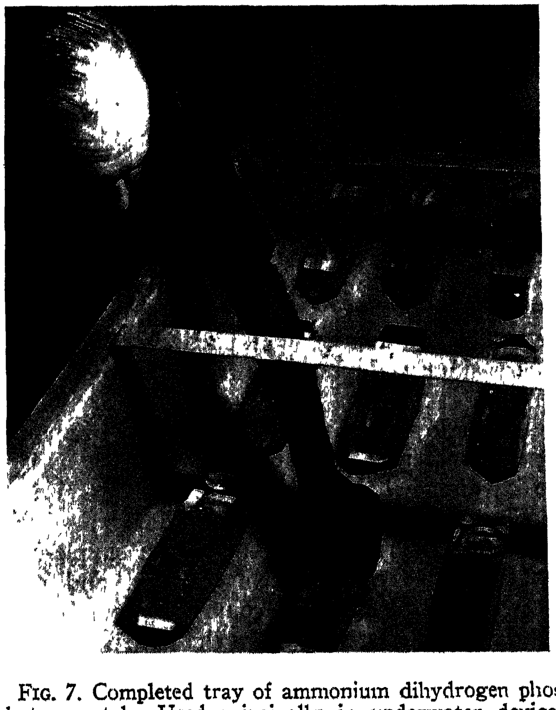


FIG. 7. Completed tray of ammonium dihydrogen phosphate crystals. Used principally in underwater devices

FIG. 8. Furnace and high-pressure vessel used for hydro thermal synthesis of quartz.

to synthesize quartz or a suitable replacement, but in every case the materials require hydrothermal synthesis. The growth problems are quite similar to normal crystal growth from solution except that high temperatures and pressures are required to dissolve the material. Most of the work in this field has been concerned with synthetic quartz, and growth on a small scale is already highly successful. An efficient large-scale process awaits the solving of some engineering problems, none of which appear to be major obstacles.

None of the possible substitute materials is likely to be developed very quickly because the scarcity of data on high temperature-pressure systems necessitates an elaborate phase-equilibrium study for each crystal before growth experiments can be efficiently conducted. Moreover, the difficulties of hydrothermal techniques make for slow progress because of the heavy equipment (Fig. 8) necessary for the high temperatures and pressures; so that even the synthesis of small particles of the correct composition is far removed from successful crystal growth.

Natural quartz and calcite have been important optical materials, but they can now be replaced to advantage by a number of synthetic crystals. Sodium chloride was the first widely used synthetic and is typical in that it is a simple-cubic halide, stable at high temperatures, and easily grown from the melt. The common process—developed on an industrial scale at the Harshaw Chemical Company—involves a crucible with a conical bottom lowered slowly through a furnace with a sharp temperature gradient at the melting point of the crystal. A single crystal forms in the bottom of the cone as the melted material reaches the gradient section of the furnace, and the remainder of the material deposits on that seed as it in turn solidifies. Figure 9 shows an elevator mechanism and a complete boule being removed from the furnace. An alternate method involves getting a nucleus of material solidified on the tip of a cool rod and slowly withdrawing it from a crucible of melted material.

A considerable variety of optical crystals is now available, each useful for a particular spectral range. Recent work has been concerned with thallium halides, particularly a solid solution of thallium bromide-iodide, which was developed by the Germans during the recent war for transmission in the far infrared (5-$40\,\mu$). Silver chloride remains a valuable material for infrared optics because of the great ease with which it is grown, and the fact that it can be easily rolled into thin sheets or pressed to desired shapes without losing its crystalline form.

Crystals grown from the melt are also in considerable demand by solid-state physicists since the simple-cubic structures readily grown by this method are amenable to investigation. A wide

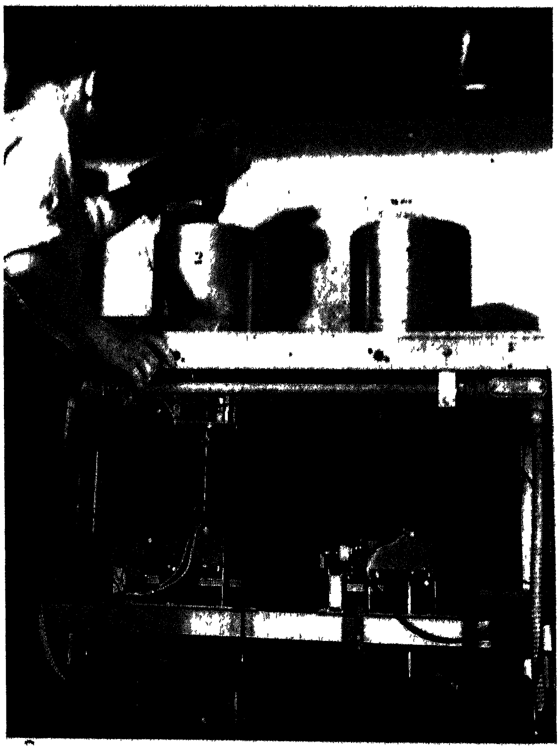


FIG. 9. Electric furnace and elevator mechanism for the crucible-melt process. The completed crystal is KRS-5 (thallium bromide-iodide) in a glass crucible.

variety of pure compounds, solid solutions, and crystals with controlled lattice defects and impurities are all being investgated with respect to fundamental optical, electrical, mechanical, atomic, and nuclear phenomena.

The melt technique has also been used for certain types of semiconductor materials, including silicon and germanium, which are widely used as high-frequency rectifiers. The very recent development of crystal amplifiers by the Bell Laboratories has created new interest in this type of material. In a demonstration test, all the tubes in a household radio were successfully replaced by tiny crystals.

By extending the temperature range of electric furnaces, several minerals that are not available as pure crystals from nature have been grown. Of particular interest are scheelite and a series of similar tungstate compounds which emit luminescent scintillations in a manner that provides a sensitive measurement of nuclear radiations.

The tungstates have also been grown by an alternate technique for high-temperature materials, the flame fusion process. This technique was developed by Verneuil and improved to an efficient industrial process by the Linde Air Products Company. A finely powdered reagent is fed evenly into the oxygen stream of an oxyhydrogen burner of special design. The material is fused in the flame and crystallizes on a ceramic support surrounded by an insulating jacket (Fig. 10). The ceramic rod is lowered as the material deposits to form a boule. Considerable witchery is involved in this technique, and for a new material the success is likely to depend on the skill of the operator. Production has been limited to corundum (Al_2O_3) and spinel ($MgO\ Al_2O_3$), with small concentrations of additional metal ions to produce a variety of colors, e.g., for synthetic rubies. Important applications have been developed for these crystals as instrument and watch bearings and for certain other applications where hardness is important, such as thread guides, but the largest market has been as synthetic gems. The reluctance of the jewelry trade to accept synthetic stones as high-grade gems has received two serious setbacks recently. One was the development of star sapphires by the Linde Company. These gems are high-quality sapphire with unusually well-defined stars, and are being well received as expensive stones. The other de-

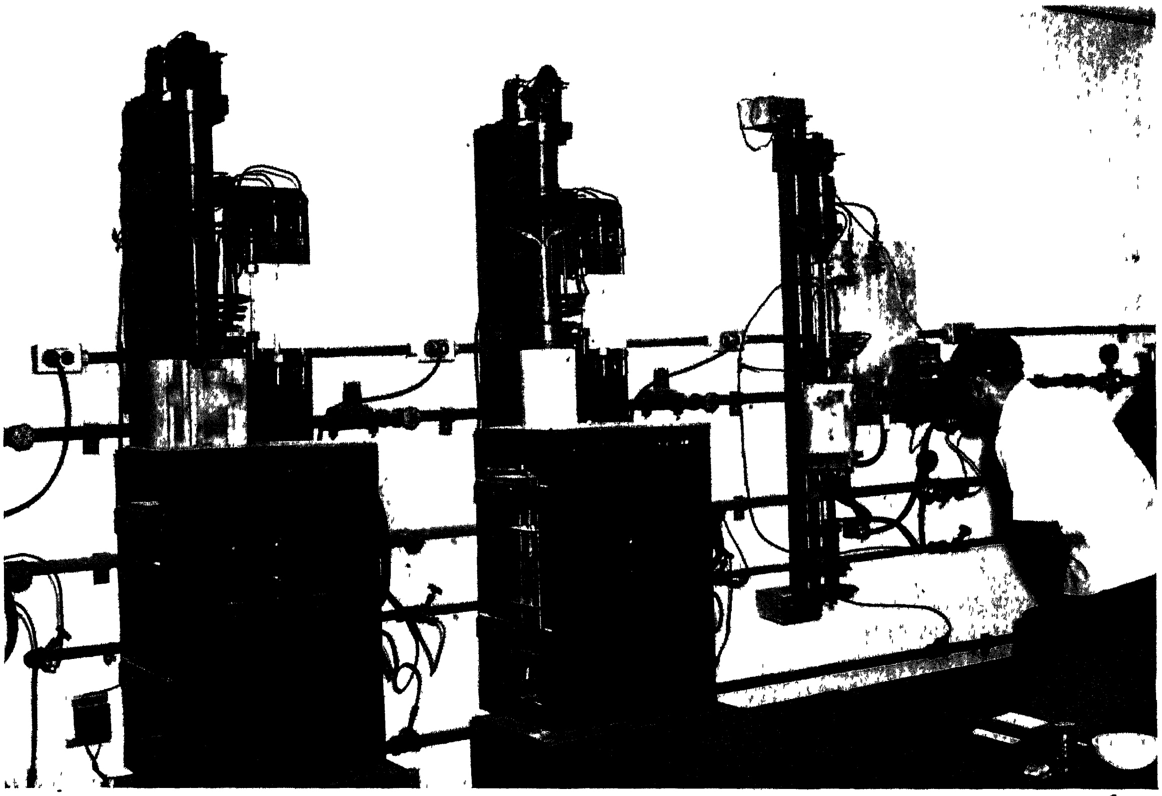


FIG. 10. Equipment for growing crystals by the flame fusion method. (Photo by courtesy of the Titanium Division, National Lead Co.)

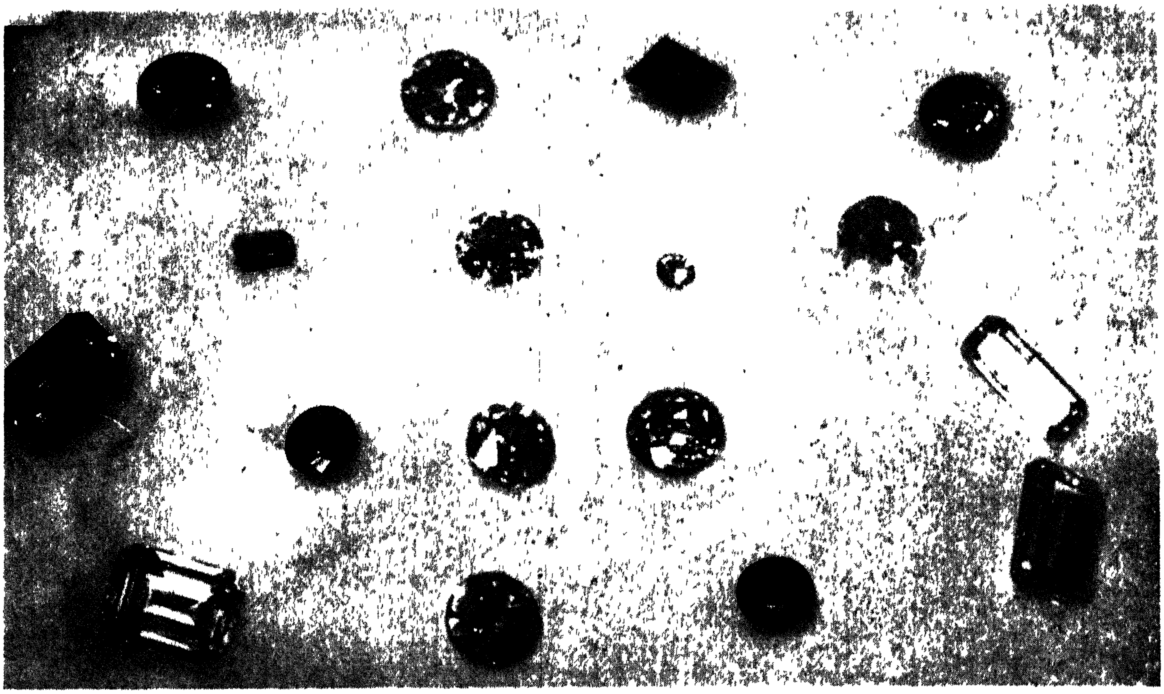


FIG. 11. Display of cut rutile gem stones. (Photo by courtesy of the Titanium Division, National Lead Co.)

velopment, even more recent, was the flame fusion synthesis of rutile (TiO_2) by Dr. Charles H. Moore, Jr., of the Titanium Division of the National Lead Company. This material, which at present is produced only as a faint yellow stone, has an appreciably higher dispersion than diamond, and when faceted in the same manner is an exceptionally brilliant gem (Fig. 11).

Rutile is also interesting for its unique electrical properties. By slight variations in the oxygen content, the material can be changed from a superdielectric to almost a metallic conductor, so that it promises to be a profitable material on which to investigate basic electrical phenomena.

A great many compounds remain for which no suitable method of crystal growth has been devised. This is particularly true for a large body of compounds that are unstable at their melting points and highly insoluble even at high temperatures and pressures. Among such compounds are many oxides, sulfides, and other typically semiconductor materials which are needed for investigation of photoconductivity, luminescence, and similar electronic processes. Improved crystals are needed in vital applications that depend on these phenomena. Some slight success has been achieved by growth from the vapor phase—such as the work on CdS by Frerichs; all such attempts, however, have resulted in thin plates or needles, and the vapor phase appears to be inherently unsuited for efficient growth of large crystals regardless of the technique.

One growth possibility that has not been exploited is a melt process under high-pressure controlled atmospheres, and plans are now in progress for such attempts. Another possibility is the combustion of a neutral gas to avoid the problem of chemical reduction in the oxyhydrogen flame fusion process. Whatever the techniques developed, future crystal research would appear to require considerable ingenuity, but the incentive is sufficiently great to insure that the necessary methods will be developed. Progress in many fields of science depends on obtaining single crystals on which to base future research.

SCIENCE ON THE MARCH

CANCER CHEMOTHERAPY

UNTIL recently the only effective measures for treating cancer involved the use of radium or X-rays or surgery. Sometimes surgery and irradiation were combined. Other therapeutic efforts consisted essentially of supportive treatment such as analgesics for the relief of pain, vitamins and proteins to counteract the loss of appetite and weight, and the treatment of anemia When cancer is diagnosed and the patient treated early the majority of sufferers can be saved from dying of the effects of a malignant growth. For example, 75 percent of early cases of cancer of the breast are curable; over 90 percent of early cases of cancer of the skin can be saved; and over 80 percent of early cases of cancer of the uterus are curable. Cancer of the lung and of the stomach, intestine, and elsewhere are also responsive.

Although such encouraging facts have changed completely the outlook for the patient with a malignant growth, often he does not seek treatment when he can be cured of his disease Sometimes surgery may prove fatal. And, occasionally, the tumor is not accessible to surgery or irradiation. In these cases other treatments would be of inestimable value.

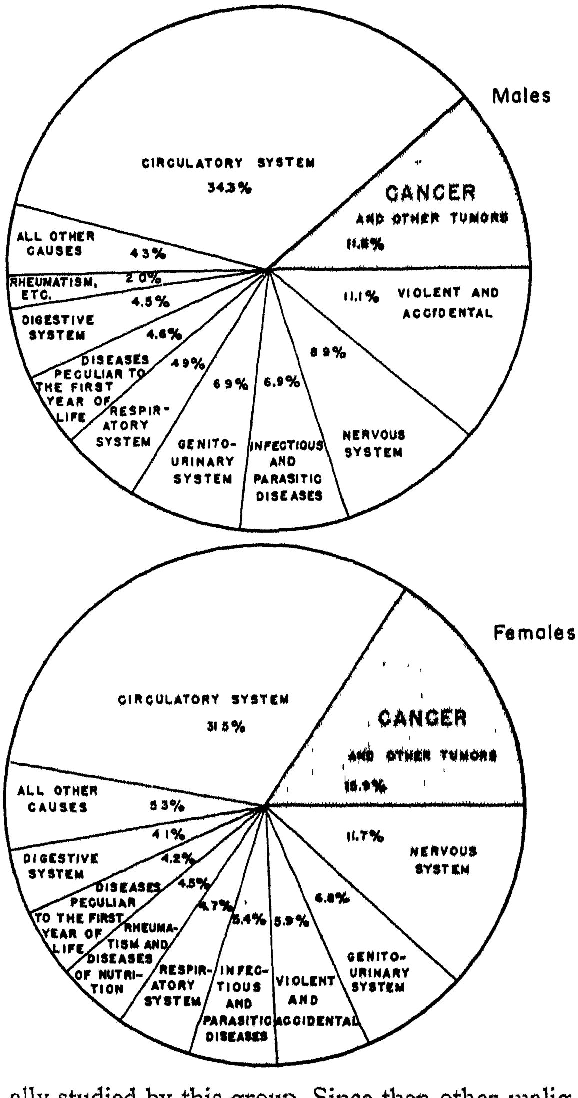
In a promising vein, discoveries have been made within the past few years that offer hope of obtaining further control over cancer. Several avenues have been followed, but the most interesting approach at the moment involves chemical investigations. In fact, so much of value has been studied that it has become almost impossible to correlate all the data and to explore all the divergent hypotheses. Certainly they cannot be set forth in one brief article. One aspect of the newer research is related to the use of endocrine and endocrinelike preparations, and some details concerning this phase of investigation may be interesting, since good therapeutic effects have been obtained in the endocrine therapy of cancer. There are some researchers who have advanced so far in their studies that their work is said to form the basis of a specific phase of investigation, cancer chemotherapy.

Less than a decade ago, there appeared a report indicating that the synthetically prepared chemical diethylstilbestrol, which is capable of eliciting responses in the body similar to those obtained by the administration of estrogens, can influence the course of prostatic carcinoma. Surgical castration was known to achieve similar results, and some individuals began to speak loosely of "hormonal castration." The fallacy of such a term in our present state of knowledge is obvious, but its suggestiveness did provoke, nevertheless, some interesting contemplation. In spite of conflicting reports, it was evident that the use of estrogens with or without castration increased the survival rate of those suffering with prostatic carcinoma, relieved pain, and permitted increase in weight, appetite, and vigor. It was equally apparent that such treatment was palliative and not curative.

Following these earlier observations, research workers demonstrated that endocrines might have value for the treatment of cancer elsewhere in the body—for example, in the breast. This probably seemed surprising to some individuals, because researchers have known for years that excessive doses of certain endocrines can cause, at least in experimental animals, the formation of tumors; but it merely reflects once again the variations that occur in body responses as the intensity of potent influencing factors is varied.

As might be expected, cancer chemotherapy became such a lively topic that research spread in all directions. Many investigators studied the effects of hormones on tumor processes and published their results, often without being aware of what was under investigation in other laboratories. Time sometimes is lost because of lack of knowledge of data held by the various researchers and by repetition of work already completed but not reported. Because of the importance of the cancer problem and the need for advancing research in this field as quickly as possible, there was formed by the Therapeutic Trials Committee, a standing committee of the Council on Pharmacy and Chemistry of the American Medical Association, a subcommittee to encourage the coordination of effort of clinical researcher and drug manufacturer. Already certain conclusions can be drawn. Early in its organization this subcommittee warned that steroid hormone therapy does not provide a cure for carcinoma of the breast, the malignancy origin-

CAUSES OF DEATH BY SEX
United States - 1946



ally studied by this group. Since then other malignancies have been under consideration, but there still is no indication that this form of therapy is more than palliative; in certain carefully selected cases symptomatic relief is definitely provided, and the life expectancy in some individuals may be prolonged. On the other hand, there is urgent necessity for proper selection of cases for steroid hormone therapy, and the possible dangers of careless choice and mismanagement of cases cannot be emphasized too strongly. Furthermore, as the research progresses new protective findings arise; for example, estrogen administration may cause water retention, which may influence a weakened heart unfavorably.

Radical surgery with or without roentgen radiation is the only procedure recommended for curable malignant lesions, but careful selection of patients whose disease has progressed too far for operation may permit hormone treatment. Androgen therapy sometimes is of help to patients with metastatic lesions in bone, there following within two or three weeks relief from pain, increased appetite, gain in weight, less need for narcotics, and renewed interest in life. The relief is comparable to that seen in cases of prostatic carcinoma after castration. Beneficial results are seldom observed on the primary disease or soft-tissue metastases, although intensive testosterone therapy in premenopausal women may lead to a so-called chemical or medical castration which will result in changes in soft tissue. At the same time the bone undergoes some repair and regeneration, as observed by roentgenologic studies and changes in the chemical findings in the blood. The duration of relief is variable, as recurrences ensue, but further treatment may be given.

The criteria for the selection of patients for estrogen therapy differ from those for androgen therapy. Favorable effects appear in older women who have soft-tissue lesions, definite regressions often occurring in the primary tumor and the metastases. There is considerable variation in the responses among individual patients, and relief usually persists for comparatively short periods only.

The scope of the project undertaken by the Subcommittee of the Therapeutic Trials Committee can be estimated by the number of current collaborators: fourteen manufacturers of the androgens and/or estrogens and fifty clinical centers are pooling their efforts and knowledge. Furthermore, the Cancer Grants Division of the National Cancer Institute and the Committee on Growth have been kept informed, and the American Registry of Pathology will receive biopsy material for examination by a group of consulting pathologists. Similar arrangements have been made for the review of X-ray films obtained in this study.

With such exhaustive projects in operation and with many other investigators continuously seeking new information on cancer and related cell abnormalities, many and diversely interesting approaches are being made to the problem. For example, studies are being made on animals to determine the influence of heredity, carcinogenic compounds, nutrition, and possibly even viruses on tumors. Thousands of pure chemicals and other substances have been synthesized, isolated, or extracted as treatment measures. Treatment has ranged from hormones to bacterial filtrates, colchicine, podophyllin, urethane, nitrogen mustards, and radioactive elements. Even substances such as

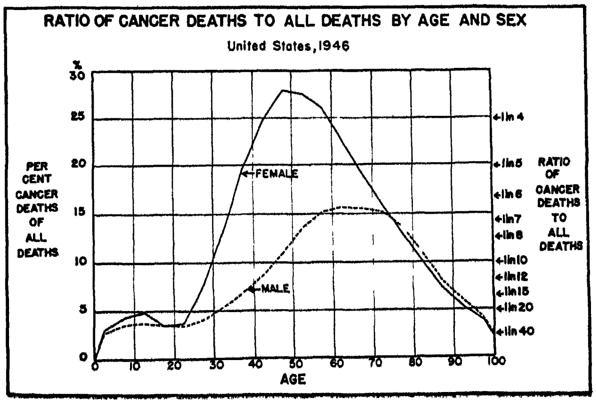
the antireticular cytotoxic serum of Bogomolets (ACS) and the endotoxin of *Trypanosoma cruzi* ("K.R.") have been subjected to scrutiny as possible therapeutic agents.

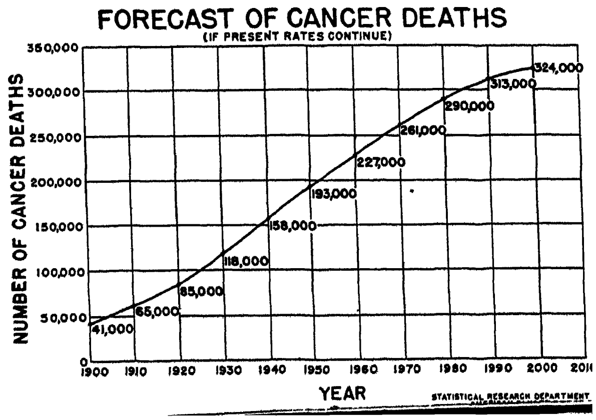
Sometimes promising agents later prove toxic or otherwise disappointing, such as has been the case for urethane in the treatment of chronic myelogenous leukemia. Attempts also have been made to develop technics or substances that are specific for the unwanted tissues; thus, some have searched for radioactive isotopes that would be absorbed by the cancerous process and not by the surrounding healthy tissue. Further work obviously is needed in this phase of cancer research, but at least one group of researchers claims to be able to distribute as desired radiation in the liver, spleen, and bone marrow by varying the particle size and the compound. Radioactive iodine appears to have selective action when properly administered. Hormones of the androgen family; rutin (which is supposed to aid in the control of bleeding following exposure to radiation); substances such as "Fraction X" (it resembles methylcholanthrene under spectroscopic examination) from cancerous human livers; the inactivation of enzymes such as beta-glucuronidase (this enzyme is related to the activity of the so-called "sex" hormones which have been shown capable of developing cancer in mice); the use of toluidine blue to counteract bleeding following an overdose of X-rays; the development of radioactive gold; the use of isotopes for the treatment of polycythemia; the use of irradiated zinc to study leukemia; the use of urethane for the treatment of leukemia (and the subsequent discovery that this chemical causes pulmonary edema in leukemic mice); the use of radioactive diiodofluorescein in the diagnosis and localization of brain tumors, and irradiated cobalt (which may be as effective as radium and is more abundant and cheaper) also are items of discussions involving cancer. The use of radioactive isotopes has become so important in today's research that a training course has been organized at Oak Ridge, Tennessee, for those who may handle the materials.

Occasionally enthusiastic reports appear which later require modification, as has been suggested elsewhere in this article. Unfortunately, such reports mislead the hopeful, who are ever anxious to leave proved treatment in favor of the new ideas that may only result in death; or, if individuals with questionable reputations are involved, serious financial loss may ensue. Furthermore, it is sometimes necessary for renowned and careful researchers to evaluate the newer proposals to obtain a more realistic appraisal. Such waste of valuable time is unfortunate.

Until recently the effective treatment of cancer consisted of the use of surgery to remove the offending tissue and the employment of radium or X-rays to affect unfavorably the growth of the malignant cells. From time to time there have appeared reports of miraculous "cures" from some simple chemical or from a strange mixture of botanicals, biologicals, or even earthy material. None of the "cures" so far has resisted careful scientific scrutiny, and sooner or later they have disappeared, although often not before irreparable damage was done to many sufferers. Usually the claims for cure were based on the results observed in individuals whose maladies were incorrectly diagnosed. Occasionally, a concoction was used in persons who experienced spontaneous recovery from their affliction. Such spontaneous recoveries are yet to be explained satisfactorily by those trained in biology and pathology, but it is known that they do occur rarely, without treatment.

The problem of trying to determine specific and effective chemotherapeutic measures for cancer is complicated by the lack of knowledge that exists concerning the growth of cells, normal as well as abnormal, and the mechanism by which cancer





invades other tissues. Cancer cells do not become organized in orderly fashion like the cells in the normal body organs, and yet they seem to obtain nourishment. Furthermore, there are various kinds of tumors which often seem to behave independently of each other. In addition, some tumors cause symptoms such as fever, loss of weight, weakness, and anemia, whereas others result in practically no such signs of disease.

Cancer can be studied in animals—in fact, it can be precipitated in animals by hundreds of agents, from X-ray to simple mechanical irradiation—but findings obtained on laboratory animals cannot always be transposed to humans. It is known that many cancer cells, on chemical analysis, fall within the range of the findings of some normal tissues, although there are differences; for example, the consistently high rate of glycolysis and increased water content in cancer cells. What major differences exist still remain to be solved by the development of more acute chemical and other tests.

Cancer chemotherapy can be approached in several ways, such as direct destruction of the cancer cell, prevention of invasion, interference with growth, and by other means. The best attack remains yet to be determined. It does seem apparent that the most effective agent has not been developed, nor has the most effective method of attack, or attacks, been discovered. It is possible that specific treatment may have to be initiated for each type of cancer and perhaps for the individual, so that ultimately cancer may be subjected to a barrage of treatments in the same way that infectious diseases are now assaulted by chemotherapy. One thing is certain, however: researchers interested in the control of cancer are now standing on the brink of some remarkable discoveries, and these will surely become known within a comparatively short time.

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FIBERGLAS IN DENTISTRY

THE quest for new products for dental application is virtually continuous. Through constant research, additions to the materials used in prosthetic dentistry have become numerous in recent years. Not quite as many new products have reached the field of operative dentistry, however.

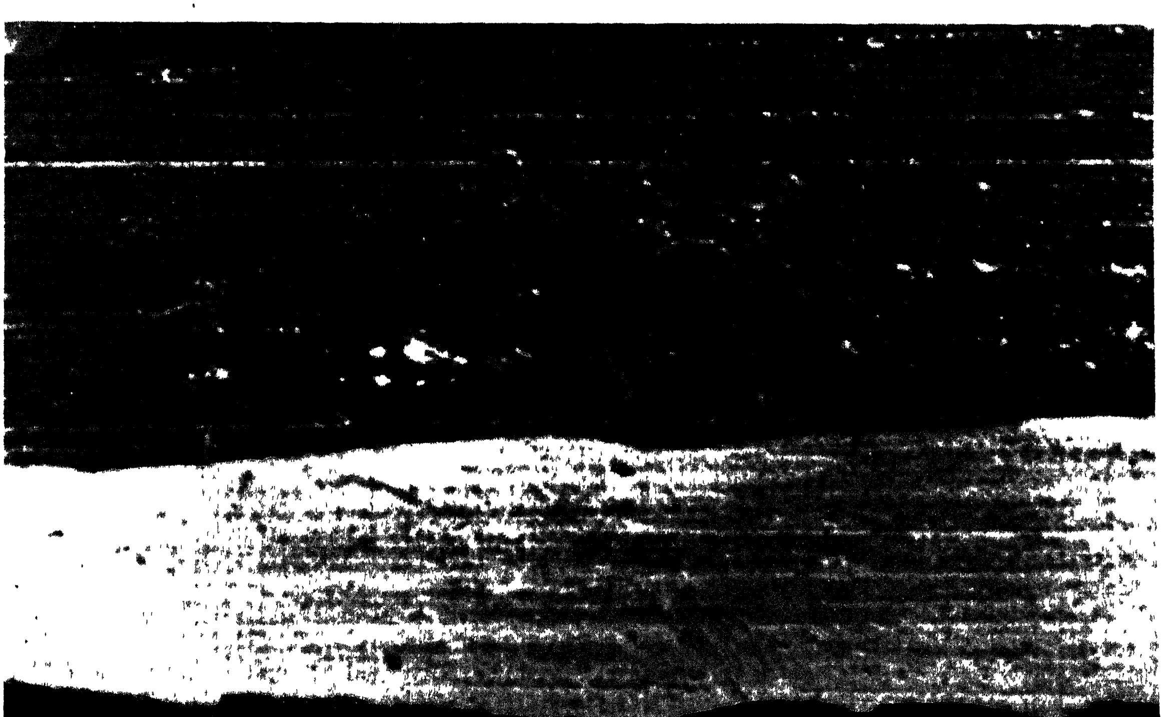
In operative dentistry, for almost a hundred years, the search has been pursued for a suitable root-canal filling material that would possess, among other necessary properties, the one of great dimensional stability. From the historical standpoint it is of interest that since 1847, when gutta-percha was first introduced into dentistry, a number of other materials, in quest of a better one, have been tried. Included in these experiments through the years were silver amalgam, asbestos, balsam, bamboo, cement, copper, cotton, crystallizable substances, gold, indium, ivory, lead, paper, paraffin, pastes, pitch, rosin, rubber, spunk, thistles, wax, and wood. And as early as 1859 root canals were occasionally filled with condensed cotton saturated with creosote.

Of this diverse group of materials, gutta-percha is the most widely used for filling root canals. It it used for this purpose in the form of points and small pieces, in a solution of chloroform or eucalyptus. The Council on Dental Therapeutics of the American Dental Association, however, will not accept root-canal filling materials that may undergo significant volumetric change. On page 175, *Accepted Dental Remedies, ADA* (13th ed.), we read: "A volatile solvent, such as chloroform, would tend to evaporate and thus cause a significant change in the volume of a root canal filling."

For an improved root-canal filling material Fiberglas serves as a reinforcing material in a capacity comparable to that whereby steel reinforces concrete. Fiberglas is glass in a fiber or filament form. It is an inorganic, nontoxic, nonallergenic, nonsensitizing, and chemically stable substance that produces no harmful effect upon human tissues. It is pliable and possesses great tensile strength and a high degree of dimensional stability. Fiberglas is nonhygroscopic and noninflammable. The individual fibers do not absorb water; Fiberglas is therefore easily sterilized and resterilized. Glass is not classed as a carcinogenic substance. In medicine, Fiberglas materials have been used for measurement of nitrogen loss in exudate from burned skin; tracer threads in surgical sponges; experimental surgical sutures; culture of microorganisms; blood plasma filters; airborne cross-infection control; penicillin production; Fiberglas-plastic artificial limbs; and insulation of sterilizable closed motors.

Glass fibers possess the greatest tensile strength-weight ratio of any commercial material, natural or synthesized by man. Fibers averaging 23/100,-000 inch in diameter have a tensile strength of more

Tooth section showing silver amalgam filling in crown and Fiberglas mass base. ×25



Section showing Fiberglas mass used as a base under silver amalgam filling. ×25.

than 250,000 pounds per square inch. Experimental fibers have been produced with a diameter of 2/100,000 inch and with tensile strength considerably higher.

The glass fibers possess remarkable dimensional and physical stability. At tension approaching breaking strength the fibers show an elongation up to 3 percent. High tear strength and dimensional stability and resistance to moisture, heat, chemicals or oil are responsible for the utilization of glass fibers in dentistry. In the selection of a root-canal filling material, great dimensional stability is a cardinal property for consideration. For this purpose a root-canal filling has been made, and used experimentally and clinically, consisting of zinc oxide, eugenol (or beechwood creosote), a trace of rosin, and Fiberglas hammer-milled to 1/16-inch lengths, superfine yarn, clean glass without treatment. (Some Fiberglas has been treated with a water repellent.) The physical appearance of the Fiberglas is not unlike that of white cotton wadding. The filaments are about 3 microns in diameter, and the material is very pliable.

To make the Fiberglas mass, the ingredients without the Fiberglas are first spatulated to a medium thick mass and the Fiberglas is then worked into this mixture. It will be noticed that at this stage the mixture becomes a little thinner as the Fiberglas is incorporated. To obtain the desired consistency, more zinc oxide is added with more Fiberglas as necessary. The resulting putty-like mass can easily be divided into any-sized portions or small particles without shredding. The mass is now ready for use as a root-canal filling material, as a base under metal restorations, as a temporary filling, or as a capping over exposed healthy pulps. To reduce the setting time of the Fiberglas mass, zinc acetate to a minimum of 0.5 percent of the solid portion is added. As a root-canal filling material, clinical evidence has proved the Fiberglas mass to possess the following advantages: plasticity, comparative ease of manipulation, ready conformity to the contour of the canal; no evaporation, as in chloroform-gutta-percha mixures, during filling and X-ray checking procedures; great dimensional stability, assuring intactness of a filled canal; insolubility in tissue fluids; radiopaque property, which permits X-ray recording of the stages of filing and upon completion; does not absorb moisture; is impervious to fluids; has no capillarity; filling material can readily be removed from the root canal if necessary.

It has been further clinically demonstrated that the Fiberglas mass makes a dependable temporary filling material. Considerable edge strength and wear-resisting tendencies make such reinforced fillings useful in the treatment of certain cavities in deciduous teeth. The substance seems to have a density closely comparable to that of dentine, as condensed gold-foil restorations have been placed in teeth in which the Fiberglas mass was used as a base. As a protective base under silver amalgam fillings the Fiberglas mass has been found clinically satisfactory.

After being treated with a water repellent, Fiberglas can be placed over cohesive gold foil in the container before the cork or stopper is inserted, giving better protection than cotton wadding.

In conclusion, it can be stated that it has been demonstrated that the great dimensional stability and the high insulation property of the material make the utilization of Fiberglas of distinct value for the dental purposes described.

REFERENCES

MAETH, H. Fiberglas Used as a Root Canal Filling Material, Preliminary Report. *Dental Digest,* 1945, 51, 555–6.

MAETH, H. Fiberglas Used as a Root Canal Filling Material. Progress Report. *Dental Digest,* 1947, 53, 332–5.

MAETH, H. Further Applications of Fiberglas in Dentistry. *Dental Digest,* 1948, 54, 451–4.

SCHOLZ, R. P., and MOUNTJOY, P. S. Fiberglas Suture Material, Preliminary Report. *Am. J. Surg.,* 1942, 61, 619–21.

Pioneering Uses of Fiberglas Materials in Medicine. Owens-Corning Fiberglas Corp., 1945.

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THE EARTH SCIENCES PROGRAM OF THE OFFICE OF NAVAL RESEARCH

On August 7, 1946, the Office of Naval Research was permanently established by the Seventy-ninth Congress through Public Law No. 588. At the same time the old Office of Research and Inventions, established in 1945, was abolished. As interpreted from the legislative charter for the Office of Naval Research, one of the primary functions of the Office is to establish fundamental research projects and to stimulate research in various ways, such as by providing government-furnished equipment under research contracts, providing necessary military transportation, and making available to scientists surface craft, aircraft, and submarines whenever a justifiable need arises under a research contract. One other prime method of stimulating research has been the

organization and carrying out of symposia upon various scientific problems that confront scientists in industry and universities today.

The field activities under the cognizance of the Office of Naval Research are The Naval Research Laboratory at Anacostia, D. C.; the Underwater Sound Reference Laboratory at Orlando, Florida; the Special Devices Center at Sands Point, Long Island; and the branch offices at Boston, New York, Chicago, San Francisco, Los Angeles, and London, England. From this it can be seen that we are spread fairly well over the country, with one arm reaching into Europe.

The headquarters office in Washington is divided into three main groups: Administrative, Patents, and Research. Under the Research group is the Physical Sciences Division, which is divided into branches that are concerned with Physics, Nuclear Physics, Mechanics and Materials, Mathematics, Chemistry, Fluid Mechanics, Electronics, and Geophysics. It is the Geophysics Branch that has charge of the Earth Sciences program. We have arbitrarily separated meteorology and oceanography into sections of their own. We do not refuse to call these fields earth sciences, but for reasons of our own we prefer to include in the earth sciences those fields, such as geology, geography, vulcanology, glaciology, gravimetry, etc., which are fairly well earth-bound.

Before discussing the earth sciences program, however, it may be well to speak of the philosophy of the Geophysics Branch, which necessarily is a part of the philosophy of the entire Office. In the first place, we try to avoid soliciting research projects; we prefer instead to have the ideas for these projects spring spontaneously from the minds of the scientists of the country. When this occurs, we are confident that the scientist is interested in the work that he proposes to do and we can feel sure that the work will be carried out to its logical conclusion. Second, we try to avoid telling the scientist how to carry out his project. And, third, we do not place restrictions upon the publication of scientific results, unless, of course, these results are of a classified nature, in which case we try to warn the scientist in advance that his publication will quite likely be curtailed. We thus allow him to choose whether he wishes to carry on the work.

It may be well to mention also that the Geophysics Branch has gone through its stage of expansion. We can no longer take on new research projects at the same rate indulged in a year and a half ago. We can do some new work, but in every case we try to support only the best of the good projects that come to our attention. Eventually it will become necessary to drop one old project for each new one supported, but that day has not yet arrived. At the present time we are still welcoming the submission of new research proposals, with the warning that they are being considered in competition with proposals from all over the United States, and that most of the proposals we are receiving currently are very good.

Since its establishment in 1946, the Geophysics Branch of the Office of Naval Research has allocated a share of its research funds to the support of the earth sciences. During the past two years a total of ninety-seven research proposals in the earth sciences have been received. Of this total, forty-two projects have been supported. Some of these have already been concluded. The number of projects now active are distributed among the various sciences as shown in Table 1:

TABLE 1

Geochemistry	5
Gravimetry	3
Seismology	7
Tectonophysics	2
Terrestrial magnetism	4
Geography	2
Geology	11
Instrumentation	3

In order to indicate the disposition of funds in the Geophysics Branch, I have constructed two diagrams. Figure 1 shows the percentage distribution of funds among the earth sciences during the past two years. This is intended to show only the order of magnitude of support in the various fields. Note that I have differentiated between geology and the sciences that deal more particularly with the physics of the earth. This graph shows that each of these earth sciences is receiving a substantially equal amount of funds. Such small differences as exist can be attributed to the fact that it costs more to pursue some types of research than others, and that a greater number of proposals for research have been received in some fields than in others. Geology is somewhat low in the percentage of funds received. This is not because research proposals in geology have not been received, but because of the fact that the Geophysics Branch believes that such sciences as stratigraphy, paleontology, and the like fall more logically within the purview of the Geological Society of America and the U. S. Geological Survey.

Figure 2 shows the percentage distribution of funds by agencies. This graph indicates that almost two thirds of the funds available are going into universities and institutions. Those funds

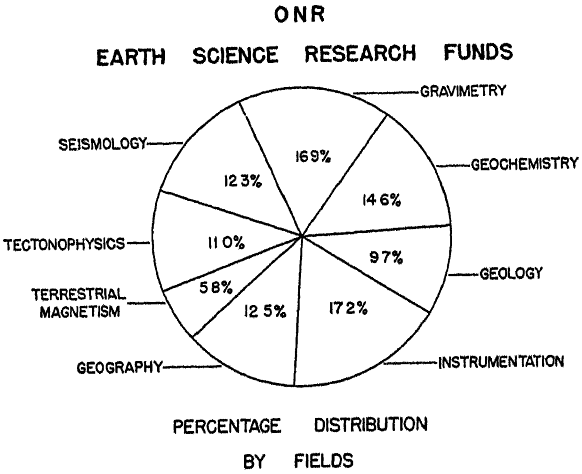

FIGURE 1

going to other government agencies are for the most part confined to specific instrumentation development, which cannot conveniently be done at a university.

Turning now to some of the specific projects that have been supported during the past two years, I shall have space only to report on a few examples from the entire program, but these examples have been chosen as a representative cross section.

In geochemistry, Dr. J. F. Goldsmith, at the Department of Geology, University of Chicago, has essentially completed one phase of his work on the disordered state of silicates. Working with melilite solid solutions, he has shown that as much as 15 percent of $Na_2Si_3O_7$ will go into solid solution with gehlenite and that, at the other end of the isomorphous series, akermanite will not form a solid solution with the same material. Professor Goldsmith discusses some of the factors involved in the reported high Na_2O content in some of the melilites toward the akermanite end of the series in a paper published in the *Journal of Geology* for September 1948. His paper also discusses the possibility that gehlenite may take as high as 10 percent calcium aluminate into solid solution.

In the field of gravimetry, the data now being collected by Professor Maurice Ewing and his associates at Columbia University, when compiled, will give us a better understanding of the shape of the geoid and some of the problems associated with the orogenic zones of the earth's crust. To date, nine gravity-measuring cruises on board U. S. Navy submarines have been completed, and the tenth cruise began last December. Cruises made up to the present have been along the eastern coast of North America, in the Caribbean from Balboa to Trinidad, in the region of the Bahamas and the Straits of Florida, down the western coast of South America from Balboa to Valparaiso, and currently across the southwest Pacific. No report has been received on the number of stations occupied on the recent Pacific cruises, but through June 11, 638 stations had been completed from the nine cruises made.

One of the important projects in seismology under ONR is that being conducted by the Carnegie Institution of Washington under the joint directorship of Drs. Merle Tuve and Roy Goranson in cooperation with the Naval Ordnance Laboratory. The work is concerned primarily with measurements of the thickness of the earth's crust by means of artificial explosions. Instrumentation having the proper characteristics was developed as the first phase of this project. After development, the second phase has been to determine the layering of the crustal structure around the District of Columbia and the Appalachian Highlands. A total of thirty-two seismic shots, ranging in size up to 4,000 pounds, have been made. Indications are that the granitic layer in the Washington, D. C., area is 10 km in thickness, the first basaltic layer extends on down to 24 km, and that the second basaltic layer extends down to 42 km, or to the depth of the Mohorvicic Discontinuity.

A project in the field of geography that will be of considerable incidental interest to all scientists is project CIMA, or the Coordinated Investigation of Micronesian Anthropology. This work is under the directorship of Dr. Harold J. Coolidge, of the National Research Council. CIMA is directed toward an understanding of the peoples of the Trust Territories of the Pacific, their past history, and present-day social customs and con-

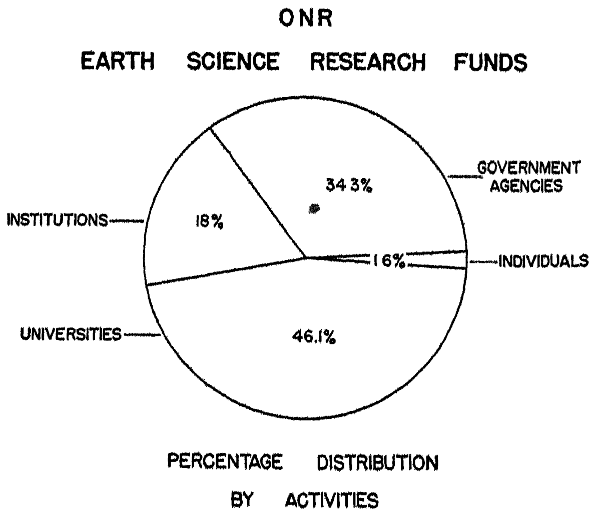

FIGURE 2

ditions of health. Approximately forty-two geographers and anthropologists from twenty-three universities and museums have participated in project CIMA during the past year. The results of this work are being incorporated into a volume known as the *Pacific Ocean Handbook,* which was compiled at the Stanford School for Naval Administration under another ONR project. This volume will be available shortly at the U. S. Government Printing Office and will contain information of great value to those scientists whose main interests lie in the Pacific Ocean areas.

Many geological projects have been sponsored; I can mention each of them only briefly, however.

Professor Francis P. Shepard is pursuing his investigations of the west coast beaches and offshore bottom areas under our large oceanographic contract at the Scripps Institution of Oceanography. I refer you to his recent book entitled *Submarine Geology* (Harper, 1948).

Drs. Belcher, Hemstock, and Kranck, working through the Arctic Institute of North America, have conducted investigations into the geology and permafrost conditions of the Arctic and sub-Arctic.

Professor Robert Sharp, of the California Institute of Technology, has conducted an investigation in glaciology in the Mount Saint Elias Range. The final report of this work has not yet been received.

The U. S. Geological Survey has conducted several investigations in cooperation with the Office of Naval Research. The most outstanding of these projects have been the airborne magnetometer survey of the Aleutian volcanoes and the northern Marshall Island areas and the excellent reconnaissance gravity survey conducted in the Gulf of Mexico just off Louisiana and Texas, which resulted in the location of several new possible salt dome structures out to a distance of about 75 miles from the shore.

I have tried, in this brief discussion, to give a cross section of the program at present being carried on in the earth sciences by the Office of Naval Research. The ultimate responsibility for this program rests with ONR, and more specifically with the six scientists of the Geophysics Branch. It has been found beneficial, however, to seek outside advice in making certain that only the most scientifically worth-while projects are supported and to ascertain that the funds are going to competent scientists, with adequate facilities and staffs.

To this end, the National Research Council established an advisory committee on the earth sciences for the Office of Naval Research. This committee has the responsibility of advising on each research proposal submitted for possible financial support. The names of the members and administrative directors of this committee have been given wide publication in the professional journals of this country and need not be repeated here. It is in order, however, for me to say that the advisory committee has worked long and well in discharging its voluntarily accepted responsibilities; the members are due a vote of thanks from the geologists and geophysicists of the country for their valuable assistance in the direction of public funds along worth-while channels.

GORDON G. LILL

Geophysics Branch
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BOOK REVIEWS

THE HISTORY OF SCIENCE IS SCIENCE

Readings in Biological Science. Irving W. Knobloch, Ed. 449 pp. $3.00. Appleton-Century-Crofts. New York.

THIS anthology is designed to provide supplementary reading material for liberal arts students taking an introductory course in biology. With the exception of the first chapter, which includes selections from the works of Hippocrates, Aristotle, Pliny, Leeuwenhoek, Darwin, and Huxley, the bulk of the book is made up of extracts from the writings of modern authors, fifty-eight in all. Many of the authors are professional biologists and teachers, some are physicians, and a few are professional science writers.

The subject matter is classified under fifteen chapter headings as follows: Biological Background; Life and the Cell; The Structure and Function of Higher Plants; Nutrition; Circulation; Nervous and Endocrine Control of the Body; Reproduction; Embryology; Heredity; Eugenics; Evolution; Ecology; Health and Disease; Economic Biology; and Biological Philosophy.

Among the authors whose writings appear may be mentioned Donald C. Peattie, S. J. Holmes, Henshaw Ward, Russell M. Wilder, A. J. Carlson, Emanuel Radl, T. H. Morgan, A. Scheinfeld, W. M. Krogman, W. Overholser, B. Jaffe, J. S. Huxley, and many others.

Brief biographical notes and a reading list appear in an appendix. There is no index.

In his aim of selecting materials characterized by readability and popular appeal, the editor has been eminently successful. One may be permitted to question, however, the success of Dr. Knobloch in selecting a group of readings which will actually contribute to the student's ability to understand and think about the problems of science in our present society. In his introduction, the compiler points out that "World War II has shown us the important fact that the human mind is capable of performing astonishing feats when put under pressure." If this view is correct, then it would seem that the readers for whom this book is intended should be able to handle even more difficult material than is here presented. The jaw muscles are hardened not by custard but by chewing on tough meat. The development of a "tough-minded" society capable of wrestling with the problems of today is accomplished by hard, not easy, reading.

If a second edition of this book is called for, it is hoped that the compiler will consider the inclusion of some less popular but possibly weightier discussions than are contained in the present book. Selection from the vast amount of material available will not be easy, but the results would justify the extra effort. There is no educational task today as important as that of imparting something of the spirit of science and of scientific inquiry to those who will, as adults, not follow the profession of science or aspire to the title of scientist.

MORRIS C. LEIKIND

Library of Congress
Washington, D. C.

The Shipwright's Trade. Sir Westcott Abell. xii + 219 pp. Illus. $4.50. Cambridge Univ. Press. New York.

THE title of this excellent book may be misleading to many Americans, as the word "shipwright" has a much narrower meaning in the United States than it has in Great Britain. The book deals with the evolution of ships and the development of the art of ship design and shipbuilding as a whole rather than with the shipwright's part as we use the term.

The book is written down to the ready comprehension of the layman, as the author explains much of the terminology that is peculiar to shipbuilding. It is regrettable that a little space was not also devoted to an explanation of those baffling terms "ton" and "tonnage" as applied to ships.

In Part I the author briefly covers shipbuilding in ancient times and as practiced by primitive people in modern times. In Part II he deals with the wood shipbuilding era and weaves the biographies of some of the distinguished English master-builders into the story. The pages devoted to the Bakers, father and son, the three Petts, Sir Anthony Deane, and William Sutherland are particularly interesting. He describes the steps in the transition of the shipwright's trade from a secret and mysterious craft, handed down from father to son, to an art with its roots well implanted in science. The era of wood and sail in marine transportation reached its climax early in the nineteenth century. The author writes the obituary of this era in the following lyric paragraph: "The wooden structure had reached about its limit, and was not to appear again in marked fashion until much later in the century. Then for a brief spell came the clipper ships, vessels with iron frames and with wooden skins and decks to amaze the world in the Swan Song of the Sailing Ship."

The last half of the book, Part III, treats of iron, steel, steam, and oil in the designing, building, and operation of ships. Biographical sketches of those titans of science and engineering, I. K. Brunel, Scott Russell, William Froude, his son, and of Charles Parsons, enliven the pages. The author mentions only

briefly those in other countries who improved the ship. His principal interest would naturally lie with the British contributions, but it is surprising to find only a paragraph devoted to the Swedish naval officer Fredrik Chapman, who in the United States is regarded as the greatest and most scientific naval architect of the eighteenth century.

Profuse illustrations add greatly to the charm and value of the book. Sir Westcott merits the thanks of all who are interested in the sea and in ships for the enormous labor he must have put into the writing of the book. He is to be congratulated on the result achieved.

JULIUS A. FURER, Rear Admiral, USN (Retired)
Washington, D. C.

The Life of Science. George Sarton. vii + 197 pp. $3.00. Henry Schuman. New York.

THIS collection of essays by Dr. George Sarton, professor of the history of science at Harvard University, is designed to provide the general reader with a better understanding of science—its history, scope, purpose, and methods.

Beginning with essays devoted to the story of the spread of understanding, the book covers such matters as the story of early discoveries, the role and importance of special methods and instruments in science, and the relation of these searching activities to other activities of man—in medicine, in religion, in art, and in politics. In this sense the history of science becomes in part the history of civilization, as emphasized in the subtitle of the book.

Following this overview of scientific development, the author documents scientific progress with several biographies of men of science, pointing out that the history of mankind may be divided into (1) political history, or a history of the masses, and (2) intellectual (scientific) history, or the history of a few individuals. Political history, unfortunately, has claimed the main attention of historians, he says. Intellectual history, therefore, needs to come into its own, for it is in intellectual history that one finds the essential aspects and story of human progress.

Political vicissitudes, wars, revolutions, natural catastrophes, are for most men events of primary significance, Dr. Sarton continues, for the reason that man feels directly affected by such occurrences. Yet it is the discoveries of a Galileo or a Newton which, by transforming man's outlook, constitute the cardinal events of the world's history. This is the *essential* history of mankind—and it deserves to be better known.

This small, erudite, highly readable book deserves a place on the required reading lists of all students of science, whether in college or beyond it, as an aid in the extrication of self from time-narrowness. For the history of science, as Dr. Sarton puts it,

> even more than ordinary history . . . familiarizes us with the ideas of evolution and continuous transformation of human beings; it makes us understand the relative and precarious nature of all our knowledge; it sharpens our judgment; it shows us that, if the accomplishments of mankind as a whole are really grand, the contribution of each of us is, in the main, small, and that even the greatest amongst us ought to be modest. It helps to make scientists who are not mere scientists, but also men and citizens.

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Exploring Electricity. Hugh Hildreth Skilling. vi + 277 pp. Illus. $3.50. Ronald Press. New York.

A PLEASANT surprise awaits the readers of *Exploring Electricity*, by Dr. H. H. Skilling. The author has a real literary style that is seldom found among textbook authors. Even if the story were not one of exceptional interest, the author's way of telling it would hold the reader. *Exploring Electricity* thus takes on a live, vital quality in telling the stories of the men who make up electricity's family tree. It has none of the forced, labored, and obvious effort of a scientist trying to step out of character and temporarily replace the studied, meticulous, and cold accuracy which is inherent in science with the feel of the pulse of life and the vagaries of individual humans.

The story—and it is a real story—takes the development of the key discoveries and advances in electricity from their earliest beginnings in ancient times through the release of atomic energy in the bomb. Not only do you get the "feel" of such great names as Franklin, Galvani, Volta, Ampere, Faraday, Henry, Maxwell, Hertz, Bohr, Fermi, and Meitner as living people hemmed in by human weaknesses and nobilities, you see how their contributions complement each other to achieve real progress. Not once does the author forget and drop back into those purely technical passages which would leave the nontechnical reader high and dry—particularly dry. Nevertheless, technical readers will find pleasure in the way the author's own technical proficiency has enabled him to give the story the cross checks and tie-ins necessary to those who know the field.

Checking over electrical textbooks written by the author, your reviewer found an interesting and unusual thing. Here and there in the mathematical and scientific material composing the text are inserted paragraphs of a vital type which bring in the lives and accomplishments of the men who have made electricity live. In fact, almost a whole page can be found in one text covering Faraday, Ampere, and their relationship to Maxwell and his hypothesis.

Not only is this a book which anyone who has any interest whatever in electricity should read, it is a book that should be collateral reading for all whose studies take them into that field.

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American Telephone and Telegraph Company
New York

CORRESPONDENCE

S. 247

Now that the National Science Foundation is before the Eighty-first Congress, it is appropriate that scientists also keep it actively in mind. On February 25, S. 247 was reported out of the Senate Committee on Labor and Public Welfare. It is virtually identical with the bill that was passed unanimously in the Senate in the spring of 1948, and it was frankly introduced without any changes in content or in phraseology because it had such an easy time getting through the Senate. Perhaps scientists, who are more intimately concerned with its provisions and its significance, should review it more critically than the senators.

Like its predecessor in the Eightieth Congress, S. 247 is claimed to be a compromise bill. The odds are in favor of its passage, and there is definite reason to believe that it will not be vetoed, even though it is not the type of legislation that the President is known to favor. Direction of Foundation affairs is vested in a board of twenty-four civilians, who will serve on a part-time basis, and who will determine policy. Functioning as the active administrator of Foundation activities is a director, who will be appointed by the President and who will apparently have dual responsibilities to the President and the board. The duality of this position makes it a rather unattractive post and creates administrative problems which any executive would instinctively avoid.

As in all the earlier bills that have passed one or both houses of Congress, the social sciences are not specifically included; and, as if to justify the omission, the bill stresses the promotion of "basic" research. It may be questioned whether basic and applied science can be so sharply differentiated in a Foundation which should be charged with the responsibility of formulating national policy in scientific matters.

S. 247 also provides (Section 14, Subsection (1)) that "The Foundation, after consultation with the Secretary of Defense, shall establish regulations and procedures for the security classification of information or property (having military significance) in connection with scientific research under this Act, and for the proper safeguarding of any information or property so classified." The wisdom of extending security classification into new areas of scientific research may be seriously questioned and viewed with some alarm. Certainly, a recommendation that has been made by the American Federation of Scientists should be weighed carefully, and scientists should express themselves freely on this important problem to their representatives in Congress, and, more especially, to the members of the House Committee on Interstate and Foreign Commerce, to whom this legislation will be referred. The Federation of American Scientists has urged that this section be amended to read: "It shall be the general policy of the Foundation not to give continuing support to research requiring security classification. When any research under its auspices is deemed by the Foundation, in accordance with such national policies as may be established by Congress, to require security classification, the Director shall consult with the Secretary of Defense to determine the appropriate agency to which support of the research should be transferred. Pending such transfer, security classification and procedures shall be determined by the Director after consultation with the Secretary of Defense."

Although a great deal of scientific advice has been contributed to the senators (notably Senator Smith of New Jersey) who have formulated this bill, it is important that all scientists familiarize themselves with its contents. There has been inadequate expression of opinion, and it seems simpler to organize a reasonably satisfactory and generally acceptable Foundation at the start than to tamper perennially with an imperfect organization. In the Seventy-ninth and Eightieth Congresses, congressmen complained that scientists were inarticulate. If they remain inarticulate while National Science Foundation legislation is under consideration, they must assume full responsibility for the kind of legislation they get.

HOWARD A. MEYERHOFF
Administrative Secretary
American Association for the Advancement of Science
Washington, D. C.

HOOP-SNAKE STORIES

In his very interesting paper on "America's Mythical Snakes" Mr. Moore states: "There appears to be no classical or European analogue of the American hoop-snake story." It may be true that no written record is to be found on the subject, but verbally such stories certainly do exist. Some fifty years ago, when I grew up on a farm in northern Sweden where the common viper (*Pelias berus*) occasionally was seen, small children were earnestly requested, when seeing a snake, always to run uphill. If one were running down a slope, the snake might form a hoop and catch the runner. It is impossible to judge in what degree the warnings were really in earnest, but that at least some people believed in the stories seems likely. However, when we boys had reached the mature age of twelve to fourteen years, snake hunting became a good sport, and then, I think, the respect for hoop snakes had vanished.

ALFRED ASLANDER
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The Royal Institute of Technology
Stockholm, Sweden

INTERESTED

I am an interested, even though from an educational standpoint a quite unqualified, reader of this magazine. A number of the articles are technically beyond my full understanding, but many offer me mental food obtained from no other source, and stimulate my thinking to reach out and beyond my everyday life; my work, my house, my garden. Yet this thinking is also a part of these simple activities and enriches them.

RUTH R. PRETTYMAN
New Paris, Ohio

Correction: February issue, page 119, line 40, for "6 percent," read "60 percent."

THE

SCIENTIFIC MONTHLY

MAY 1949

THE LITERATURE OF ATOMIC ENERGY OF THE PAST DECADE

H. H. GOLDSMITH

Mr. Goldsmith is head of the Information and Publication Division of Brookhaven National Laboratory. One of the earliest workers in neutron physics at Columbia University, he was, during the war, coordinator of Information in Physics at the Metallurgical Laboratory, University of Chicago. He is co-editor of the Bulletin of Atomic Scientists *and senior consultant of the U. N. Atomic Energy Commission. He is also a member of the National Research Council Committee on Nuclear Science and chairman of its Subcommittee on Publication and Information.*

THE study of atomic energy spans half a century. During the first four decades its literature pictured a normal, active branch of modern science. In the past decade, however, nuclear science exhibits the most curious mélange of writings and silences in the history of science. For it was during this period that the swift transformation of the discovery of nuclear fission into the most fearsome of weapons coincided with the most devastating war of our era.

Even in its earlier "normal" phase, the literature of nuclear science was distinguished by its variety and rapid growth. On the one hand, it included the highly abstruse treatment of some of the fundamental intellectual problems of twentieth-century science, the establishment of the nuclear model of the atom, the analysis of the structure of atomic nuclei, the prediction and discovery of the elementary particles within the nuclei, and the study of the strange, powerful forces exerted between these fundamental particles. On the other hand, the literature mirrored a tumultuous growth in the minutiae of experiments—the details of the properties of the hundreds of individual nuclei, and the techniques ranging from setups reminiscent of sealing wax and string to the gargantuan gadgets of the most ingenious of modern engineers. It was the interaction of these typical aspects of science that resulted in the remarkable growth culminating in the thirties.

During the decade preceding the war, certainly in the field of physics, the study of the atomic nucleus had become the preoccupation of the majority of research workers. A mere glance at the *Physical Review* reveals the startling shift from the studies in atomic spectroscopy in the twenties, to the investigations of atomic nuclei in the thirties. The immense bulk of publications in this field is seen from the fact that about 15,000 titles are included in a bibliography on atomic energy recently issued under the auspices of the United Nations. This bibliography makes no claim of completeness; probably at least twice this number of papers have been published on the atomic nucleus and closely related topics. Of these, perhaps 20,000 have been published during the second quarter of the century.

The rich variety of subject matter of interest to the nuclear scientist may be seen from a glance at the table of contents of this bibliography. The classical branches of the field—natural radioactivity, mass spectroscopy, the effect of high-energy radiations on biological systems, the beginnings of tracer techniques—gradually merge into the exciting disclosures of the prewar decade. It is in the

thirties that the journals become crammed with the many significant discoveries from which are fashioned our present concepts of the atomic nucleus: the discovery of the neutron, the deuteron, the positron, and the meson; the detailed study of the products of the transmutation of all the nuclei; the disclosure of artificial radioactivity; the determination of the angular momenta of the nucleus by means of molecular beams, the unraveling of cosmic-ray phenomena. A mere listing tells the story.

The fission of the atomic nucleus ranked high among these discoveries. The swift succession of publications in early 1939, the many examples of practically simultaneous disclosure of the same phenomenon, reflect the excitement of the scientific worker of the period. Despite this burst of publication, there grew in the minds of more and more nuclear scientists a realization of the serious military consequences of nuclear fission, and this realization soon led to a radically different attitude toward the publication of scientific results in this field.

There exists no detailed documentation from which we can accurately reconstruct the events and thoughts of the beginnings of the atomic-bomb projects, and of the secret literature of atomic energy. Our sources for the work in the United States are the brief treatment of the early period in Smyth's official report, and the memories of some of the leading participants of this period. For the work in Germany there is no official story available with even as much detail as is given by the Smyth Report. But the prewar scientific literature, Goudsmit's book *Alsos,* and a postwar article by Heisenberg in *Naturwissenschaften,* permit us to discern at least the main features of the period.

What is striking is the similarity in the treatment of the publication problem in both countries. In Germany, within a few months of the discovery of fission, some of the more important nuclear physicists had formed a secret, unofficial society—the Uranium Club. One might, naïvely, expect that publications in this subject would cease abruptly. This was not so. The investigations by Hahn and Strassmann of the chemical nature and radioactive properties of the fission products continued to be published throughout the entire war. A moderate number of papers on other aspects of the fission process appeared in apparently normal fashion in the usual journals. To a casual reader, although the German journals of the war period were issued in an increasingly irregular fashion, the treatment of nuclear science seemed to be a continuation of prewar publications. The decrease in the number of papers in the subject, it could be reasoned, was the natural consequence of the diversion of the nuclear scientists from this field to problems of greater practical military importance.

A more careful scrutiny of the journals revealed, however, that all the published material was of the same peripheral nature as fission-product chemistry, and that central practical problems like the chain reaction were not discussed at all. We now know that this omission was not due to a lack of research in these topics, but rather to a suppression of publication of the results of research The discussions and investigations of groups like the Uranium Club were not published in the pages of *Naturwissenschaften* or the *Zeitschrift fur Physik.* They were initially reproduced in a very small number of copies for the exclusive use of members. A few months later, when Germany marched into Poland, the scientists presented their conclusions to government officials. Thereafter, their secret reports were handled in a more official and bureaucratic fashion. (The individual technical reports of the various German atomic-bomb projects have never been published in this country. A detailed technical summary of the reports has been prepared in the FIAT series under the auspices of the U. S. military authorities by the leading German nuclear scientists of the projects, but this has not yet been released in this country.)

A similar pattern of free publication of innocuous facts and the concomitant suppression of the more practical results concerned with nuclear fission is exhibited in the United States. It should be recalled that three years, instead of one, elapsed between the discovery of fission and our official entry into the war. Furthermore, an influential group of American scientists was frankly skeptical of the early claims by Szilard, Fermi, and Wigner that nuclear fission might play a practical role in the immediate future. Consequently, in early 1939, there was considerable resistance to the suggestion that this new, exciting academic field of research be placed immediately on a wartime basis with large-scale governmental support, and complete suppression of publication. It took the constant and imaginative prodding by the *émigré* nuclear scientists, led by Szilard, to alter these views.

Early in 1939, Szilard initiated a bold, unconventional attempt to establish a voluntary international censorship by all workers in nuclear fission outside Germany. Although the prestige of Niels Bohr was enlisted in this venture, lack of

cooperation by the French quashed the project. But this abortive attempt was fruitful in its educational effect on American scientists and editors. In the spring of 1940, almost two years before Pearl Harbor, a voluntary national censorship was successfully established by the scientists of this country. It was set up under the auspices of the National Research Council as a Reference Committee to control publication policy in all fields of military interest. If an editor of a scientific journal questioned the advisability of publishing a paper in nuclear science, the paper was referred to a small group of nuclear scientists constituting a subcommittee of the Reference Committee. Their completely unofficial decision was universally accepted by all the authors.

Of course, the pages of the *Physical Review* during most of 1940 and 1941 still presented a picture of lively activity in nuclear science. This activity was more apparent than real. Some of these papers represented early work which had been delayed in publication; others were reports of the final researches in the field by nuclear physicists who were already shifting to war work on radar; and a number of the papers on fission were from scientists in other countries—Japan, Denmark, Italy, Russia.

Behind the façade of normal publication was the growing mass of memoranda and technical papers describing the early secret work which was performed, first under the Advisory Committee on Uranium appointed, in the fall of 1939, by President Roosevelt, and, later, after June 1940, under the auspices of the NDRC. These secret reports were, in some cases, a continuation and refinement of the *Physical Review* articles of 1939. They dealt with the measurements of the neutron cross sections of uranium, the determination of the number of neutrons emitted in the fission process, and the study of the radioactive properties of various fission products. Most of the reports, however, described new concepts and strange setups, the exponential experiment for determining the multiplication constant, the lattice structure (or pile) of uranium and graphite, the detailed methods of separating the uranium isotopes, and the chemical, radioactive, and fission properties of neptunium and plutonium.

As the ramifications of the projects grew, and emphasis shifted to the combined scientific and large-scale engineering stage, the control of technical publication became much more formal. A policy of "compartmentation" soon emerged. This was based on the concept that the construction of the atomic bomb was the culmination of the separate but coordinated efforts of a large number of widely dispersed teams or projects. A member of a given team, it was assumed, could successfully complete his specific task without information concerning the work on the other projects. Thus, a person working on the problem of the chain-reacting pile to produce plutonium need not even know of the existence, much less the details of, the gaseous diffusion project for separating U-235. Nor did a member of the latter project have to be told about alternative schemes of separating U-235, such as the electromagnetic or the thermal diffusion methods. Further, a scientist on any of the above four projects could be valuable within his project without knowing anything about Los Alamos, where the construction of the bomb was being studied. Even within his own project, a scientist in one branch, say, biology, did not have access to all the results in some other branch, say, physics.

In effect, scientific publications were placed in separate compartments. At Chicago, for instance, within the Metallurgical Project (the scientific center of the project to manufacture plutonium), reports in physics were placed in series labeled "CP," and a report was usually identified by its number in this series, "CP516," or "CP2540," etc. Similarly, chemistry reports made up the series "CC——," biology and health reports, the series "CH——." On employment at the Metallurgical Project, a physicist was given access to reports in the "CP" series and, depending on his specific task, to a few peripheral categories. Under ordinary circumstances, he would not be permitted access to all the reports issued from the Metallurgical Project. Nor would he normally see any reports of the scientific work done on the separation of isotopes, or those concerned with the production plants at Oak Ridge and Hanford, or with the research and development work on the weapon.

Naturally, there was occasional official and unofficial relaxation of the strict application of compartmentation. After a scientist had become something of a veteran on the project, he was granted access to a sufficient number of subject categories, or to enough specific reports, to permit his functioning in a tolerable fashion. Some scientists, who were merely interested in their special subjects, even functioned in a quite normal manner. Further, the senior scientists, in time, obtained a complete understanding of their own project and, in outline, knowledge of the objectives of the other projects, with some inkling of the current stage of

progress toward these objectives. Even a junior scientist, on occasion, required information ordinarily stored in distant compartments; in such cases, his scientific superior, or the responsible administrator, permitted him a brief glance into some hitherto forbidden reports. Further breaks in compartmentation occurred with the rare shifts of personnel from one site to another. Thus, toward the end of the war, a sizable scientific group from the Metallurgical Project, having completed its assignments with the successful operation of Hanford, was transferred to Los Alamos, to aid in the final stages of the work on the bomb.

A by-product of secrecy and compartmentation was the change in some of the normal traditions of publication. So few scientists were permitted to read a given report that there was no need for more than about one hundred copies of any report. Most of the copies remained on the site of origin to be read by the workers in the original and in some of its neighboring compartments. The rest went to civilian and military administrators, leading scientists at other sites, and to a few central repositories where they could be read only by running a gantlet of compartmentation rules. Coupling the wartime emphasis on speed with the small number of required copies, there seemed to be little reason for the better methods of reproduction. Consequently, the average report was reproduced in slovenly fashion by Ditto or Mimeograph machines.

More important than these features of reproduction and distribution was the fact that the writing and editing of the report bore little resemblance to normal practices. A scientist is traditionally meticulous in the presentation of his paper in journals that are to be read by his colleagues and by later generations of scientists. If the quality of writing of a particular article is poor, editors enforce revisions. During the war, however, even the scrupulous among the scientists often wrote sketchy or poorly organized reports. As a result, information was irretrievably lost—information which was familiar to the small group who performed the experiments or the calculations, but which was not effectively transmitted to others. In the opinion of many scientists, their results were often crude approximations of the polished quality of the prewar work; hence, the description of these results, they felt, did not warrant polishing. Also, they realized that their reports were read mainly by a small group of equally harried colleagues. Under such circumstances, attempts at editorial revision were, to say the least, inadvisable.

Within a brief two or three years, the twin policies of secrecy and compartmentation wrought a startling change from the free international exchange of information of prewar days. If a scientist lived outside of Germany, the United States, England, or Canada, he was not aware of any substantial progress in nuclear science. Within these few countries, only those scientists employed on the secret atomic bomb projects were permitted access to the results of wartime researches; even for this select group, compartmentation reduced the amount of information made available to any individual.

In this country, the prewar nuclear physicist, whether he worked on some secret weapon, such as radar or the proximity fuse, or whether he remained at his teaching post, was not able to participate in the formulation and solution of such problems as diffusion theory, the construction of piles, the discovery of the transuranic elements and their isotopes, the accurate measurement of neutron cross sections, the details of the fission process, and the other branches of the field which were initiated or radically transformed behind the curtain of secrecy. In effect, this hiatus in their own research and the lack of access to the results of the research of others produced a "have-not" class from a group of former "haves." As a corollary, the scientists within the projects, whether old-timers or youngsters, obtained a temporary monopoly on the concepts and techniques in this field, a monopoly maintained as long as normal publication remained suspended.

The effect of this monopoly on the professional lives of individual scientists is, of course, trivial compared to its consequences for the well-being of nuclear science in our country. For secrecy is two-edged. It means withholding information from potential or actual enemies of our country. But it also means that the vast majority of physicists, chemists, biologists, engineers, and industrialists in this country are kept in ignorance of the advances in their special fields, and are thus prevented from the participation necessary for our own maximum progress.

After the disclosure of the bomb at Hiroshima and Nagasaki, and the termination of the war, it was clear that the maintenance of complete secrecy was neither possible nor desirable. Information had to be released to the people of the United States, to its scientists, engineers, and industrialists, to its congressmen, and to the peoples and political leaders of other countries. Some of this information was published almost immediately, partly in the form of announcements by high gov-

ernment officials, but mainly through the publication of the Smyth Report—*Atomic Energy for Military Purposes—The Official Report on the Development of the Atomic Bomb under the Auspices of the United States Government 1940–1945.*

The Smyth Report is still the most comprehensive single volume describing the work of the atomic-bomb projects during the war years. It made no pretense at being a complete account of the technical problems and their solutions; no details of the production plants or of bomb construction are presented; of the various scientific phases of the program, only some are treated, and these in general terms. It was, however, the first publication following a long silence, and because it gave a coherent over-all picture of the objectives and of the vast extent of the projects, the initial effect of the Smyth Report was startling even to the compartmentalized project scientists.

For about a year, the Smyth Report remained the only major technical publication in the field of nuclear science. Further publication awaited the study and establishment of criteria for releasing or withholding nuclear information. By 1946, the first formal statements of these criteria were embodied in two documents, the Atomic Energy Act and the Declassification Guide. The Act declares that the Atomic Energy Commission must "control the dissemination of restricted data in such a manner to assure the common defense and security." The term "restricted data" is defined as all data "concerning the manufacture or utilization of atomic weapons, the production of fissionable material, or the use of fissionable material in the production of power, but shall not include any data which the Commission from time to time determines may be published without adversely affecting the common defense and security."

Any broad, legal definition of "restricted data" must perforce be ambiguous, and no inflexible definition, even if originally precise, can keep pace with new discoveries. In practice, the interpretation of the term "restricted data" must be made by the Atomic Energy Commission. Not only does it administer the Act, but, in its intimate contacts with scientists, it is in the best position to devise flexible and practical solutions to the problems of secrecy and publication.

When it took office, the Commission inherited a set of rules and an administrative structure by which it could decide which information to withhold and which could be released for publication. These rules were originally laid down by the Tolman Committee, which included some of the leading project scientists, and were then formalized as a Declassification Guide. At present, the Guide, which has already undergone two revisions, lists more than fifty topics which must remain secret, and a like number of categories about which information can be released.

If the author of a technical report, or an official of his institution, wished to have his paper released for publication, it was submitted to a hierarchy of reviewers who rendered a decision based on the rules of the Declassification Guide. By applying this procedure to individual papers over the course of the past three years, almost 3,000 technical reports have now been "declassified." A goodly fraction of these reports have been submitted to, and accepted by, the scientific journals; the rest are being made available for sale as separate reports, or are being collected for publication in the volumes of the projected "National Nuclear Energy Series." From the contents of the declassified papers there emerges, for the first time, a fairly comprehensive picture of the purely scientific work of the atomic-bomb projects since 1940.

A major element of the program of publication of wartime research is the "National Nuclear Energy Series." This is to be a collection of about one hundred and twenty volumes, half of which will be published; the rest will remain secret. At present, only one volume has appeared. It is expected that the sixty publishable volumes will appear over the course of the next few years. Unfortunately, because of the many thorny problems of declassification and the shift in the interests of scientists at the close of the war, the rate of preparation and publication of this series has been much slower than the parallel program of the Radar Project, in which almost all of a planned set of about twenty-five volumes have already appeared.

The lag in the publication of the volumes of the NNES has led to some serious gaps in our present understanding of the wartime advances in nuclear science. To take one example: during the war, the radiochemists studied, in great detail, the characteristics of the many isotopes making up the fission products. These researches were summarized in hundreds of project reports. Practically none of these have yet appeared in print, almost four years after Hiroshima. This delay is not due to security reasons; it is merely the result of a decision to publish all the hundreds of reports simultaneously as volumes of the NNES. Contrast this with the case of nuclear physics. Here scientists decided that, irrespective of the planning of the NNES, it was imperative for rapid resumption

of research to declassify individual papers as quickly as possible. Today research in the academic portions of nuclear physics has reached a fairly normal stage in both AEC and university laboratories.

Parallel with the problem of the publication of wartime research is the treatment of postwar research in nuclear science. With the cessation of hostilities, nuclear scientists surged back to the universities, where research was swiftly resumed. But the increased cost of the tools of nuclear research, coupled with an unprecedented decrease in private endowments, led universities to rely heavily on government support. Hiroshima had taught government that the support of nuclear science was of paramount practical importance; and after the war the armed service branches of government were in the best position, budgetwise, to offer this support. It seemed ironic, at first sight, that the scientists who had been restive under wartime control by the Army Engineers Corps found themselves working at their universities with funds obtained from the Office of Naval Research. In reality, this situation was ironic for an opposite reason. Because of the enlightened attitude of the ONR, the nuclear scientists in the universities worked under a free publication policy, whereas their colleagues, under the civilian Atomic Energy Commission, were still struggling with the publication restrictions inherited from the Manhattan District.

This situation has now been ameliorated by the establishment of "unclassified areas" of research. This is the most important advance in the return to normal publication of nuclear data. Previously, declassification was confined to individual reports; now all reports in an entire field, an unclassified area, can be published without submitting them to the declassification procedure. The effect of specifying unclassified areas of research is that the scientists of the atomic energy projects who work on subjects of purely academic interest now have the same freedom of publication as their colleagues doing identical work in the universities. There remains one important difference: the AEC scientist is not permitted to work on even unclassified research without prior security clearance. Our universities have not placed such restrictions on pure research in nuclear science, even when the financial support derives from the military branches of the government such as the ONR.

To summarize, at present a paper in nuclear science may fall in one of three general classes:

1. Unclassified. Research in these subjects can be conducted in AEC or nongovernmental laboratories, without secrecy restrictions; the reports of these researches can be published in normal fashion.
2. Declassifiable: Work conducted under security restrictions; reports may be published after scrutiny according to the Declassification Guide.
3. Classified: Research specified as restricted by the Atomic Energy Act; therefore, results cannot be published.

As illustration of these three classes, we extract the following partial listing* for each class from the latest report of the AEC to Congress:

Unclassified areas. In general, the unclassified areas cover the pure science related to atomic energy, but not plant processes or specific experimental data of vital project importance. Included are:

1. Pure and applied mathematics, except that applying to specific classified projects.
2. Theoretical physics (except the theory of fission, of reactors, and of neutron diffusion, and weapon physics)
3. All physical (except nuclear) properties of all elements of atomic number less than 90. Nuclear properties of most isotopes.
4. The basic chemistry of all elements (except for the analytical procedures and technology of the production of fissionable materials) and the physical metallurgy of all elements of atomic number less than 83.
5. Instrumentation, including circuits, counters, ionization and cloud chambers, neutron detectors (excluding fission chambers), electronuclear accelerators, such as cyclotrons, betatrons, Van de Graaff generators, etc.
6. Medical and biological research and health studies (excluding work with elements of atomic number 90 and above).
7. Chemistry and technology of fluorine compounds (except the specific applications in AEC installations).

Declassifiable information. The declassifiable information which may be expected to be found in the general literature after official declassification includes:

1. Most reactor and neutron diffusion theory, except for those parts involving semiempirical methods or related to specific assemblies.
2. Certain physical properties of isotopes of elements of atomic number greater than 90, and the nuclear properties (except for certain neutron and fission characteristics) of isotopes of elements greater than 90.
3. Analytical procedures (except for production applications); most physical and process metallurgy of elements of atomic number greater than 90.
4. Medical and biological research and health studies with elements of atomic number 90 and above.
5. Certain properties of experimental reactors, such as: fluxes, neutron distribution not revealing lattices and

* These lists of topics are indicative only. They are not an exact statement of types of data which are unclassified, classified, or declassifiable. The complete lists are themselves classified. All who write, or speak, on atomic energy, and who are uncertain about classification status, should inquire of the Declassification Branch, United States Atomic Energy Commission, Washington, D. C. They should not try to make their own evaluation on the basis of these partial listings.

information regarding thermal columns, and the velocity spectrum in the thermal column.

Classified information The types of information which are clearly classified information include

1. Information on the production of fissionable material—equipment used, technology, handling, and disposition—including the technology of production of feed materials—and specifically all quantitative and qualitative output data.
2. The technology of production and power reactors, including design, operating characteristics, and working materials.
3. Information dealing with nuclear weapons and their components, including production technology, handling, disposition, testing, and technical data relating to military employment.
4. Certain information relating to the operations and facilities of the United States atomic energy program which may be of value to an enemy in sabotage planning, or in studies of the strategic vulnerability of the United States, or defense potential of the United States with respect to atomic weapons.

It is often overlooked that an important factor in the release of technical information is the widespread demand for popular information on atomic energy. Thus, the AEC must not only decide what technical information may be released to the scientists, but it also must reformulate this information on various levels of complexity, directing it at such specific groups as the personnel of the national military establishments, the industrial executive, the high-school science teacher, the member of national civic organizations like the League of Women Voters, the newspaper reporter and editor, and the "average" citizen. It is as a result of the many pressures exerted by such groups that the definition of restricted data is undergoing constant revision.

In this paper we have been concerned primarily with the restrictions of the technical literature as seen by the scientists. And, in the popular mind, it is the scientist who protests against the hobbling effects of secrecy.

During the past year, the principal pressure for more release of technical information has come not from the scientist but from the military and the industrialist. If the definition of "restricted data" were applied literally, the armed services would be completely stymied in their military employment of atomic energy. Imagine the effect of the cumbersome FBI investigations if they were required for every person participating in such huge operations as those at Bikini or Eniwetok. To cope with this unwieldy situation, the Commission has appointed a Weapons Effects Classification Board. The Board has now devised procedures for the dissemination of information on military use of atomic weapons to armed service personnel without the security provisions which apply to technical personnel of the AEC who actually deal with data concerned with the "development or manufacture of fissionable material and weapons." A notable forward step in the short history of declassification is the current preparation of a weapons-effects handbook, to be distributed for use in training military personnel, and to be made available to the public.

The dissatisfaction of industry with the present publication program of the AEC has been summarized in the recent report submitted to the Commission by its Industrial Advisory Group. The need, in this case, is for different forms of presentation of material as well as the release of more information. The engineers and the industrialists of the country, except those who have participated as contractors or consultants of the Commission, feel that they are being kept in ignorance of information most useful to them. If we are to have an atomic-power industry within this generation, more engineers must be trained in the relevant branches of nuclear science, as well as be apprised of the engineering advances made within the projects. Further, industrialists must have sufficient knowledge of the details of the Commission's operations to permit them to ask those questions which will elicit the data they need in order to decide in what way they may profitably participate in the industrial aspects of the program. In the words of the Advisory Group,

> The essential precondition to increased industrial participation is knowledge of the subject so that industry may recognize opportunities to take part as they arise. Much information has already been published by the Commission, but most of it is not in a form that is useful to industry. In addition, a vast amount of nonsecret information, of potential value to industry, is buried in the files and activities of the Commission. In connection with certain other information, still classed as secret, the continuance of secrecy is of doubtful value. The Commission or groups acting under its auspices should organize and clarify already published material, and issue reports on it in a form useful to industry. In the same way, the Commission should publish in useful form the nonsecret but as yet unpublished information. Still secret information, which is properly declassifiable and of special interest to industry, should be declassified and published.

What are the postwar policies of other countries on the publication of information in atomic energy? This is likely to be one of the knotty questions of the next decade. In some countries like Holland, Italy, and Switzerland, there were no war projects, and there are, at present, no plans for piles or separation plants. In the postwar literature of these countries, papers on nuclear sci-

ence have reappeared; but no discussions of "secret" subjects are published.

England and Canada collaborated with the United States in the secret wartime work on atomic energy. In England, Parliament has enacted laws that are essentially similar to our own Atomic Energy Act in their legal restrictions on the dissemination of information. The Ministry of Supply, the counterpart of our AEC, has amplified and implemented the laws in order to permit research, development, and production within the Ministry, as well as nuclear research in the universities, to be carried on in a tolerable fashion. As far as war research goes, declassification policy is identical in England, Canada, and the United States. In fact, revisions of the Declassification Guide have been made at the joint three-power conferences. It is, however, difficult to predict England's policy with respect to information discovered independently of the joint war effort. As technical information from the operation of the piles at Harwell and at Chalk River accumulates, England and Canada will have to face and solve the same sort of publication problems with which the United States has been grappling. Will England's publication policy remain identical with ours in the future? The answer to this important question will undoubtedly influence our own publication policy.

The case of France is even less clear. She has just announced the completion of a research pile, and the plans for the construction of a second. Some of her scientists working in this field have had no direct access to the classified documents of the British, Canadian, and American war projects, and presumably have unearthed nuclear data independently. Others played an active role in certain branches of the British project, and were intimately associated with the work on the Canadian pile. Furthermore, the head of the French AEC, Joliot, is a prominent member of the French Communist Party. At this writing, in March, the first reports published on the French pile read much like the declassified accounts of the British and U. S. piles.

Perhaps the most interesting, and certainly the most difficult, question is that of Russia's publication policy in atomic energy. The difficulty is partly due to the linguistic barrier that must be hurdled by the average American even to understand the titles of the articles now being published. A further difficulty lies in the fact that most of our libraries lack complete files of Russia's wartime and postwar journals. But the main problem arises from the atmosphere of the cold war, which makes objective study almost impossible.

A summary glance at the titles in the Russian journals reveals a few simple facts. Between the discovery of fission and Russia's entry in the war, articles on such topics as fission products, spontaneous fission, and some general treatments of the chain reaction appeared in their periodicals. Although not nearly so numerous as the articles of the same period in *Physical Review*, the Russian papers exhibit the same enthusiasm for work in fission as that of other countries. During the war, because the issuance of journals was extremely infrequent, as was to be expected from a country so hard-hit, nothing was added to our knowledge of the Russian work in nuclear science.

Since the beginning of 1947, the Russian journals have been issued with a regularity exceeding that of German and French periodicals. How is nuclear science treated in the pages of these journals? In the field of cosmic rays there is a spate of articles. Occasionally, papers appear on subjects like artificial radioactivity, nuclear reactions, and theoretical nuclear physics. But topics like the fission process, the chain reaction, the theory and construction of piles, the separation of isotopes, are conspicuous by their absence. When one couples this with the fact that some of the members of the German atomic war projects, with certain knowledge of these subjects, are now in Russia, it is reasonable to assume that publications are being withheld. The pattern in Russia is much like that in Germany and in the United States a decade ago, as described earlier in this article. But the suppression of publications in Russia and the very cautious release of information of a purely academic nature in the United States are merely two different aspects of the same fundamental fact —the existence of an atomic arms race.

In this country we are beginning to enjoy a considerable measure of our prewar publication freedoms. And it is reasonable to expect further expansion of the known areas in nuclear science in response to peacetime demands of scientists, engineers, industrialists, legislators, and lay citizens here and abroad. It is clear, however, that the literature of atomic energy in the coming decade will be critically influenced by the temperature fluctuations of the "cold" war and by the intellectual level of our foreign policy.

RADIO ASTRONOMY

CHARLES R. BURROWS

Dr. Burrows (Ph.D., Columbia, 1938) is director of the School of Electrical Engineering at Cornell University, where extensive research is being done in the new science of radio astronomy. He has been a member of the National Television Systems Committee and of several radio engineering committees working on problems of wave propagation, transmission lines and antennas, and multiplex transmitters

RADIO astronomy, like the older branches of astronomy, is concerned with the measurement of extraterrestrial electromagnetic radiations, but the frequency range and experimental techniques are different. Radio astronomy gets its name from the fact that the measurements are made at the same frequency range as that used for radio communication. The experimental techniques are the same as those of the radio engineer, so it is natural that experimenters in this new branch of astronomy should be radio scientists

Early astronomical observations were made with the naked eye. The sensitivity and positional accuracy of the observing apparatus were greatly extended with the invention of the optical telescope by Galileo. The frequency range of the electromagnetic radiations observed was extended slightly on either side of the visible by the use of photographic processes involving emulsions sensitive to either the utraviolet or the infrared. The use of thermocouples and photoelectric cells with the same type of telescope makes possible measurements further in the infrared. By using all these methods it is possible to make measurements over a ten to one frequency range (a decade) that includes the visible frequencies.

Observations in radio astronomy extend over a frequency range of approximately 3.5 decades from 10^7 to 3×10^{10} cycles per second (a wavelength range from 30 meters to 1 centimeter). The experimental techniques are applicable at frequencies both lower and higher than these, but absorption by the earth's atmosphere effectively isolates us from electromagnetic radiations outside this window. At lower frequencies the ionosphere (ionized region of the upper atmosphere) effectively shields the earth from the outside, and at higher frequencies absorption by molecules of oxygen, water vapor, carbon dioxide, and methane is substantially complete. This molecular absorption extends for a range of about three decades, beyond which measurements can again be made by thermocouples.

Equipment for observations in radio astronomy looks more like a radio receiver or a radar set than a telescope, although the receiving antenna for measurements on the higher frequency may be put on the same type of equatorial mounting as that used for the larger optical telescopes, as shown in Figure 1.

The antenna and electronic equipment associated with a radio telescope are similar to those used with a radio receiver, but the frequency spectrum and direction of arrival of the signal are different. The signal received on a radio telescope extends over a wide frequency range—in fact, it has many of the properties of thermal noise and sounds like thermal or set noise if the electromagnetic vibrations are converted to sound by a loud speaker. For this reason the signals are called solar noise or galactic noise, depending upon whether their origin is in the sun or elsewhere in our galaxy. Because the received power is spread over the entire frequency spectrum instead of being concentrated near the carrier frequency of a communication signal, it is measured in watts per cycle rather than watts, and many of the devices, such as electrical filters which aid in improving the sensitivity of communication receivers, do not perform the same function for a radio telescope.

The nature of the signal received by a radio telescope also places much more importance on the directivity of the receiving antenna than is usual in a communication system. Galactic noise is more intense near the center of our galaxy in the constellation of Sagittarius, but it comes from all directions, so that the signal is measured in watts per cycle per second per steradian. This requires an accurate knowledge of the directional characteristics of the receiving antenna used for the radio telescope. Because of the extremely large aperture needed to obtain even moderate directivity, it is necessary to solve an integral equation to determine the signal received from any particular direction.

The power received by a radio telescope may be expressed as:

$$P = \int I(x,y,z,\theta,\phi,\nu,\rho,t)\ G(\theta,\phi,\nu,\rho)\ F(\nu) \sin\theta d\theta d\phi d\nu$$

where I is the specific intensity of the incident electromagnetic radiation. It is a function of the

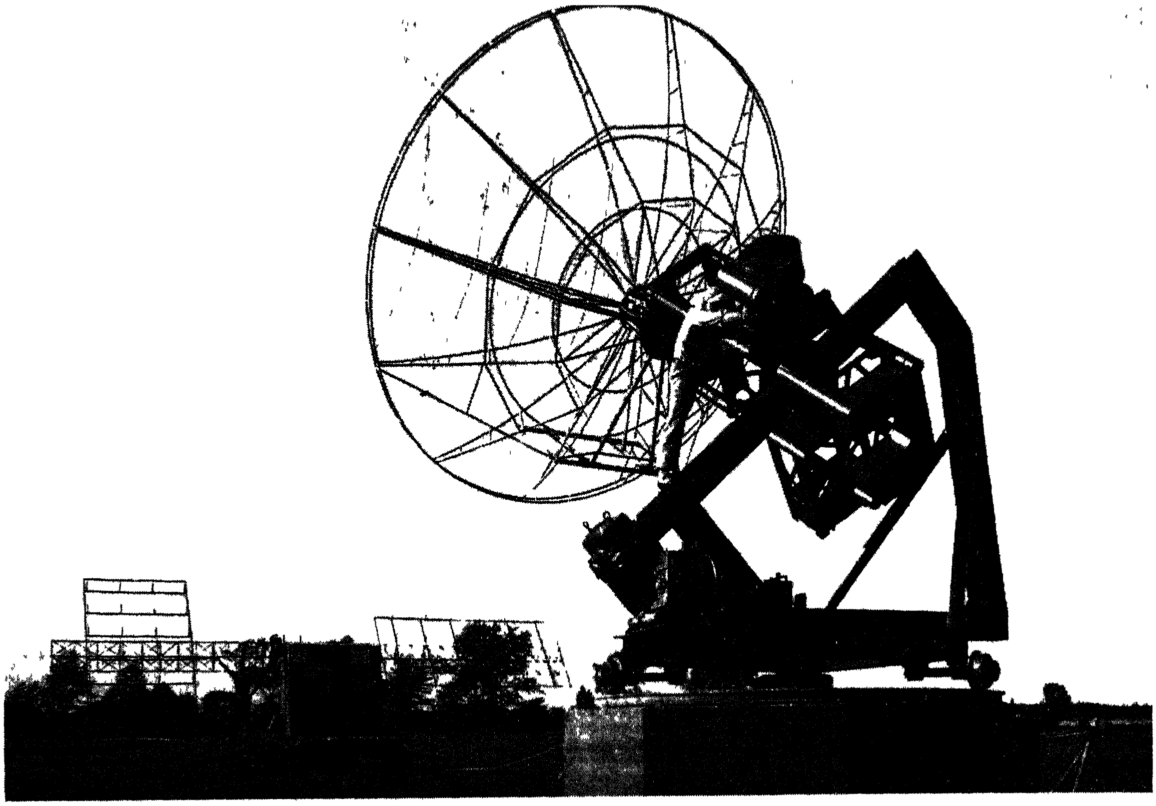


FIG. 1. Radio telescope nearing completion at Cornell University. The 17-foot saucer-shaped radio antenna will collect radio waves from 5 to 60 inches in length.

position or location of the radio telescope (x,y,z), the direction of the source of the radiation (θ,ϕ), its frequency (ν), polarization (p), and usually the time (t). The contribution of the radiation to the output of the telescope depends upon the directional characteristics of the antenna $G(\theta,\phi,\nu,p)$. This is not only a function of the direction (θ,ϕ) but of the frequency (ν) and polarization (p). The dependency on frequency is usually avoided by making the frequency response of the telescope such as to limit the response to a band of frequencies sufficiently narrow that the antenna gain and directivity are substantially the same over the entire band. The response is proportioned to the solid angle $(\sin\theta d\theta d\phi)$ and the frequency interval $(d\nu)$. The telescope measures the value of this integral, whereas the desired quantity is the specific intensity, I, which occurs under the integral sign. By designing the telescope so that all the variables are independent of frequency throughout the band-width for which there is a measurable response, the integration of $Fd\nu$ may be made separately, giving the band-width of the telescope, B. This leaves the following integral to be solved:

$$P = B\int I(\theta,\phi)G(\theta,\phi)\sin\theta d\theta d\phi.$$

Because of the fact that electromagnetic radiation is polarized and radio antennas show a preference for a particular polarization, this equation must be solved twice. If the response is found to be a function of time, further complications result.

If the source of radiation subtends an angle that is so small that the response of the antenna is substantially constant over this angle, then the integration with respect to angle may be performed to give

$$P = I\pi R^2/r^2,$$

when R is the radius of the source, r is the distance to the source, and $\pi R^2/r^2$ is the solid angle subtended by the source. For many radio telescopes the sun subtends such a small angle, so that data on solar noise are more readily reduced to specific intensity than data on galactic noise, provided the measurements are made under conditions for which the background galactic noise does not contribute appreciably. Accordingly, solar noise will be considered first.

The spectrum of electromagnetic radiations from the sun as received at the earth's surface is given in Figure 2. The fine curve gives the radiation that would be received at the earth if the sun were a black body at 5,713 degrees absolute and there were no absorption in the earth's atmosphere. The integral of this curve gives the correct total radiation received at the earth. This equation

$$I = \frac{2h\nu^3}{c^2(e^{h\nu/kT}-1)} \longrightarrow \frac{2k}{c^2}\ T\nu^2$$

gives a relationship between specific intensity, I, and apparent temperature, T. Here h is Planck's constant, k is Boltzman's constant, and c is the velocity of light. The expression at the right, which applies at radio frequencies, gives a particularly

simple relationship between specific intensity and apparent temperature. Since the unit of specific intensity is somewhat difficult to visualize, the term "apparent temperature" is widely used. It does not imply that the source is in temperature equilibrium, but merely that the specific intensity in the frequency interval under consideration is the same that would be produced by a black body at this temperature.

In the visual frequency range the sun radiates as a black body at a temperature of about 6,000 degrees. At higher frequencies in the ultraviolet, the solar radiation is somewhat reduced because of absorption in the solar atmosphere. At frequencies above 1.5×10^{15} cycles per second, the ozone layer in the earth's atmosphere acts as an effective shield. At the infrared side of the visible range, the sun radiates as a black body at about 7,000 degrees, but the radiation received at the earth's surface is greatly reduced by the absorption bands of carbon dioxide, water vapor, and methane. At frequencies below about 10^{13} cycles per second, atmospheric absorption is substantially complete. At still lower frequencies, microwave radio techniques allow the observation of the sun through the absorption caused by atmospheric gases. Radio astronomy makes measurements through the window extending from 3×10^{10} to 10^7 cycles per second, at which frequency the conductivity of the ionosphere of the earth again acts as an effective shield.

At the high-frequency edge of this radio frequency window in the earth's atmosphere, the apparent temperature of the sun is only slightly greater than that in the visible frequency range. At the lower frequencies, however, the apparent temperature of the sun increases, until at frequencies below about 3×10^8 cycles per second, the apparent temperature of the sun is of the order of one million degrees absolute. This increase in the apparent temperature of the sun is a natural result of the solar ionosphere. The mean kinetic temperature of the electrons in the solar corona which make up the solar ionosphere is believed to be about a million degrees, from spectroscopic measurements The solar ionosphere is opaque to the lower radio frequencies, so that the solar noise at these frequencies originates in the solar corona rather than in the photosphere. Accordingly, the apparent temperature of the sun at the lower frequencies is that of the solar corona rather than that of the photosphere.

In addition to this radiation from the quiet sun, bursts of solar noise upwards of a thousand times this value have been observed at the lower radio frequencies. Figure 3 shows some samples of the records of solar noise at a frequency of 205 megacycles per second obtained at the Cornell Radio Observatory. The upper chart shows the noise from a quiet sun. This is the minimum value of solar noise measured at this frequency. In the period represented by the center diagram there were several bursts of solar noise to values three to six times the quiet sun. During the period represented in the lower record the general level of the

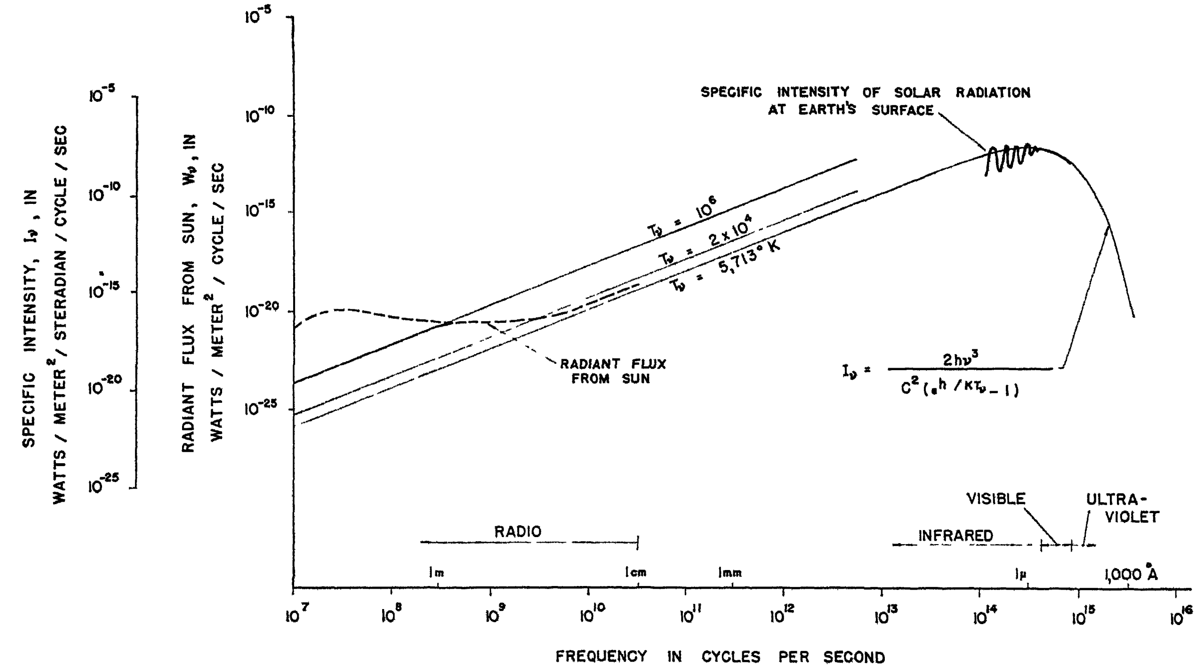


FIG. 2. The spectrum of the sun as measured at the earth's surface.

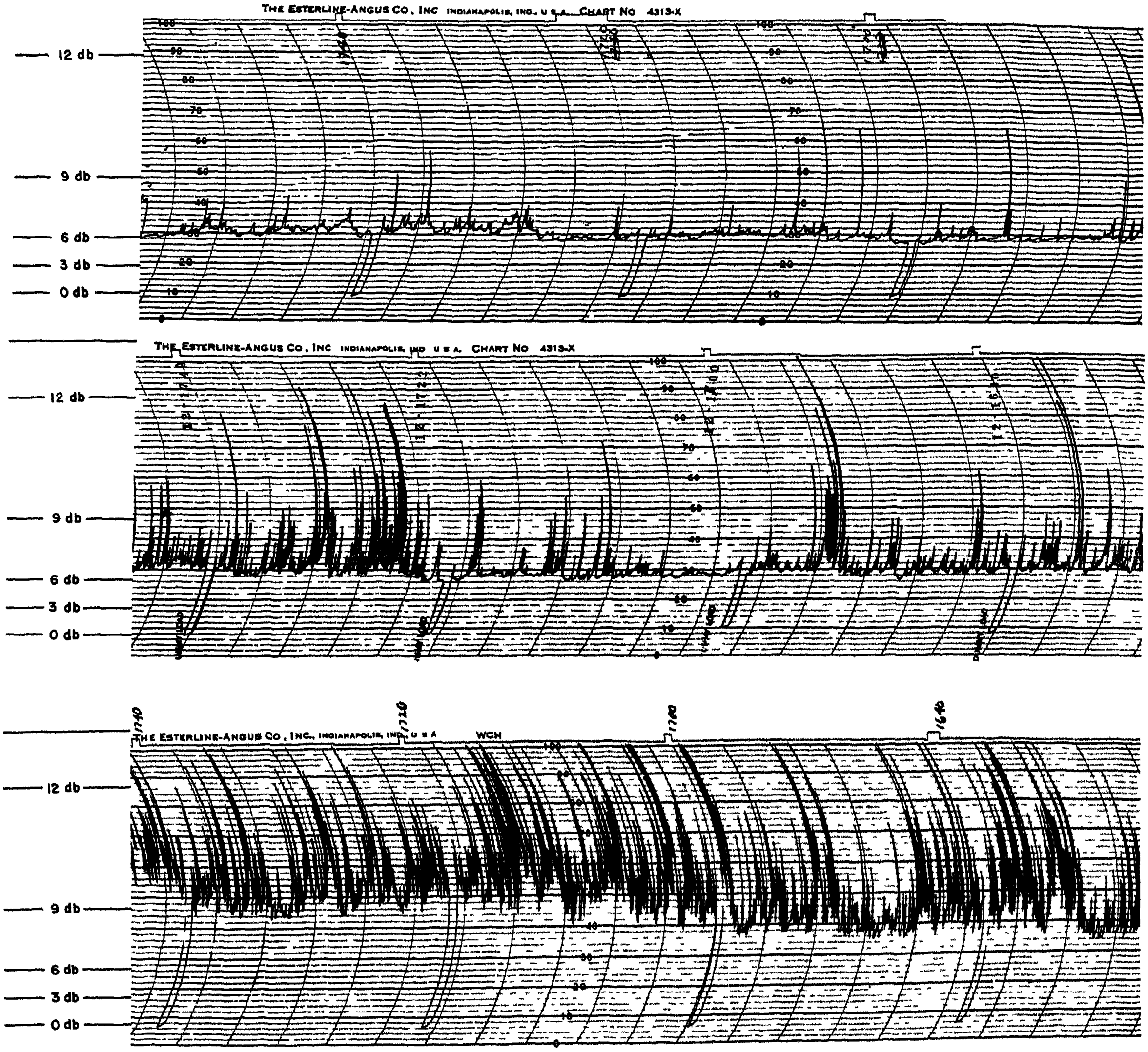


FIG. 3. Solar noise records. The horizontal interval is approximately four minutes. Upper chart is for the relatively quiet sun; center, bursts of solar noise superimposed on the quiet sun level; and, below, bursts superimposed on enhanced noise level.

solar noise was enhanced to about three times the value for a quiet sun, and fluctuated rapidly.

Enhanced solar noise is related to sunspots. Some measurements have shown that enhanced solar noise is circularly polarized, as would be expected if it were produced by electrons moving in circular orbits around the magnetic field of the sunspots. Other measurements have shown the source of enhanced solar noise to be in the immediate vicinity of large sunspot groups.

Measurements made during a solar eclipse suggest that the source of enhanced solar noise is to be found in the prominences and flocculi. The area of uneclipsed prominences and flocculi showed good correlation with the intensity of enhanced solar noise. During the time of this eclipse the prominences increased the effective diameter of the sun at 200 megacycles to 1.35 times the diameter in the visual frequency range.

It is well known that there is an intimate relationship between disturbances on the sun and radio wave propagation conditions. It may well be that a study of solar noise will provide a better means of predicting long-distance radio communication conditions. Besides giving us another tool for observing solar activity, the measurement of solar noise is actually the measurement of a radio wave that has been propagated through the entire ionosphere. This provides us with a means for measuring the ionosphere above the height of maximum density, which of course is not possible by the usual ionospheric sounding measurements. Hence solar noise measurements may allow us to learn more about the earth's atmosphere.

Measurements of galactic noise may result in a greater contribution to astronomy than measurements of solar noise. The stars of our galaxy, of which the sun is one, are concentrated near the plane of the Milky Way, with a denser spherical concentration at the center of the galaxy in the direction of the constellation Sagittarius. Besides the stars, there are interstellar molecules and dust particles which in the aggregate absorb so much energy at visual frequencies that there is some question whether it is possible to observe the center of the galaxy with optical telescopes.

Measurements of galactic noise on three frequencies are shown in Figure 4. The abscissa gives the galactic longitude, or the angle around the plane of the Milky Way. The ordinant gives the galactic latitude of the angle from this plane. The specific intensity is shown by contours. The numbers on the contours indicate the number of decibels the specific intensity is above 10^{-21} watts per square meter per steradian per cycle per second. (An increase of 3 decibels in the specific intensity is equivalent to double the specific intensity. An increase of 10 decibels in specific intensity is equivalent to increasing the specific intensity tenfold.) At each of the three frequencies the major maximum is in the constellation Sagittarius at a galactic longitude of about 330°. The specific intensity in this maximum direction is roughly the same order of magnitude at each of the three frequencies. There is a secondary maximum in the constellation Cygnus. For the two higher frequencies, the curves represent the measured received power rather than the specific intensity, since sufficient data were not available to solve the integral equation. At the lowest frequency, however, the contours represent a solution of a differential equation giving the specific intensity. The contours on all three frequencies show the general distribution of intensity that is found at visual frequencies.

Perhaps the most startling result from the discovery of galactic noise is the observation of intense apparent point sources. These are shown in Figure 5. The point sources were found by interferometer measurements, either by using two antennas pointing at a high angle of elevation or by using an antenna at its image in the sea. By this means it has not only been possible to determine the position with good accuracy but also to determine an upper limit to the size of the source. The source in

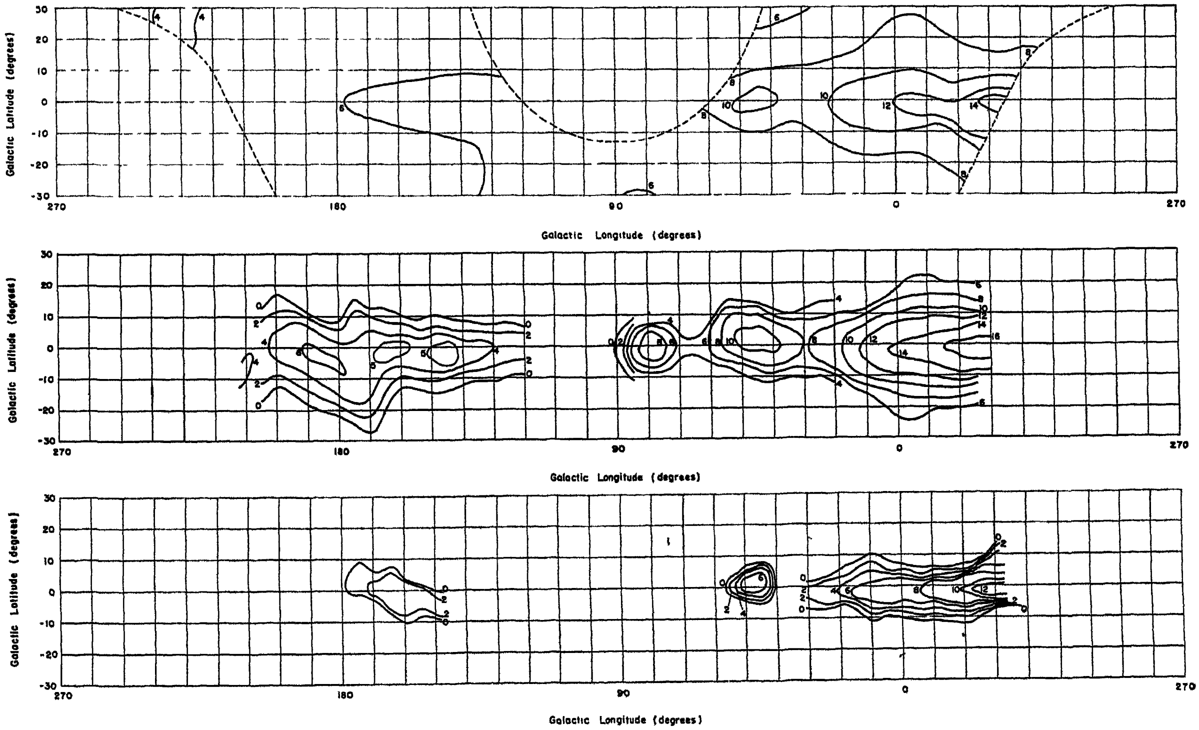


FIG. 4. Specific intensity contours of galactic noises plotted on galactic coordinates on (*above*) 64, (*center*) 160, and (*below*) 480 Mc. Numbers on curves give the specific intensity in decibels above 10^{-21} watts per square meter per steradian per cycle per second. Upper graph represents the solution of the integral Equation 2 by Hey, Phillips, and Parsons, using data obtained by them on a frequency of 64 Mc. The second and third graphs represent the received power obtained, measured by Reber with his equipment on 160 and 480 Mc, respectively. In constructing these two diagrams, advantage was taken of a careful analysis of Reber data made by Miss Ruth Northcott, of the David Dunlap Observatory, to determine the galactic pole, which was kindly made available to the writer.

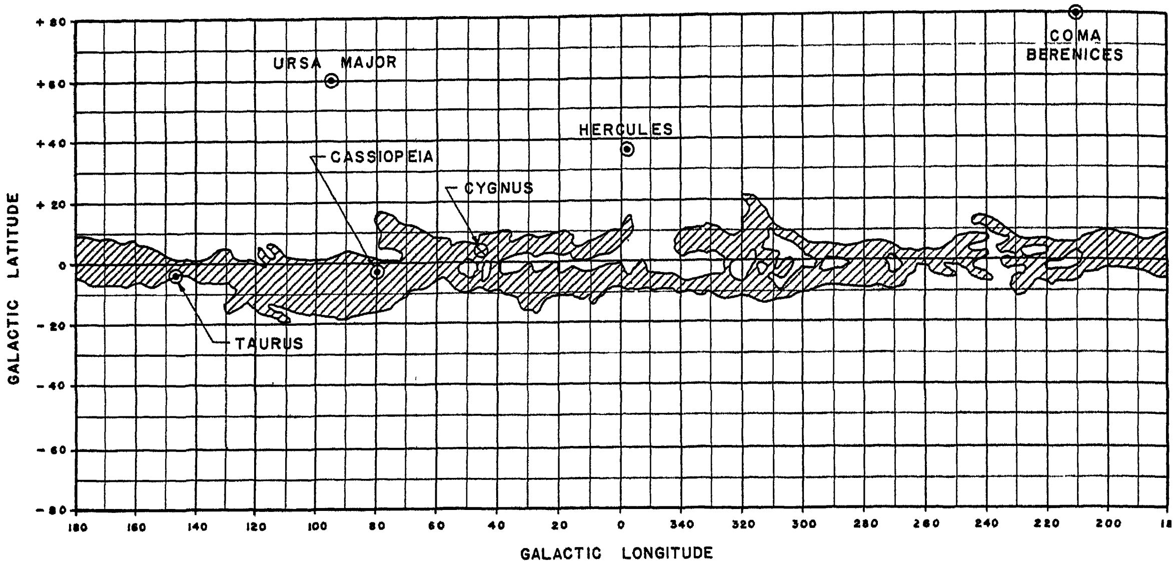


FIG. 5. Point sources of galactic noise: ⊙, constant source; ⊕, variable source.

Cygnus is smaller than the limit of measurement, which is approximately eight minutes of arc in diameter. The most startling thing about these point sources is the fact that it has been impossible to observe anything that might be identified with them at either the visual or infrared frequencies. Apparently, the radio astronomers have observed something that cannot be seen with the ordinary telescope. Although some of these point sources appear to be constant, others have been observed to show marked variations with time. An unexplained fact about these point sources is the intensity of the galactic noise received from them. Even if the entire solid angle which represents the maximum possible size of the source were completely filled with stars radiating energy of the same intensity as our sun under enhanced solar noise conditions, the total energy radiated would fall short of that observed by a factor of the order of one thousand. Also, the radiation from these point sources has a random polarization rather than the circular polarization found for enhanced solar noise. Clearly, these point sources represent some astronomical phenomena that have not heretofore been observed.

None of the hypotheses that have been proposed can explain all the observed phenomena of galactic noise. Undoubtedly some of the phenomena have one origin, and other phenomena have another. At present it is not possible to say whether the origin of galactic noise is to be sought in the stars or in the interstellar gas or dust particles. One hypothesis is that galactic radiation is caused when an electron approaches a hydrogen nucleus in a parabolic orbit. This is the process that is often referred to as the free-free transition of electrons in the field of a proton. Another suggestion is that the electrons in interstellar space are radiating as classical oscillators.

It is also possible that some of the galactic noise is the result of scattering of radiation from B-type stars by the electrons in interstellar space. A measurement of the polarization of galactic noise should help in determining the validity of this hypothesis, since the scattered radiation should have a preferred direction of polarization.

It may be that galactic noise is the result of the sum of radiation from many stars that are producing radiations of the type of enhanced solar noise.

There are many questions about both our sun and our galaxy that can be answered by the new field of radio astronomy. This branch of science is now at about the point where astronomy was when Galileo invented the optical telescope. Scientists in this new field are just asking themselves the pertinent questions that will be answered in the years to come.

POETIC SCIENTIST

CHARLES J. ENGARD

Dr. Engard (Ph.D., Chicago, 1938) has been on the faculty of the University of Hawaii for the past ten years and associate plant physiologist at the University Agricultural Experiment Station since 1947. He has published a study on Organogenesis in Rubus. *His present research is in the morphogenesis and growth hormones in sugar cane.*

Minds like Goethe's are the common property of all nations, and, for many reasons, all should have correct impressions of them —Thomas Carlyle

FOR more than half a century I have been known as a poet in my fatherland, and undoubtedly also abroad; or at any rate I have been permitted to pass for one. But the fact that I have busily occupied myself with Nature in all her general physical and organic phenomena and have constantly and passionately pursued seriously formulated studies—this is not so generally known; still less has it been accorded any attention."

Thus wrote Johann Wolfgang von Goethe a few years before his death in 1832. The greatness of the poet had blinded people to the greatness of the man of science.

The whole educated world is acquainted with *Faust, Werther,* and *Wilhelm Meister*; but little is known about an almost equally important phase of Goethe's life. This was his scientific life, stemming from, and maintained until his death by, a profound interest in nature. He performed a great variety of experiments, and recorded many observations on plants, animals, minerals, rock formations, light, and colors. Many of his observations were remarkably correct, and some of the conclusions he drew have had a marked influence in science, persisting down to the present time. The most important aspect of his scientific studies, however, was his method of approach and of analysis of biological phenomena. The method was one of comparison for similarities between things rather than of contrast for differences, and because of it Goethe was able to establish the science of comparative anatomy and to achieve the basic concept of organic evolution.

Two tendencies may be recognized in the thinking of men. One of these is the analytic, or separatist, kind of thinking, whereby fine differences between things are emphasized; the other is the synthetic, where fundamental similarities of things are ordinate, and the differences are subordinate. Goethe was a thinker of the latter kind, a fact that is superbly manifest in his creative writings. He was, furthermore, a lover of nature, and he had an unlimited curiosity concerning many aspects of natural phenomena. When his profound interest in nature led him to study problems in biology, his method of thinking soon set him apart from his contemporaries. For Goethe was not just a recorder and separator of facts, but was, in addition, a correlator and comparator of them. From his first experiments he began to see, or rather to sense, a unity of form among organisms, however variable the specific structures might be. He had the inherent feeling that in nature all objects are patterned after a form; that, for instance, a unity of structure or basic sameness prevails among plants and among animals. This idea was dominant in all his studies, and he sought confirmation of it in the accumulation and comparison of facts and observations. And thus he came close to formulating the theory of descent—the principle of organic evolution.

Goethe's first interest in biology was in osteology. He became a student of the anatomist Lavater, and progressed so quickly in his studies that he was able to aid informally in the work which led to the publishing, in 1776, of Lavater's *Physiognomische Fragmente.* His interest in botany was kindled when he moved to Weimar in 1775, where he accepted a political post under Karl August, the Duke of Saxe-Weimar. He became fond of the surrounding forests and meadows, and especially of the Duke's beautiful gardens. He was permitted to take up residence in the quiet and secluded *Gartenhaus* in the ducal gardens, and here he did much of his writing and embarked upon a lifetime study of nature.

He applied his comparative method of analysis first to the problem of the intermaxillary bone in man. This bone—the small center bone of the upper jaw, containing the incisor teeth—is separate and distinct from the other jawbones in animals exclusive of man. In man, the bone is fused left and right with the upper jawbone, and in most normally developed skulls there is no line of demarkation of one from the other. Anatomists of the day, finding no evidence of the intermaxillary bone, maintained that it did not exist in man, and one of them even considered the absence of this bone to be the one important mark distinguishing man from the apes. To Goethe, with his preconceived archetype of animal—the essence of his philosophy of

the unity of nature—such a distinction between man and the other animals could not exist. In the animals the intermaxillary bone held the incisors; man had incisors; therefore, according to his doctrine, man must have an intermaxillary. Goethe discovered the bone in the latter part of March 1784. Upon comparing the skulls of animals he noted that the bone varied with the nutrition of the animal and the size of its teeth. Also, in some animals the intermaxillary bone is not always separate from the jaw as it usually is. He discovered that the sutures were clearly evident in the skulls of children as compared with those of adults. Despite the importance of these researches, however, forty years passed before his discovery was fully recognized by anatomists; nevertheless, Goethe's method of comparing the structure of various animals in various stages of development was the beginning of the science of comparative anatomy.

Goethe's interest in the animal skull was rekindled during his visit in Italy in 1786–87, an interest that resulted in the development of a concept which has remained influential in the thinking of anatomists down to the present day. While he was wandering in a cemetery near Venice, Goethe came upon the skull of a ram which had been cut longitudinally. It occurred to him upon inspection that the face of the ram was composed of three vertebrae, and later he wrote: "The transition from the anterior sphenoid to the ethmoid was evident at once." Thus was born the vertebral theory of the skull.* This famous theory holds that the skull is composed of three or more vertebrae variously modified, but conforming to the same plan as those of the trunk. The theory was criticized by Huxley in 1858, and by others since then, and was gradually replaced by the current "segmental theory." This holds that the head is segmented like the trunk of the animal, the distinction between head and trunk being less marked in more primitive animals. The head region of the animal is now considered to be the result of the frontal segmentation of the body being modified and fused. The posterior portion of the skull is homologous with the segments of the spinal column. The vertebral theory, although greatly modified, has not been entirely discarded, a manifestation again of the debt modern science owes to the versatile mind of Goethe.

Goethe began his botanical studies at Weimar, attracted first to mosses, fungi, and algae. But later he became particularly interested in the cycle of development of the flowering plants, beginning with the germination of the seed and ending with the formation of the flowers. He began to read the works of the great contemporary biologist Linnaeus, and through this contact another source of stimulation was found. Busy as he was with his scientific and literary work at Weimar, his thoughts on the various subjects of nature did not begin to crystallize until he undertook his journey to Italy. His sojourn in that country was one of the most important episodes in his life, for there, enhanced by the climate and vegetative luxuriance of the land, his ideas of nature, especially in respect to plants, began to form.

All during his visit Goethe kept a diary in which he made notes of his observations of plants and recorded the variations in the structure of plants with variations in environment. He observed and made notes on germination, growth, and the formation of flower and fruit. He began to evolve the concept of plant metamorphosis, and returned to Weimar convinced he had found the secret.

In 1790, at the age of forty-one, Goethe published the results of his studies on plant development, the most famous and lasting of his scientific writings. The substance of this essay, entitled *Versuch die Metamorphose der Pflanzen zu erklären*, or *An Attempt to Explain the Metamorphosis of Plants,* which he later published under the title *Die Metamorphose der Pflanzen,* is now known as the Doctrine of Metamorphosis. During his Italian journey Goethe had written: "All is leaf, and through this simplicity the greatest diversity becomes possible." This means that the basic structure of the plant is the leaf—not necessarily the leaf one sees on any particular plant, but a leaf as an idea, or plan, from which all other parts are made, just as, for instance, the potter may have in mind not a real but an imaginary form of vessel, from which idea he makes a great variety of vases, pots, and urns.† Fundamentally, these receptacles are modifications or transformations of one type. (No matter what you choose to call them, all pots are basically alike.) From this archetype, or imaginary model, a great diversity of forms can be related. Goethe, who, a few years earlier, in trying to find similarity in the form of skulls discovered the intermaxillary bone, now evolved the doctrine

* Goethe did not publish has views on the theory but referred to it in letters and discussed it privately with friends. In 1806, the anatomist Oken, unaware of Goethe's ideas, independently arrived at the same conculsions, but he more thoroughly investigated the problem and published in the following year a dissertation on the subject. To Oken, therefore, is usually given credit for the theory, based on priority of publication, although occasionally Oken and Goethe are cited as co-originators of the theory.

† Goethe used the German word *Blatt,* which applies to the foliage leaf; but Goethe's *Blatt* is not just a leaf as commonly understood, but a leaf as a type or idea, and should perhaps have been designated by the word *Urblatt,* or leaf prototype.

of metamorphosis. This doctrine holds that all appendages of the plant—seed leaves, foliage leaves, sepals, petals, stamens, and the parts of the fruit—are variations of a fundamental form or model, the leaf. Furthermore, progressive change occurs in the form of these structures, as the plant develops from embryo to adult, beginning with the fleshy "unleaflike" seed leaves, or cotyledons, as the botanist calls them (the "halves" of the peanut or bean seed), and ending with seed pods.

Intermediate between these two extreme types of leaves are the foliage leaves (ordinarily understood as "leaves"); bracts, such as the leafy structures enclosing a bud; sepals, the reduced leaflike green objects at the base of the flower; the petals, often large, highly colored, usually obvious parts of the flower; and the stamens, bearing the pollen sacs at their tips. At the end of the series of modifications are the most highly transformed of the fundamental leaf. These comprise the fruit, and they may be fleshy or dry, simple or fused into groups. A good example of this kind of modified leaf is the pod of the pea plant. When the pod is split open longitudinally, and flattened out, there is before us, according to Goethe's concept, a leaf bearing the structures of reproduction—seeds—along the edges. The mature pod is, therefore, a leaf with its two edges folded together. After the stamens, the pistils are the "perfectest" type of leaf, whose pollination terminates one generation of plant growth and initiates the next. Goethe says:

> Whether the plant sprout, blossom, or bear fruit, it is always the same organs which, in various functions and with frequent changes in form, fulfill the dictates of Nature. The same organ which on the stem expanded as a leaf and assumed a highly diverse form, will contract in the calyx, expand again in the petals, contract in the sex organs, to expand again in the fruit.

This, in brief, is the doctrine of metamorphosis, now referred to by modern plant anatomists as the concept of homology. Every person who has studied elementary botany is acquainted with the theory to a greater or lesser degree.

One of the most important aspects of Goethe's comparative method of thinking might, if he had pursued it further, have made him one of the world's greatest biologists. This was his close approach to the theory of organic evolution.

The seeds of this theory, which did not attain complete formulation until the publication of Darwin's *Origin of Species* in 1859, were beginning to germinate in the mind of Goethe even earlier than they did in the minds of the more generally recognized forerunners of Darwin. For instance, Lamarck, noted for his theory of the inheritance of 1794, and did not formulate it articulately until 1809, whereas Goethe had given some expression to the thoughts as early as 1790 in his essay on plant metamorphosis, and had given a clear exposition of it in an essay on the subject, *Versuch einer allgemeinen Vergleichungslehre,* or *An Attempt to Formulate a General Comparative Theory,* in the early 1790s.

It was inherent in Goethe's approach to all living things that he compared them to seek more for similarities than for differences. This mode of thinking first became evident to him when he was studying the work of Linnaeus. From Linnaeus he learned much, but Linnaeus' approach to the plant kingdom did not satisfy Goethe. There was a fundamental difference between their concepts of the organization of the plant kingdom, for where Linnaeus was disposed to separate, distinguish, and classify the myriad plant forms as different and distinct species, Goethe sought always the basic similarity among those forms, and to fit plants and animals into an anatomical type. (Goethe had said: "Separating and counting was not in my nature.") The type, to be sure, was a hypothetical one, an idea, but one which enabled Goethe, and most botanists since, to understand parts that are different in appearance, but alike in plan, or related to the same ancestral form. From the versatility of this type Goethe says there are derived without exception the many genera and species known to us. The type still exists through all the transformations of form in plants and animals. In his *History of My Botanical Studies,* Goethe wrote:

> Through direct observation my attention was powerfully directed toward the circumstance that each plant seeks its opportunity and demands a condition in which it may appear in fullness and freedom. Mountaintop, deep valley, light, shade, aridity, moisture, heat, warmth, cold, frost, or whatever the condition may be, species and genera are necessary if plants are to grow with full strength and abundance. To be sure, in certain places and in many situations, they yield to Nature and allow themselves to be swept into variations, without giving up completely, however, the form and quality which they had acquired through their own efforts.

There can be little doubt that Goethe was a forerunner of Darwin. The discourse on the influence of environment on speciation refers to natural selection, and "form and quality—acquired through their own efforts" can be construed as hereditary (genetic) sequence. His thinking is phylogenetic, and his proximity to the modern theory of descent is evident in his writings. Goethe seems to have recognized the impossibility of distinguishing between the primitive plants and animals. Perhaps this was a result of his observations of microscopical plant and animal forms. He had stated:

rudimentary stage they are hardly to be distinguished . . . creatures gradually evolving as plants and animals out of a relation in which it is scarcely possible to draw a separating line between them, develop toward perfection in two opposite directions, so that in the end the plant culminates in a tree, enduring and stationary, while the animal reaches its highest degree of locomotion and freedom in its crowning representative, man.

Surely this is the modern theory of descent, simply stated. On another occasion Goethe said: "Who knows but that, after all, the complete man only indicates an aim at a still higher mark?" (In an essay on some fossil bones of an ox, Goethe referred to the "ancient creature" as the parent stock from which the common ox and zebra may be descendants.)

Finally, in his conversations and correspondence, Goethe often referred to the *Principle of Continuity.* Nature would not make a horse "if all animals did not precede mounting as by a ladder, to the structure of a horse." And in August 1796—two years after he wrote *Wilhelm Meister*—he wrote: "I am more than ever convinced that one can arrive at an excellent understanding of organic nature by means of the concept of continuity." It was impossible for Goethe to view living things in any other light than as a continuity of transformation, resulting in the production of a variety of structures based on a fundamental plan. In Goethe's writings one can find the same reasons for variations and transformations as set forth in the modern theory of evolution: adaptation, disuse of

But let us point up the concept with one more quotation: "Form is something mobile, something becoming, something passing. For the doctrine of formation is the doctrine of transformation. Metamorphosis is the key to the whole alphabet of nature." Darwin, a generation later, did not have an entirely new idea.

Another facet of the versatile mind of the German poet reflects his dual interest in the laws of nature and in art. What would be more natural than for Goethe to inquire into one of the crowning achievements of nature's "crowning representative"—the development of art? Goethe, by nature an artist, had made many accurate sketches and paintings of plants, rock formations, scenes, and had designed stage scenery. An enthusiast for Greek literature and art, he acquired a sizable collection of casts of Greek sculpture, and made excellent sketches of some of them. He was from youth always deeply concerned with painting, paints, colors, lights and shadows, and the effects these things produced upon people. He held many discussions with painter friends on the subject of colors, their mixtures, and their aesthetic effects, but his friends' ideas were vague and unsatisfactory to Goethe. He began to theorize on the nature of color and lights and shadows. He spent more time on this study than on any of his other scientific subjects, and wrote several essays on it. He performed a large number of experiments with prisms and simple optical equipment, and his observations were precise and thorough, although many of his conclusions were erroneous. He first advanced his ideas on light and color tentatively to the public in 1791 when he published his *Beiträge zur Optik,* or *Contribution to Optics.* When the theory was ignored by the physicists, Goethe merely was stimulated to greater efforts on behalf of it; in fact, he became very sensitive to criticism of the theory, and it became his greatest love. He once remarked: "As for what I have done as a poet I take no pride in whatever . . . but in my century I am the only person who knows the truth in the difficult science of colors,—of that I say I am not a little proud." Eighteen years after publication of his *Contribution to Optics* he produced the *Farbenlehre,* or *Theory of Colors.* The theory, so well documented in the publication, can be given only a brief, and therefore somewhat unfair, accounting here.

Because he was pressed to return some optical apparatus he had borrowed and kept for a long period, Goethe picked up a prism and looked through it at the white wall of his room. Instead

white wall was interrupted by opaque objects from which no light was being reflected. When he directed his sight toward the window, the frames showed some color, but not the sky. He immediately exclaimed: "Newton's theory is false!" This hasty conclusion, based on ignorance of Newton's principles and sound experiments, resulted in almost a lifetime devoted to overthrowing Newton's theory, when, in fact, he was fighting his own error. His first experiment was to view through a prism a white disk on a black background; it gave him the spectrum at the boundaries, as in Newton's theory. A black disk viewed against a white background gave a similar result. He concluded that color is not contained in white light, but is produced as a result of the mingling of light and darkness. He reasons further: How can white light be composed of colored lights when every color is darker than white? To obtain color there is necessary an intermingling of light and darkness; the color nearest the light is yellow, that nearest darkness is blue. Mix the two and green results. Blue tends to become red through violet, and yellow tends to become red through orange. These and many more observations and conclusions of interest convinced Goethe that colors are produced by mutual influence of light and darkness; the prism only served to move light and darkness over each other.

The fundamental error in Goethe's interpretation is this: whereas white light to him is homogeneous, Newton's is composed of many darker lights. As Newton had demonstrated in a series of simple experiments when he was a student at Cambridge, a beam of light from the sun, when passed through a prism casts a spectrum of lights of different wave lengths and corresponding colors. The red, or light of longest wave length, is refracted the least, violet the most. A second, reversed prism may be placed in the beam, and white light will emerge from the second prism. Goethe apparently did not perform this experiment, or he would have been convinced that white light *is* composed of "darker" colored lights. One other observation is of significance in relation to Goethe's theory; namely, that the source of light must be narrow, and the prism so placed in the beam (near the source) that the spectrum when cast upon a screen or wall will not be impure. If the beam of light which strikes the prism is wide, the red rays formed from the basal part of the beam will overlap the blue rays from the beam passing through the top of the prism. The overlapping results in an imperfect spectrum, such as Goethe obtained, showing red to yellow at the top and blue-violet at the bottom, with the central region white. A piece of white paper, for example, when viewed through a prism is colored only at the edges, as Goethe observed, but it is too wide a source of light (reflected) to obtain a pure spectrum. It is necessary to use a pinpoint beam of light, which might be represented as a single line of light striking one point of a prism.

When contemporary physicists refused his theory the honor of consideration, Goethe requested the French Academy to report on his work, but his request was rejected. With criticism of his observations withheld by those who were best qualified to criticize, Goethe defended his theory of colors rather bitterly and with a certain amount of obstinacy to the end of his life.

It is significant that Goethe's one failure in the field of science also represents his one departure from the method of synthetic thinking. He had no aptitude, nor even interest, in mathematical analysis of phenomena, and this contributed much to his failure to derive from his observations a correct understanding of light and color. But great respect is due Goethe's accumulation and systematization of facts and observations; and the artists of the period, concerned mainly with qualitative knowledge of colors as perceptions rather than as physical phenomena, derived a great benefit from the *Farbenlehre*. Even today, when one reads the monograph, one is struck with the fact that Goethe's explanations of his observations constitute a monument to ingenuity.

Goethe was fortunate in possessing the combination of a brilliant mind, the broadest natural interest in things, and the accident of living at the threshold of a great scientific era. For not only was Goethe one of the greatest contributors to the world's literature, he was one of the few men possessed of the kind of thinking necessary to bring to science an entirely fresh point of view. He lived at a time when science was turning from the purely descriptive era to one in which synthetic thinking produced the first great concepts in the realm of biology. Goethe was endowed with the ability and the zeal to be one of the first great contributors: in zoology, the vertebral theory of the skull; in zoology and botany, the comparative method; in botany, the doctrine of homology; and in biology, although not credited with a part in it, he was one of the contributors, before Darwin, to the concept of organic evolution. Because these contributions belong to all mankind, a mind like Goethe's *is* the common property of all nations.

STRATEGY, ECONOMICS, AND THE BOMB

JOSEPH E. LOFTUS

Formerly economic adviser at the Office of Economic Stabilization and at the Office of War Mobilization and Reconversion, Mr. Loftus is now Director of the Teaching Institute of Economics at the American University, Washington, D. C. During the past year, the Institute has concentrated its resources on an extended examination of the economic aspects of atomic energy utilization. This paper is one of a series of studies developed in the course of the Institute's work.

THE publication of Professor Blackett's book *The Military and Political Consequences of Atomic Energy* last October in London, and the subsequent American "translation" under the title *Fear, War, and the Bomb,* has elicited wide attention and comment. The volume of book reviews in the past few months has been so large that a reviewer, at this late date, can quite safely assume that his readers have at least read enough reviews—if not the book itself—to be quite familiar with the general thesis of the book, its method of analysis, the main evidential materials of the analysis, and the general conclusions.

Accordingly, this review will not spend limited space on another summarization of the contents of the book. Nor will it concern itself with an analysis of its political arguments, or the general conclusions and recommendations of Professor Blackett. Rather, this review will limit itself to a detailed examination of three themes which, on the one hand, are fundamental to Blackett's position, and which, on the other hand, have not been scrutinized too critically or carefully in the reviews published thus far. These three themes are: (1) the emphasis on the employment of "numerical or statistical" analysis in military and economic problems, (2) the imputed importance to Russia of atomic energy as a source of electric power, and (3) the effort to deflate, in the light of World War II experience with large-scale aerial bombardment, the generally held opinions on the tactical and strategic consequences of the A-bomb.

ON QUANTITATIVE ANALYSIS

In the introduction to his subject matter, Professor Blackett emphasizes the importance of "numerical or statistical" reasoning as opposed to "qualitative" reasoning in considering the military and political consequences of atomic energy. He summarizes his own predilections in this respect with a quotation from Charles Babbage: "Nor let it be feared that erroneous deductions be made from such facts; the errors which arise from the absence of facts are more numerous and more durable than those which result from unsound reasoning respecting true facts." It is not just a flippant digression to point out that this opinion of Babbage's was not arrived at by a numerical or statistical comparison of the number and durability of errors obtained by the one method as against the other! It is purely and simply a qualitative value judgment of precisely the sort that Professor Blackett is in principle complaining against.

To the extent that Professor Blackett, in this emphasis on quantitative reasoning, means simply that what is required today in the conduct of human affairs is less hysterical and emotional thinking and more—much more—realistic and sober thinking, one must thoroughly agree with him. To the extent, however, that he means that there is an intellectual process called "numerical or statistical" analysis as opposed to, and distinct from, "qualitative" reasoning, one must vigorously disagree with him. One must disagree even more vigorously if he means also that there is something objective and conclusive per se about what he calls "numerical" reasoning. The blunt fact of the matter is that in dealing with social science and military science data, quantitative and qualitative are inseparable. The manipulation of quantitative data requires not less but more exercise of the highest order of qualitative reasoning from beginning to end, from the initial determination of the statistical categories into which the data will be sorted to the final interpretation of the numerical results.

This point cannot be emphasized enough in view of the fact that there prevails in our age an unhappy inclination to identify facts and figures

with objectivity. Professor Blackett's convincingly written discourse on method in the preface, coupled with his impressive use of facts and figures throughout the book, may lead the unwary into believing that subjective and qualitative considerations are at a minimum, when really they are not.

Lest this appear to be quibbling over a methodological nicety, one somewhat extended illustration is worth while exploring.

All of Professor Blackett's analysis of the effectiveness of the atomic bomb, in terms of the experience of the bombing offensive against Germany, hinges upon the proposition that there is a rough rule-of-thumb damage equivalence between the atomic bomb and ordinary high-explosive bombs. The equivalence is of this order: 2,000 tons of ordinary bombs equal one Nagasaki-model atomic bomb; 3,000 tons of ordinary bombs probably equal one improved-model atomic bomb. Now in the technical discussion (pp. 44–46), Professor Blackett correctly, but quietly, states that this equivalence holds true providing the ordinary bombs are *properly distributed over the target.* Nowhere does he examine the problem of what are the real difficulties, the real probabilities, in an actual bombing situation of obtaining this sort of "proper distribution." Even more important is the fact that in all other parts of the book where this rough principle of damage equivalence is applied, not a single reference is made to the "proper-distribution" assumption.

That this is most important can be demonstrated from just two typical case studies from the USSBS reports. Monograph Number 71 in the European Theatre Series of the USSBS studies[1] describes in some detail the bombing effort that was made to eliminate the Hermann Goering Werke steel plant near Hallendorf. The buildings and equipment of the plant were generously spaced over a 1,200-acre plot, although the main installations (i.e., the coke plant, the blast furnaces, the steelworks, the rolling mills, and the 360,000-KVA powerhouse which supplied not only the steel plant but also a substantial amount of power to the industrial economy of this area of Germany) were located in the center of the site, covering an area less than two thirds the size of the total site area.

During the course of the war, this plant was raided twice in 1941, twice in 1943, fifteen times in 1944, and twice in 1945. Accurate bomb tonage figures were available for only four of these raids; but since this plant was never a priority target it can safely be assumed that through the end of 1944 the tonnages would have been so light as to have caused no significant damage. That this is a prudent assumption is indicated by the fact that the tonnage dropped on December 17, 1944, was only six tons! Accordingly, it is not unreasonable to assume that the bulk of the damage that was done to the plants was done during the 478-ton raid of January 14, 1945, and the 253-ton raid of March 29, 1945.

Although it would be interesting to discuss in detail the nature of the damage done, the important point to be emphasized is that despite the fact that both these raids were conducted in midafternoon at a time when the air forces had complete control of the air, no "proper distribution" of the ordinary bombs was achieved. For example, in the big January raid, out of some 3,190 high-explosive bombs dropped (mainly 500-pound bombs), only 1,200—much less than half—fell within the total plant area, and the bombs that did fall within the plant area were neither spread evenly and symmetrically over the whole area nor concentrated at the main vulnerable points. What was in fact obtained was a random scattering of bombs all over the site. The powerhouse, coke plant, and the ore preparation plants, which offered "the greatest opportunity for disrupting operations with the smallest direct bomb effort," were not severely damaged.

Picture the results in the same 1,200-acre plot if an atomic bomb were dropped anywhere in the center of the large triangle formed by the powerhouse, the steelworks, and the second battery of blast furnaces—or, for that matter dropped, within limits, outside the periphery of the 1,200-acre plot. It is a significant point, though not mentioned at all by Professor Blackett, that the A-bomb when used in precision bombing of key industrial and military targets has greatly extended the destructive range of "near-misses." The atomic bomb literally provides a new connotation for the old boyhood taunt, "A miss is as good as a mile"!

The second case study that illustrates the unreality of an easy assumption of "proper distribution" of ordinary bombs is taken from Monograph Number 185[2] of the USSBS series on the European Theatre. This monograph describes in detail the bombing offensive against the Synthetic Oil Plant at Meerbeck-Hamburg, Germany. The plant was one of the largest Fischer-Tropsch process plants in Germany, and, although it accounted for only 1.7 percent of Germany's synthetic oil production, was an important producer of mixed and light Diesel oil, *Triebgas,* and gasoline.

As a target, this plant was located on an even smaller land area than the Hermann Goering plant at Hallendorf. All the buildings were concentrated close together on a trapezoidal area covering but 100.8 acres. Despite the relative smallness of the land area of the target, it was found necessary, during the war, to drop a total of 7,403 tons of high explosives on the target. Of this total, 6,343 tons were dropped during the intensive attack period, July 20, 1944, to November 20, 1944.

The plant, of course, was completely destroyed by the end of the intensive attack period. The significant point is, however, that of the 7,403 tons dropped, the USSBS was able to find evidence of only 116 tons of bomb hits in the plant area. Or, stated another way, of 19,126 bombs dropped, only 328 fell within the plant area Only a vigorous effort finally obtained the distribution of bombs over the target area that was required to render it inoperative.

With the plant located in such a small area (100.8 acres), one atomic bomb dropped anywhere in the area, or anywhere outside the area for a considerable distance beyond the periphery, would have accomplished the same physical destruction with considerably less total effort.

Detailed examination of the bomb plots and the analyses of air raid damage contained in the USSBS individual monograph studies of sixteen German cities subjected to heavy area bombing eloquently testify that it is a most difficult task to obtain a "proper distribution" of ordinary bombs.[3]

To summarize, Professor Blackett has based a substantial part of his elaborate "numerical" analysis of the number of atomic bombs required to accomplish the same amount of damage inflicted on Germany by ordinary bombs on the principle of equivalence that one new-style atomic bomb equals 3,000 tons of high explosives properly distributed over the target. His failure to adjust his "numerical" application of the equivalence principle to take account of the fact that one of the reasons that such great amounts of high-explosive bombs were required was the enormous difficulty of obtaining a proper distribution of bombs, represents on his part a qualitative judgment (or lack of it!), that raises some serious questions as to the validity of the conclusions of some of his extended quantitative analyses.

Other and more important instances of the same sort of thing will become apparent in the subsequent discussion of Professor Blackett's analysis of the economic and military implications of the atomic bomb. The caution to be emphasized, for the moment, is that in reading Blackett one must ever be on the alert to avoid mistaking statistical arguments for objective arguments. It must be recognized that the making of significant qualitative assumptions is inescapable in the process of assembling and interpreting numerical data. It is these qualitative judgments that ultimately determine the validity and the significance of the numerical results.

THE ECONOMICS OF THE BOMB

In a somewhat brief chapter entitled Power From Atomic Energy, Professor Blackett attempts to establish the argument that one of the significant contributory causes for the breakdown of negotiations for an international agreement on the control of atomic energy has been—and will continue to be—the disparate importance of atomic energy as a source of electric power in the USA, a power-rich country, and in the USSR, a relatively power-poor country.

The argument, cleansed of the exaggerated—and unnecessary—digressions on the motives of certain people and groups in the USA, can be summarized as follows:

1. The industrial strength of any modern nation (and thus, also, its standard of living and its military potential) depends on the availability of electric power;
2. But historically there has been a wide disparity in the levels of energy production and consumption in the USA and the USSR—the former in 1935 consuming six times as much energy as the latter;
3. Now, atomic energy shows bright promise of providing a new source of electric energy, with unique and attractive physical and cost characteristics.
4. In a world *without* international regulation of atomic energy, therefore, the USSR would in all probability exploit the development of atomic energy for industrial purposes at a faster rate and in greater magnitude than the USA because she has a much greater incentive to do so.
5. But, in a world *with* international regulation of atomic energy (a la Baruch), the USSR would be prevented from exploiting atomic energy for industrial purposes at the speed and to the extent that she otherwise would in an unregulated world. This, Blackett contends, would hold true regardless of whether in the international regulation arrangements the allocation of primary generating plants were done by an initial binding treaty or by a series of *ad hoc* decisions by an international regulatory commission. If the former, the treaty would have to allocate primary plants according to a formula that would preserve the "strategic balance" among the great powers, thus perpetuating the existing energy disparities as between USA and USSR. If the latter method were adopted, the USSR would always be a minority member of such a commission. As such, she would always have to fear discrimination by the majority; the best she could hope for would be the sort of automatic treatment resulting from a decision of the committee scrupulously to pursue a policy of preserving the "strategic balance."

At first glance, the argument sounds most plausible and convincing. Upon close scrutiny, the argument can neither be substantiated nor refuted. Too many unknown factors are involved. The strongest statement that can be made is that this is a *hypothesis,* not an argument, that merits much more analysis and *quantitative* study. As the life of the world's economies unfolds in the future, the hypothesis may eventuate to have been correct; but there is no governing consideration for accepting it now. If anything, the very meager evidence available now suggests either rejecting the hypothesis or initiating in the UNAEC an extended quantitative inquiry on the problem—the sort of inquiry that Professor Blackett pleads for in his introductory chapter but does not provide in this chapter!

Despite the present paucity of good evidence, certain relevant observations can be made that will be useful in assessing the merits of Blackett's economic argument. The first set of observations concerns the differences in the national incentives of the USA and the USSR to develop and assimilate atomic energy into their economies.

In general, it may be stated that Professor Blackett tends to underestimate not only the incentives of the USA, but also, peculiarly enough, the incentives of the USSR. With respect to the latter, it is true that Russian energy consumption is roughly one sixth of American levels. Much more important, however, is the fact that in terms of estimated per capita energy potentials, the USSR has only about 60 percent as much potential energy from conventional sources as this country. Most important is the fact that, even though in the aggregate the USSR has sufficient potential energy to provide her people with a per capita consumption equivalent to current USA levels, certain economically important regions of the USSR have inadequate and uneconomical power resources. A few conspicuous illustrations of this are pertinent.

In the southern Urals, Sverdlovsk, Chelyabinsk, and Magnitogorsk (the Russian Pittsburgh) constitute an industrial triangle tha represents one of the key areas in the Russian economy. Within this triangle are located rich supplies of iron ore, copper, nickel, cobalt, bauxite, potassium, and salt, but poor and inadequate supplies of fuel and power. Coal, the *sine qua non* for development of the rich industrial resources of the region, has to be transported over seven hundred miles from Karaganda and Kuznets. Clearly, in such a resource context, atomic energy electricity, if cheap enough, would be a boon.

In the northern Urals, Bogoslavsk has rich bauxite deposits and is currently the heaviest aluminum-producing region in the USSR. This same region, however, is lacking in adequate power resources. The nearest hydro stations are beyond the range of economical transmission of power and, worse still, are located in a temperature zone where the rivers freeze over in winter. Necessarily, then, bauxite is reduced to aluminum by coal. In such a situation, a completely integrated aluminum operation based on cheap atomic power would be highly desirable.

The situation in industrial Leningrad is much the same. Leningrad has a large supply of peat but inadequate supplies of other fuels. Thus it is necessary for Leningrad to import coal distances of over a thousand miles. Clearly, atomic power, if cheap enough, could greatly benefit such an area.[4]

There can be little question, therefore, about Professor Blackett's assertion that the incentives for pressing forward the development of atomic power are strong in Russia. In fact, the incentives, considered by themselves, are stronger than he indicates. Offsetting these incentives, however, are several other important factors which would have to be taken into account by the USSR in making any final determination to press forward with the development and assimilation of atomic power into its economy.

The first such factor is an important military consideration. If, because of the absence of international regulation of atomic energy, the USSR were successful in quickly developing atomic energy as a cheap source of electric power, she would be early confronted with the problem of making a difficult decision between the economic advantages and the military disadvantages of locating atomic energy power plants in the industrial areas where it would be most needed. In the case of important industrial areas like Leningrad, Bogoslavsk, and the Chelyabinsk-Sverdlovsk-Magnitogorsk triangle, cheap atomic power would be a tremendous economic advantage. But for at least two reasons it would constitute a sizable military disadvantage. In the first place, it is clear that, with or without atomic power, the critical objective of attack in any future war will be the atomic energy industry of the enemy. The destruction of an enemy atomic energy installation would constitute an important reduction in the enemy's capacity to produce atomic weapons. The target, of course, would be doubly attractive if the atomic installation, in addition to making weapons material, were also the base supplier of energy to such

strategic industries as steel, aluminum, etc. For this reason, in a bipolar world ungoverned by atomic energy controls, the USSR would have to be extremely cautious in increasing its economic and military vulnerability by gearing its power-poor industrial areas to an atomic energy power base.

Quite apart from the problem of vulnerability would be the additional risk of having plutonium supplies for an atomic power station curtailed in time of war. Until such time as plutonium is in extremely long supply, the USSR will always have to face the possibility during a war of allocating all its plutonium supplies to weapons, even if this should entail closing down its atomic power plants. In view of this, the USSR will have to be extremely cautious in adapting its important industrial areas to a power base that it might not be able to supply without interruption.

A second factor that might tend to restrain the rate at which the USSR would develop atomic power would be the cost of the effort in terms of the other projects which have a high priority in their development effort, and in terms of what burdens the economy can stand at a given point in time. Measured in units of skilled manpower, or in units of critical materials, any rapid and extensive development of atomic power by the USSR alone would be an extremely costly proposition.

When these limiting factors are combined with the positive incentives the USSR has for developing atomic power, no clear picture emerges. At best, the Soviet Union will have to make a difficult decision between the economic advantages of cheap power and the military disadvantages. It is not inconceivable that it is the recognition of these limiting factors that prompts the USSR to make less of an issue at UN of the power implications of atomic energy than does Professor Blackett. For, Blackett's analysis notwithstanding, the curious fact is that in general the USSR delegation has been congenial to the idea of using quotas in controlling peaceful atomic activities.[5]

With respect to the USA, Professor Blackett again underestimates the incentives. In the first place, although this country is now at a uniquely high level of per capita energy consumption, there is evident no clear indication that the persistent historical surge toward higher and higher levels of per capita consumption will die down. The trend toward greater utilization of electricity-consuming equipment in the home, on the farm, and in the factory continues strong.

Second, the USA per capita annual consumption of energy is beginning to press upon its estimated per capita annual potential energy output. Currently, the USA is consuming, on a per capita basis, roughly 30 percent of the energy it could produce if its potential annual per capita output were completely exploited. The corresponding figure for the USSR—unadjusted upwards for recent territorial changes—is in the neighborhood of only 10 percent. Although even a figure of 30 percent at first glance looks so low that there need be no concern for seeking new sources of electric power, the fact of the matter is that, when you take out of the potential figure all the high-cost and locationally unattractive potential energy sources, the 30 percent figure becomes significantly higher. In short, for the long view, even taking into account the effect of a declining population, but assuming the continuance of the tendency to consume ever-increasing amounts of power, the USA has a considerable long-run incentive to develop atomic power as a significant supplementary source of energy.

In this connection, Professor Blackett might argue that even if this were true this is something for the future, and that since the USA is not accustomed or capable of taking the long view, such a future prospect would not constitute an effective incentive to the early and quick development of atomic power by the USA. Such, certainly, is the implication of his discourse on the disposition of the USSR to take the "long view" (p. 110), and the intent of his several references to the pressure groups within the USA that are hostile to the development of atomic power. It may be that Professor Blackett's opinion will prove to have been a sound appraisal of the American scene. Sadly enough, the fragmentary evidence there is suggests that he may be right; happily, however, it is not conclusive. There are sufficient forces at work in the economy for one to be hopeful that this country will take the long view as it has in other contexts in the past.

A third consideration respecting USA incentives that Professor Blackett completely neglects is the impact of atomic power on the American economy in the absence of any substantial international regulation of atomic energy. Let us suppose that the present impasse between the USA and the USSR should continue. Suppose further that the impasse should continue to be characterized as it now is by: (*a*) chronic war scares, (*b*) high armament expenditures, and (*c*) an effort on the part of both countries to achieve and maintain a supremacy in atomic weapons. If such a situation persisted long, it is not unreasonable to conjecture that the American people, burdened on the one

hand by a heavy tax rate, and impressed on the other hand by the use of atomic energy as a source of electric power, might insist on the conversion and distribution of atomic power as a means of somewhat reducing the net cost of the high national military budget. If such eventuated, the USSR—doing the same thing—would gain in its absolute level of energy utilization but would not gain in reducing the existing disparity between the levels of utilization in the two countries.

On the whole question of incentives, then, little of a really conclusive nature can be said The USSR clearly has sizable incentives, but these incentives are circumscribed by serious limiting considerations. The USA may have less compelling incentives, but what incentives she has are less diluted by limiting factors.

Even assuming, however, that somehow or other the USSR should have more intense incentives, in the final analysis the extent to which the one economy or the other will or will not embrace atomic power will depend on the cost of such power in a given situation relative to the cost of obtaining power by alternative means.

In commenting on some opinions that have been expressed on the possible economic gains of atomic power in different countries, Professor Blackett wisely asserts that

> The sounder way of estimating the potential gain to any country from increased power supplies is to calculate the total social cost, not of replacing its existing power supplies by atomic power, but of raising them to the level found necessary for the attainment of an adequate living standard. . . .

This is correct but incomplete. The sound way of estimating the potential gain to any country is to calculate not only the cost of its marginal supply of power, but also the comparative cost of obtaining that additional increment from all other energy sources. Thus for a given new addition to the power supply of a country it is necessary to calculate and contrast the probable cost of that increment if produced by atomic power, if produced by the most efficient hydro plants, if produced by the most efficient steam plants, and so forth. Atomic power, say, twenty years from now, will be adopted in any particular economic locality only if the net cost of so producing power is equal to, or less than, the cost of producing power in that same area by the then most efficient style steam stations or hydro stations or any other process for producing power.

The figures which Professor Blackett cites from Mr. Schurr's study are of little help in making such a calculation. Indeed, as they are used by Blackett, they tend to be misleading. For example, even if the Thomas estimate of 8 mills for atomic power should eventually prove to be correct, there is no reason to think that this figure would be the same in all countries of the world. This is a most plausible conjecture to make if one assumes that atomic power will be developed outside the framework of an International Development Authority. Because of the fact that atomic power will involve large capital costs, the unit cost of power from atomic energy in any country is going to be seriously influenced by the cost of money in that country. Other factors, such as differences in salary scales for skilled technicians, differences in construction costs, and variations in the demand for electricity, will tend to produce variations in the total unit cost of atomic power in different countries. For example, in the case of Argentina, it would not be impossible that because of factors such as these the cost of atomic power there would be 19 mills as contrasted to an 8-mill atomic power cost in the United States. In such a situation, Argentina would make her decision to construct atomic plants on a comparison of the 19-mill figure with the cost of producing power from the then existing most efficient alternative methods of generating power.

If the figures on atomic power costs as used by Professor Blackett are unintentionally misleading, his use of the figures on generating costs for Argentina, Great Britain, and the USA is surprisingly careless. Despite the fact that Mr. Schurr, in his article, took pains to point out that these cost data are not actual generating costs, but rather are costs estimated from the cost of fuel in various regions, Professor Blackett in quoting them states that they are "actual average cost[s] of electricity" (p. 108, Table 3).

This, by way of digression, is a good illustration of the importance of recognizing always the qualitative foundations of a quantitative analysis that was referred to in the earlier paragraphs of this paper. Mr. Schurr chose to use estimated costs rather than actual average costs for a quite sound reason. Had he used average actual costs, his final cost figure would have had an upward bias because the average figure would reflect the costs not only of the latest and most efficient plants but also the costs of the old, obsolete, and inefficient plants. Since reliable figures on costs of the most efficient plants were not available, he adjusted the costs of a modern 100,000-kilowatt plant as described in the December 2, 1939, issue of the *Electrical World* to reflect variations in the cost of fuel in different areas. To the extent that there have been techno-

logical improvements resulting in cost reductions since 1939, the figures overstate the costs of generating electricity in the several countries. To the extent that there have been changes in the net cost of coal, the generating costs have an upward or downward bias depending on the direction of the movement of coal and transportation prices.

What Mr. Schurr has done is ingenious, yet legitimate. It must be recognized, however, by anyone using the figures to demonstrate a given point that the validity of the figures is necessarily limited to the validity of the qualitative assumptions that went into their original composition. This Professor Blackett has neglected to do.

Much more could be said on the question of the comparative costs of atomic and conventional power. The main points to be stressed here, however, are simply these two. First, the studies so far available are too fragmentary, too conjectural, to be used for reaching firm policy opinions that atomic power would be more economical in one region than in another. Second, it is of the utmost importance that economists exert a more vigorous and extended inquiry into the power aspects of atomic energy. The work of Schurr[6] and Isard[7] is a sizable contribution; but measured against the amount of knowledge that is required if wise policy decisions are to be made, it is insignificant.

One last word on Professor Blackett's analysis of the power implications of atomic energy is in order. Although he describes in some detail the power poverty of India, China, and other backward economic areas, he fails to consider how in fact such countries would benefit by atomic power in a world without an International Development Authority. At best, he implies that by some mysterious process, power-poor countries possessing indigenous supplies of uranium and thorium would produce atomic power.

But such is not the testimony of history. Kuwait, with her rich petroleum deposits, has always been a power-poor country; so also Alaska, despite of her extensive coal deposits; and so also the Belgian Congo, despite its abundant hydro potential. Less dramatically, countries like most of those in the Latin-American bloc have not significantly exploited their limited energy potentials.

In all probability, availability of fuel resources is a relatively unimportant factor in national economic development. Availability of capital, climatic conditions, political stability, the cultural texture of the inhabitants—all these and many other factors influence the rate of economic development in a given country. Left to themselves, the backward economies of the world will probably have their relative energy positions worsened rather that bettered by atomic power.

This, however, would not have to be the case if atomic power were developed on a cooperative international basis. In the financial resources and in the technical knowledge of an International Atomic Energy Development Authority resides a real hope for the backward areas of the world. The same holds true for the more developed areas which lack adequate power resources and capital: France, Italy, Japan, and even England.

It is an unfortunate thing that thus far this aspect of international control of atomic energy has not been sufficiently emphasized. Too much emphasis has been placed on the necessity of international control as a mutual protection against atomic warfare; too little attention has been given to the great positive possibilities for improving the standards of living of the countries of the world that are inherent in an International Atomic Energy Development Authority. It is to be hoped that the current discussions at UNAEC on such questions as the use of quotas and the financing of an international authority will bring out in clear light this important point.

In this connection, it is not easy to appreciate Professor Blackett's low estimate of the "generosity" of the Baruch proposal. Considering the preponderant financial, technical, and personnel contribution that this country would have to make to an International Development Authority, Professor Blackett does not establish too convincing a case that the American proposal is ungenerous.

THE MILITARY ASPECTS OF THE BOMB

The backbone of Professor Blackett's book, of course, is the analysis of the strategic and tactical consequences of the atomic bomb. Starting from the prudent position that in order to assess the effects of atomic bombs in future wars, one must begin with as sound and detailed knowledge as possible of the effect of atomic bombs and weapons of comparable destructiveness in the past, he proceeds to an extended review and appraisal of the performance record of aerial bombing efforts during World War II. Since there is available only the wartime experience with two atomic bombs, released under quite exceptional circumstances, the bulk of experience analyzed is in terms of the enormous conventional bombing effort over Germany and Japan.

In studying the analyses of the air offensives against Germany and Japan, as contained in the five summary volumes of the USSBS, Professor Blackett observes that the significant fact is that

a huge weight of bombs dropped on Germany did not lead to a failure of either production or civilian morale. From this observation, he makes two important deductions: (1) since 3 million tons of bombs were dropped by the Anglo-American air forces on German and Japanese targets without decisive effect, it is certain that a very large number of bombs would be needed to defeat a great nation by bombing alone; and (2) in any future war between the USA and the USSR, the conflict would not be decided by atomic bombing alone, or in a short period of time. On the contrary, he contends, there would ensue a protracted, bitter struggle spread over much of Europe and Asia, involving million-strong land armies, huge military casualties, and widespread civil war.

Earlier in this review, the point was stressed that quantitative analysis necessarily involves the making of significant qualitative judgments. Professor Blackett's extended numerical treatment of the Anglo-American bombing offensive against Germany is another sad illustration of how qualitative judgments, however hidden they may be, determine the whole numerical outcome. For reasons known only to himself, Professor Blackett has chosen to evaluate the effectiveness of strategic bombing in terms of the total tonnage dropped from 1940 to 1945 considered as an aggregate, rather than as a series of periods in which strategic bombing as an instrument of warfare was being progressively perfected. Given this qualitative decision, it was inevitable that he should arrive at his conclusion that the Anglo-American air forces received a relatively small dividend in return for their investment of over a million and a third bomb tons dropped on Germany. Thus, also, the inevitability of his conclusion that over four hundred improved atomic bombs would be required to inflict comparable damage.

The fact of the matter is, however, that so important were the lessons learned in the course of five years that any substantive similarity between the strategic bombing of 1940–43 and the strategic bombing of 1944–45 is of only a nominal nature. The developments were not simply in the nature of improved planes, bombsights, long-range fighter bombers, navigational aids, and photographic interpretation; fundamentally, the most important development was a realization of the strategic character of strategic bombing.

The basic resources of an economy are its industrial capital equipment, its industrial manpower, and its supply of raw materials. For strategic bombing to have a decisive effect, it is essential that the bombing be directed at the most vulnerable points in these basic resources. Largely out of desperation, but partly out of bad judgment, the British Air Command chose in the early years of the war to attack that German basic resource which was the least vulnerable but the most accessible, namely, German urban manpower.

The choice was unwise for at least two reasons. In the first place, Germany at the beginning of the war through to at least mid-1944 had more than ample manpower reserves. A large native labor supply, supplemented by sizable increments of foreign labor, provided the German economy with adequate insurance against almost any conceivable high casualty rate from area bombing. In the second place, the labor supply that was concentrated in the cities subjected to area bombardment did not contribute greatly either to total Reich production or to total Reich war production. For example, Augsburg, Bochum, Leipzig, Hagen, Dortmund, Oberhausen, Schweinfurt, and Bremen—cities that were the targets for severe area attacks—contributed to total Reich industrial production in very small percentages. In the order named, the contributions of these cities were: 0.3 percent, 0.9 percent, 1.7 percent, 0.3 percent, 0.9 percent, 0.5 percent, 0.2 percent, and 1.2 percent.[3] With the exception of the iron and steelworks at Dortmund, and the aircraft plants at Bremen and Leipzig, none of these cities were significant producers of war material.

A later choice by the Air Command to attack aircraft production centers was as imprudent a decision as that to attack urban centers. Aircraft production in Germany was a highly decentralized and deconcentrated operation. Planes were produced in many plants spread throughout the land—plants that were of modern type, well camouflaged, and constructed with a view to minimizing the damage effect of bombs. As a direct target for air bombardment, it was a highly invulnerable sector of the German war economy.

Despite the fact that because of its resource base Germany was not an easy target, there were at least three points of great vulnerability: her power system, her synthetic oil industry, and her transportation system.

The first-mentioned vulnerable point, much to the wartime surprise and the postwar delight of the Germans, was never a high-priority target. The tightness of power supplies, on the one hand, and the frangible nature of most power-generating and -transmitting equipment, on the other hand, were never adequately understood by Allied air intelligence. Hence, this crucial target was unfortunately overlooked. Had it not been, much

greater returns could have been achieved by the air offensive for an infinitesimally smaller investment. For example, officials of the Berliner Staedische Elektrizitaetswerke A G. have stated

> that if the power plants of Klingenberg and West had been destroyed by bombing, the industrial life of Berlin would have come to a complete standstill. Not even the outside national networks could have supplied sufficient power without seriously curtailing the consumption in other parts of Germany.[3]

The significant fact is that 50,412 tons of bombs dropped on the city as a whole never achieved the industrial paralysis that a few hundred tons dropped on two electrical plants would have. By Professor Blackett's calculus at least sixteen improved atomic bombs would have been required to obtain the same limited industrial paralysis of Berlin that was in fact obtained. The more significant calculus is that two atomic bombs placed anywhere within a wide near-miss radius of the two power stations would have brought the industrial life of Berlin to a complete standstill. The weapon assumes an even more formidable appearance if one considers the radioactive effect of an atomic bomb detonated much closer to ground zero than was done at Nagasaki.

Because of her lack of indigenous supplies of petroleum, Germany was precariously dependent upon her synthetic oil production. Despite her invulnerable capacity to produce planes and tanks in large numbers, the final determinant of how many she could effectively put into battle rested with her synthetic oil industry. The complete destruction of her synthetic oil capacity, after allowing a time lag to account for the exhaustion of accumulated reserve stocks of processed oils, spelled the end of the German war machine. An investment of less than two dozen atomic bombs well placed could have rendered the German war machine inoperative after the expiration of the short time in which it would have used up its reserve stocks.

In this connection, Professor Blackett erroneously attributes the late mounting of the air offensive against the synthetic oil industry to the necessity of the Allied command waiting until it had advanced German bases and air superiority. Actually, a good part of the delay was caused by the long time it took the Air Command to learn the decisive strategic importance of oil in the German war economy.

The third soft spot in the German economy was her transportation system. Since coal was the key to German industrial production, war production of tanks, munitions, and so forth could have been as effectively stopped by the interdiction of the transport system as by a much, much larger effort at destroying all the individual plants producing finished goods. Once this was realized by the Air Command, extensive raids on concentration yards, bridges, tunnels, and rail bottle necks were launched. Eventually, this effort was successful in so reducing coal shipments that industrial production came to a standstill in many areas [8]

It is difficult to speculate on the effectiveness of atomic bombs on the transportation system of a country. The USSBS study of Hiroshima and Nagasaki is anything but informative. From the meager evidence available it would appear that atomic bombs might be quite effective against concentration yards; their effect on tunnels, and especially bridges, would seem to be negligible.[9] This is not merely of academic concern for at least two reasons: (1) experience in Germany suggests that the destruction of bridges was the most effective single way of disrupting rail movements for significant periods;[8] and (2) in both the USA and the USSR the transport systems are highly vulnerable points of the economy. This is especially true of the USSR.

The experience with mass bombing in the European and Pacific theatres has several important lessons that can be briefly summarized. In the first place, for strategic bombing to be strategic in the literal sense of the word, and effective in terms of military consequences, it needs must be directed at those points in the economy of the enemy that are most vulnerable, those points that if destroyed would bring about disproportionately larger disruptions of the economy and war machine as a whole. What those vulnerable points will be for a given country depends upon the structural specifics of that economy. For example, in the USSR, considering her industrial dependence on coal and the large distances that coal would have to be moved over inadequate transport facilities, a high-priority target in a rational strategic plan would be the interdiction of her rail network. Like Germany, the USSR has a highly invulnerable aircraft- and tank-production capacity; thus, only a larger bombing effort than makes sense would be required to reduce such production. Somewhat like this country, USSR steel production tends to be concentrated in one area—Magnitogorsk—thus providing a highly attractive target for strategic bombing.

A second and more important lesson is to be derived from the World War II experiences. It seems fairly clear that urban area mass bombing

would be employed in a future war in three different contexts: (1) as a "what-else," desperation effort to achieve decisive effects at the outset of a war; (2) as a by-product result of the precision bombing of a key industrial installation located within or near the periphery of a city; and (3) as an effort to hasten the conclusion of a war after the industrial capacity of the enemy to continue the war has been destroyed. This was the rationale of the urban attacks on Japan by conventional bombs and atomic bombs in the closing months of the war.

With respect to the first context, one must agree with Professor Blackett that this sort of effort would in all probability not be made in a future war. Recognizing the passive defense opportunities available, the absence of the surprise element of Hiroshima and Nagasaki, the futility of manpower annihilation against a nation with substantial manpower reserves, it is difficult to conceive of the generalship of either side of a conflict wanting to engage in such a grim, futile business at the beginning of a war.

Allowing, however, as Professor Blackett does not, for the facts: (*a*) that generalship is not always rational, and (*b*) that the radiation effects of atomic bombing are not sufficiently known for one to have firm judgments, one must admit there is more than just a slim possibility that such an endeavor would be made by one side or both. If such were the case, the USA would be at a disadvantage because of its greater urban population concentration and its lesser total manpower reserves.

It should be pointed out, however, that Professor Blackett tends to underestimate the degree of population concentration and urbanization in the USSR. For example, in discussing Dr. Oppenheimer's statement that it is not inconceivable that United States air squadrons could eradicate 40 million people, Professor Blackett asserts that to do this "many of the targets would be quite small cities with many less than 40,000 people in them" (p. 72). In other places he speaks of Russian "towns"—the clear implication being that there is relatively little urbanization in the USSR in the American sense of the word.

Such, however, is not the case. In 1939, somewhat over 34 million people lived in Russian cities of over 50,000 inhabitants. Eleven cities, with an average population of 1.1 millions had populations of over 500,000 inhabitants; 71 cities, with an average population of 200,000 people, had populations between 100,000 and 500,000; and 92 cities, with an average population of 73,000, had between 50,000 and 100,000 inhabitants. Or, stated another way, 172 cities with an average population of 198,000 had populations of over 50,000 inhabitants.[10] The comparable figures for the USA are: 197 cities with an average population of 230,000 had populations of over 50,000.[11]

Taking into account the changes brought by the war, the relocation of cities and population, and the increase in population, it is most probable that the degree of population concentration in the USSR is greater than before the war. The rapid rate of industrialization since 1939 would almost necessarily bring about a greater rather than a lesser urbanization.

Although Professor Blackett has established a convincing case that another war in the foreseeable future would not be a push-button war decided decisively in a short period of time, one cannot accept as convincing his conjecture that the next war would be fought by million-strong land armies over much of Europe and Asia. That Professor Blackett can accept this conjecture in view of his own recogniton of the tactical uses of atomic bombs is hard to understand.

A much more plausible forecast is that initially another war would be a struggle for bases, followed by an unprecedented use of air power to cripple the industrial potential of the opponent, and concluded with a bloody annihilation of cities to accelerate the recognition by the enemy that his warmaking capacity had been destroyed.

It should be clearly understood that the reviewer has the same intense feelings as Professor Blackett on the futility and inhumanity of a preventative war. The difference between the two writers is that this one is convinced that a persuading case against a preventative war cannot be made on the grounds that militarily such a war would be difficult, bloody, and of long duration, if not impossible to win. Nations simply do not behave in such a coldly rational fashion. If a preventative war *can* be prevented, it will be because of the recognition by the nations of the world of some governing moral principles, or the unification of the nations of the world around some positive program that promises overriding attractive benefits for all involved.

This rather lengthy review has narrowly limited itself to the military and economic aspects of Professor Blackett's thesis. His political analyses and conclusions, others have and will continue to discuss. When all is said and done, however, one contribution of the book will stand out to its lasting credit. Like no other book on the subject

matter, Professor Blackett's *Fear, War, and the Bomb* has brought clearly to a large number of people the realization of the importance of a wide popular consideration of the politics of atomic energy. In the last analysis, it is the people generally who can and must make the decisions concerning the uses to which this strange force is put. If nothing else, this book has taught many people that without being physicists they can comprehend the political and economic issues of atomic energy.

REFERENCES

1. *U S. Strategic Bombing Survey* Reichswerke Hermann Goering A G., Hallendorf, Germany.
2. *U S. Strategic Bombing Survey*. Synthetic Oil Plant Meerbeck-Hamburg, Germany
3. *U. S. Strategic Bombing Survey*. Detailed Study of Effects of Area Bombing on Hamburg; Detailed Study of Effects of Area Bombing on Wuppertal; Detailed Study of Effects of Area Bombing on Dusseldorf; Detailed Study of Effects of Area Bombing on Soling·n; Detailed Study of Effects of Area Bombing on Remscheid; Detailed Study of Effects of Area Bombing on Darmstadt; Detailed Study of Effects of Area Bombing on Lubeck; Brief Study of Effects of Area Bombing on Berlin, Augsburg, Bochum, Leipzig, Hagen, Dortmund, Oberhausen, Schweinfurt and Bremen.
4. Gwiazdzinski, J. The Power Resource Structure of the Soviet Economy. Unpublished research paper, Teaching Inst. of Economics, American Univ., 1949.
5 Cavers, D. F New Life for the UNAEC. *Bull. of the Atomic Scientists,* 1948, **4,** (12), 355–56.
6. Schurr, S H. Economic Aspects of Atomic Energy as a Source of Power. *Amer. Econ. Rev*, 1947, 37, (2), 98–108.
7. Isard, W. Some Economic Implications of Atomic Energy. *Quart J of Econ.,* 1948, **62,** (1), 202–208 Isard, W., and Lansing, J B. Comparison of Power Costs for Atomic and Conventional Steam Stations *Rev. of Econ. Stat*, 1949, **31** (forthcoming).
8 *U. S. Strategic Bombing Survey.* The Effects of Strategic Bombing on German Transportation.
9. *U. S. Strategic Bombing Survey*. The Effects of the Atomic Bomb on Hiroshima and Nagasaki.
10 Lorimer, F. K. The Population Statistics of the Soviet Union: History and Prospects. Geneva, 1946, Sotsialisticheskoye Stroitelstvo. Statisticheskiy Yezhegodnik. Moscow, 1939
11 *Statistical Abstract of the U S* Washington: U. S. Dept. of Commerce, 1948.

SPRING FEVER

Now we pause to look around the world
And wonder, while the cogs stand still or slip
A little; here, behind a greening bush,
We sit up suddenly in the sun to laugh
At yesterday, or frighten squirrels and robins
Fattening with Spring. The hurried April
Steams with meadow-murmur, and prodigious growth
Infolds us; close down to the mouth
Of an osmotic Nature, elbowing damp turf
And gulping like new tadpoles where a musk
Actinomyces odor hovers fresh
And pungent, we rise languidly to face
The apparition of our dignity,
Apprized of time, but laughing at the hour,
And singularly vital to the season.

Henry A. Hoffman

PLANTS AND VEGETATION AS EXHAUSTIBLE RESOURCES

STANLEY A. CAIN

Drawings by Matt Kahn, Cranbrook Academy of Art

Dr. Cain (Ph.D., Chicago, 1930), botanist, Cranbrook Institute of Science, has taught also at Butler, Indiana University, the Waterman Institute, the University of Tennessee, and at Cold Spring Harbor. His article is from an address given in the AAAS Centennial symposium on "The World's Natural Resources" last September.

MAN'S economy has always been concerned with the basic matters of food, clothing, and shelter. The nature of these, their abundance or scarcity, their quality, and their relative cost in money and labor, vary from place to place and from time to time. The condition of man with respect to these basic needs depends upon production and distribution of natural resources derivatives and ultimately, of course, upon the balance between the demands of population and the supply of resources. It is my thesis that whether or not these natural resources can be described as renewable or cyclic they are not quantitatively adequate to permit a continuing consumption at present world rates, and that man everywhere must face squarely the dual problems of the conservation of natural resources and the limitation of population or continue along the path, at an ever-accelerating rate, toward self-destruction.

THE NATURE OF VEGETAL RESOURCES

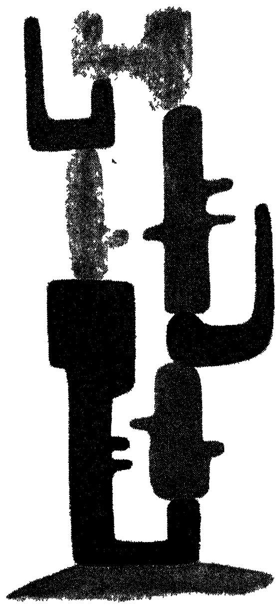
The role of plants in the satisfaction of man's basic needs is unsurpassed by any other natural resource. When we consider the kinds of uses of plants, we find that they are important as energy sources and for nonenergy products. Man's most immediate interest in plants is for sustenance as direct food for himself and, indirectly, as food for the animals that he eats or that do work for him. Much thinking about food (such as measurements of consumption, annual production, and reserves) is in terms of its calories, its energy yield when consumed by the functioning organism. Without implying for an instant that the calorific yield of food can be used as the sole measure, it is important to our natural-resource considerations, for example, that an acre devoted to sugar production yields more calories than several acres devoted to corn-fed hogs, and even more acres used for the production of range-fed cattle. But food serves other functions than that of an energy source. It is through vegetable and animal foods that we obtain the building materials and "regulators" of our constantly renewed protoplasm.

Basically, the human food problem is that of getting enough to eat to prevent physiological hunger; but we are activated scarcely less by psychosocial food hungers. We like to eat at customary times, and our desires for certain foods are outside the logic of nutrition experts. Even though the more favored few may think that other men should moderate their appetites and be satisfied with wholesome plain food, the psychosocial hungers must be considered, as everyone knows whose earnest wife confronts him with a casserole of soybeans.

Plants yield tremendous quantities of energy to man for nonphysiological purposes. In millions of families over the world the home is heated, food is cooked, and water is boiled by the burning of wood or other plant materials. In a smaller number of cases animal fats provide the energy, only a step or two removed from the original plant sources. And what Evelyn Hutchinson has referred to as "fossil sunlight" is yielded to home and industry by the combustion of coal and oil—energy long bound by photochemical processes of plants and not all lost in the vicissitudes of many chemical transformations and many millennia.

When we consider plants and their products in other connections than energy sources, we must be immediately impressed by the thousands of kinds of plants and the myriad of products—most of them manipulated, fabricated, processed, or manufactured—that enter into our housing, furniture, and clothing, into the production of the implements and machines of the means of transportation, communication, industry, business, and recreation.

If we turn our attention now to plants as vegetation, thinking of them not as individuals or kinds



We are here confronted by problems in the balance of nature in what Tansley has called the ecosystem.

but as the communities that mantle the earth, we find ourselves confronted by a more complex situation, and by products and concomitants that are less well understood. We are here confronted by problems in the balance of nature in what Tansley has called the "ecosystem." Nature is particulate and individual in its ultimate structure, but it is organized, and no isolationism is possible. If the ecologists have any single idea that is of importance to man, it is the idea of the wholeness of nature and its communal units. The phenomena of the ecosystem act and interact. The concept of the emergence of complex systems, such as the higher biotic communities, and our holistic interpretation of them, following the philosophy of General Smuts, are products of our increasing knowledge of processes and interrelations, both present and historical.

The point for our present consideration is that man cannot solve in a satisfactory manner any of his natural-resource problems piecemeal without a broad consideration of the unity of the ecosystem. Land classification, reforestation and silviculture on existing forests, range management, watershed protection with all its implications for irrigation, flood control, and wildlife management, the agricultural problems of plant introduction, disease and pest control, crop-type selection and cultivation practices, and a host of other large and small relations of man to his natural world, can only be understood, and wise programs designed and executed, when knowledge of the interrelations of the elements of the ecosystems is both available and used.

THE GROWING DEPLETION OF OUR RESOURCES

Having seen something of the nature of vegetal resources, we return to the basic proposition: Plant resources are inadequate to meet the needs of the world's population, if not now, assuming an equitable distribution were possible, certainly in the near future. Although hunger and even famine are not a new experience to man, especially on a comparatively local basis, such as in one or two provinces of China, the existence or imminence of substandard subsistence levels for hundreds of millions of people on several continents simultaneously is in many ways an essentially modern problem, and it is ironic that this has accompanied recognized advances in our industrial civilization.

Two processes have been accelerating recently at rates that were unknown to man before about a century and a half ago. Beginning with the industrial revolution, and concomitant with the expansion of agriculture onto new lands in the United States and Canada, Australia, Africa, and the USSR, these developments have been, first, a phenomenal growth of population, and, second, an appalling acceleration in the loss of natural-resource capital, especially of fertile topsoils, forests, and waters.

With the industrial revolution have come new means of tilling the soil and of distributing the products of the soil. Society has rapidly changed from various systems of organization based essentially upon a peasantry and subsistence farming to the development of farming as plant industry with specialization in cash-crop surpluses.

Work on the land before the industrial revolution was limited to manpower and horsepower—the energy of food, released by catabolism and employed by muscles. Today, with the power of a hundred horses at their finger tips and with new machines for breaking the soil, cultivating, harvesting, and hauling, and with mechanical saws, bulldozers, and tractors in the forest, fewer men have made the earth yield more resources at a faster rate. There is no quarrel with such technological improvements—the need is for a redirection of effort and a control of the new energy sources, for

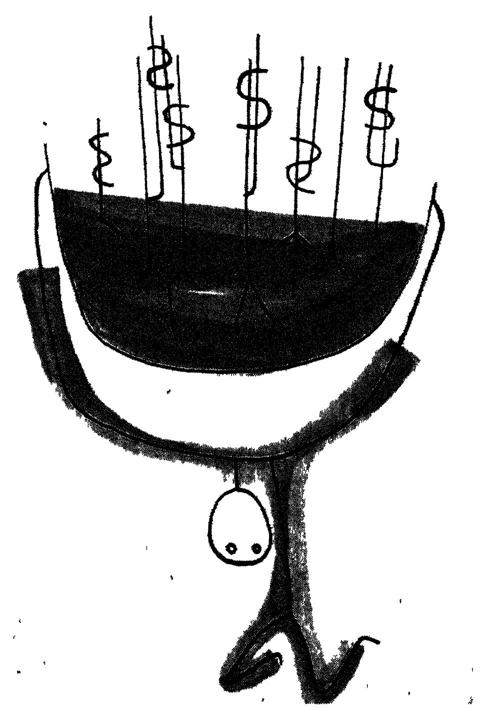
these technological improvements have brought with them undreamed-of complexities. The urbanization of a progressively larger percentage of the population as a whole, including the agriculturists, making living more pleasant, more easy, and more abundant for many, has at the same time made it more complicated, more specialized, and more interdependent.

Yet, despite the complexities of modern-day living, never in the history of man have the conditions been so propitious for an excess of births over deaths. The increased amenities of industrial civilization include more general formal education and more leisure for adult enlightenment. Obviously this may mean, among other things, a knowledge of birth control, which Julian Huxley has considered one of the major achievements of the human intellect, but this educative process also means population increase. Education leads to sympathetic approbation and financial support of research, which in turn leads to better health and better control of disease and pestilence, a lowered infant mortality, a longer useful life, and a falling death rate—this in spite of occasional reductions of birth rate in some localities and for a few special groups of people and the dying out of urbanized families. In general the birth rate has enjoyed a progressive increase through several generations, with the doubling of the world population in less than a century. At present, in the longest-industrialized countries, this unprecedented increase has hit a plateau—indeed, even a failure of women to produce sufficient girl babies to maintain the population at a given level. The waves of industrialization, however, are only now bringing the already populous areas of eastern Europe and much of Asia into the phase of geometric growth. And this in spite of the millions lost through wars, revolutions and counterrevolutions, famines, pestilences, catastrophes, and other calamities of the present half century. In most of the republics of the USSR the reproductive rate is far in excess of that of western Europe. The population of China is said to be growing so fast that all the passenger boats in the world could not transport the increment away if emigration were a possible solution. And Japan produced more than a million and a half babies last year, bringing its population density to the highest maximum yet.

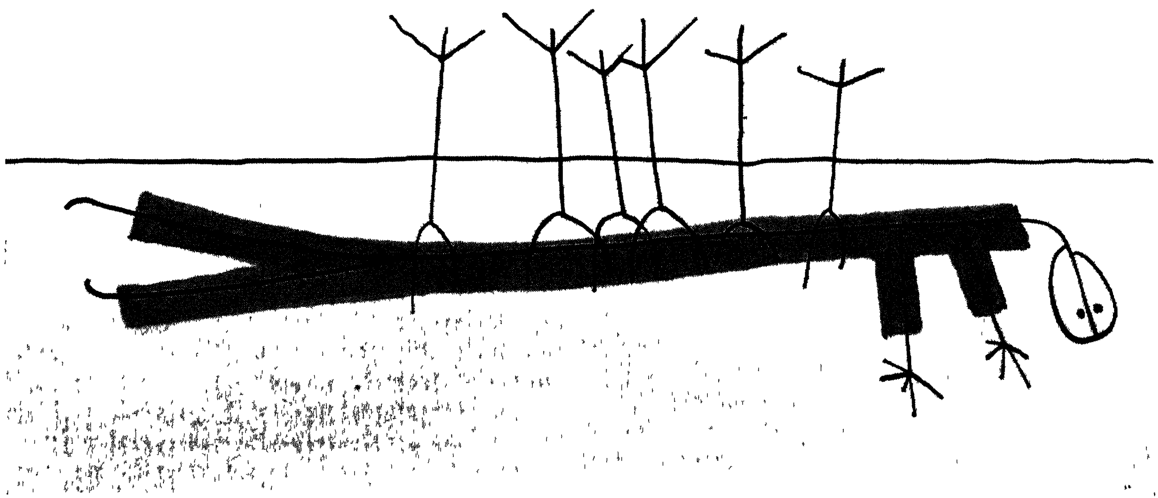
This unprecedented growth of human population cannot be attributed solely to increased technology in the production and distribution of plants and plant products and to the increased acreage of fertile soils brought to the mechanized plow; it has also been due in large part to "mining agriculture," rather than to sustaining agriculture. The result is ironic. The very land-use patterns that have helped increase our population and have raised momentarily the level of living of many of us carry with them the seeds of immediate deterioration and ultimate destruction, for there is a growing pressure of our increased population and a growing pressure of our industrialized philosophy for more and more production, irrespective of loss of balance between productive resources and demand.

THE RESULTS OF OUR HEEDLESSNESS

Our crimes against the land include types of agriculture that result in a steady deterioration and loss of topsoil, such as the growing of one cash crop, like cotton, to the very doorsteps. They include range practices that are inimical to a maintenance of cover, especially in semiarid regions, and forest practices that not only harvest the crop but often, through failure of forest reproduction and subsequent fires, destroy the forest completely and its soils and its wildlife. Even when one knows of methods for the restoration of useful vegetative cover and the repair of the soil, the necessary treat-



Our crimes against the land include . . . the growing of one cash crop, like cotton, to the very doorsteps.



We may be certain that a balance of nature will be attained; but we cannot be certain that this balance will be one pleasing to man.

ment may be uneconomical and impossible under present sociopolitical ideologies.

Accompanying the changes already described, which have brought much of the world's agricultural land to a low state, are a series of consequent water problems. One thinks of the devastating floods that result from inadequate watershed cover, the high sediment burden of streams and their pollution, the lowering of water tables, the drying up of wells, springs, ponds, and lakes, and the loss of irrigation waters. All these changes on the land and in the waters of the land have their effect on wildlife, on food and game fish and animals, on prey and predators, on plant and animal diseases and pestilence—in brief, on the balance of nature. One element of nature, man, has disrupted and dearranged the ecosystem. He cannot prevent the inexorable operation of natural processes, however, and in one way or another he must pay the penalty for his contrariness in a less satisfactory way of life, or even its loss.

We may be certain that a balance of nature will be attained; but we cannot be certain that this balance will be one pleasing to man. Without conservation of our present resources, without strenuous efforts to renew our deteriorated cyclic resources, without both positive and negative checks on the population size, mankind can only wage a losing battle with nature through a series of strategic retreats.

One of the results of a lack of conservation that seems to me to be as inevitable as soil erosion and an ultimately lowered level of living is the development of "human erosion" and of far-reaching changes in our sociopolitical structure. With the loss of the fertile topsoil there is a development of various deficiency diseases, of "hidden hunger," and ultimately such social phenomena as tax delinquency, unfavorable types of tenancy, the maladies of the sharecropper and the "Oakie," resettlement on submarginal lands, and the concomitant deterioration of good human stock along with good soil.

When the relationships between man and the soil are poorly integrated and disharmonious, the average level of living must drop. But averages

alone can give only a partial picture. Under an exploitative economic culture the range of levels of living is very great—the poor are very poor and the rich very rich. Historically, in war and famine, for example, a small minority of exploiters have always done quite well for themselves. And in the cold war of man against nature the exploiters of the natural resources will continue to profit, even as the level of living goes down, not only for the sharecropper and the Oakie, but for the average man, and the disparity will grow between the conditions for him and those for the gougers and pushers.

THE FAVORABLE FACTORS

There are some undeniably favorable factors in this otherwise dark picture. Agricultural production is being increased on a per acre basis by the spread among farmers, in a few countries, at least, of sound practices that permit the use of the land for plant production without its progressive deterioration, and often with striking improvement after a few years. Per acre production is also increased as a result of a variety of scientific investigations, many of which were initiated originally without thought of their possible practical applications.

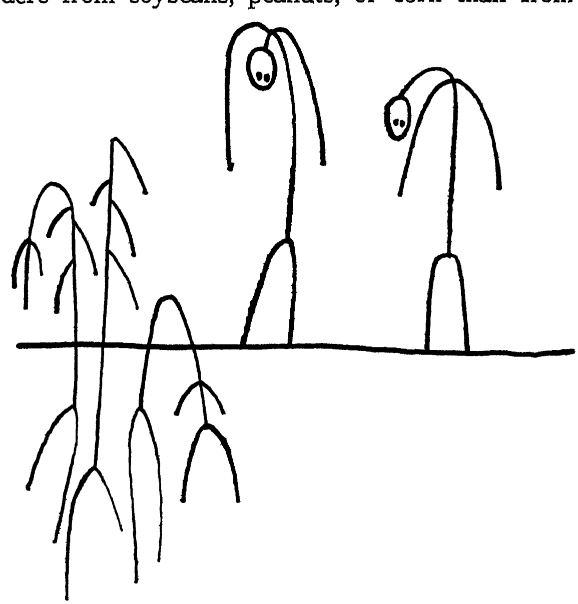
Some of the most effective discoveries have resulted from increased knowledge of the nature of inheritance and the mechanism of adaptation and evolution. The new science of genecology is resulting not only in selection of ecotypes better adapted for particular natural areas and cultural patterns, but is also actually tailoring new varieties to the needs of certain situations, as in the work of Clausen, Keck, and Hiesey, with the cooperation of the Soil Conservation Service, in the production of new kinds of bluegrass for Western pastures and ranges.

The most dramatic story in this connection is that of hybrid corn. Careful estimates show that hybrid-corn yields are more than 25 percent in excess of open-pollinated corn yields. How much this means was made clear by Dr. Stadler, of the University of Missouri, testifying on the subject of science legislation before the Committee on Military Affairs of the United States Senate. He said:

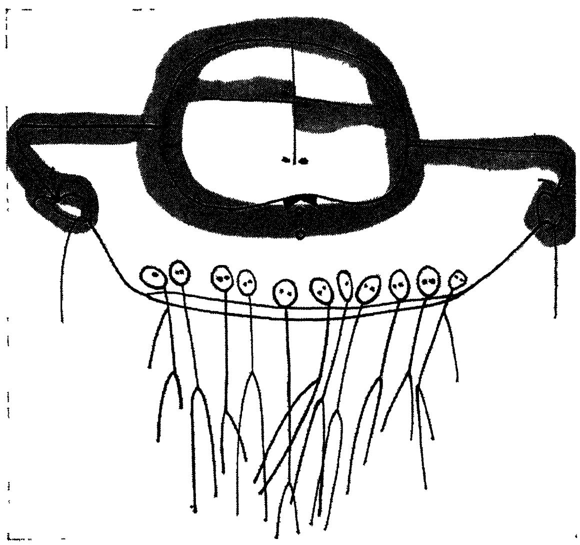
> A conservative estimate of the increase in national corn production during the four years 1942–1945, due to the partial use of hybrid corn is 1,800,000,000 bushels. The money value of this increase on the basis of farm prices per bushel is more than $2,000,000,000.
>
> It is, therefore, no exaggeration to say, speaking in terms of the overall national economy, that the dividend on our research investment in hybrid corn, during the war years alone, was enough to pay the money cost of the development of the atomic bomb.

Although science and technology offer further enrichments, we cannot expect a solution of food problems or of the wider conservation problems by their means alone. Hydroponics (the raising of plants in soilless water) can be useful only in limited connections. We have also heard much recently of food supplies from marine algae, plankton, and the new fisheries. and these are possibilities worth exploring and developing, but they promise only to be supplements and not solutions. Finally, through genetic and refined physiological studies of photosynthesis, especially with the new research tool of radioactive isotopes, there is an increased hope for an ultimate understanding of the basic food-producing processes associated with photosynthesis Whether these studies will eventually result in test-tube, pilot-plant, and commercial production of sugar from carbon dioxide and water is a secret of the future.

All technological developments that turn present waste materials into useful plant by-products, and all substitutions for present plant products of less critical materials, aid in relieving the pressure on the land and are conducive to conservation. Also, a change in sources may make for a more economical use of the land. For example, food fats are raised more economically and abundantly per acre from soybeans, peanuts, or corn than from



One of the results of a lack of conservation is human erosion, the deterioration of good human stock along with good soil.



. . . a small minority of exploiters have always done quite well for themselves

animal sources, as in the case of butter. And the students of microscopic water plants are now promising us a butter substitute from the pastures of the ocean.

One of the most important recent trends toward the solution of resource problems, however, is not in science and technology, but in special education. The idea of the County Agent was a fruitful one, because his job brought him into contact with the farm problems, not as artificially isolated problems but as parts of a complex involving whole agro-ecosystems. The establishment of the Soil Conservation Service was an even more important step. In this Service, for the first time, the manifold problems of conservation were seen as a whole, and attack was made on them, not piecemeal, but simultaneously, by engineers, soil specialists, agronomists, biologists, economists, sociologists, lawyers, and so on. The Soil Conservation Service introduced the Conservation Districts in 1935, and later the idea of demonstration farms began to take hold. These developments constitute a milestone. They are a great achievement of the mind. Problems are seen whole; solutions are attempted *in toto*. But even more important is the democratic principle of cooperative attack. Through his own effort the individual farmer becomes part of a larger cooperative whole for attack on problems he cannot solve alone. Individualism, independence, and dispersive tendencies—which have been so inimical to conservation in the past—are being replaced by joint attacks, pooling of resources, self-help, and mutual help. Governmental agencies play an important assisting role, but the individual is not collectivized in the Eastern sense behind an Iron Curtain.

The picture is also brighter for conservation in certain developments for the education of the whole public. The work in the United States of the universities and of government agencies, of sportsmen's clubs, scientific societies, garden clubs, the League of Women Voters, the Wilderness Society, parent-teacher organizations, etc. has not been as effective as their energy and earnestness should warrant. This is partly because they have sought to persuade political leaders to favor or oppose legislation instead of educating the voter first and putting their democratic faith in his good offices when he understands the problem. Important in this connection is good writing that captures the imagination—a trick the scientist can seldom turn. Paul Sears' *Deserts on the March* must have been a great help in the government's development of the Soil Conservation Service. Two very recent books promise to play an exceedingly important role in educating and capturing the imagination of the public. I refer to Fairfield Osborn's *Our Plundered Planet* and to William Vogt's *Road to Survival.* Out of Osborn's best seller and an idea of the New York Zoological Society is developing the Conservation Foundation, which is already becoming an important educational agency.

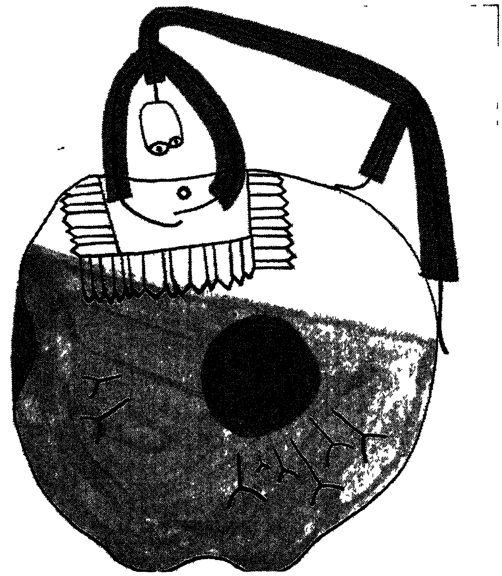
As was said earlier, the problem of conservation is ultimately an international one, and the potentiality of world education in this regard under the United Nations is a large if not immediately hopeful one.

THE FINAL DILEMMA

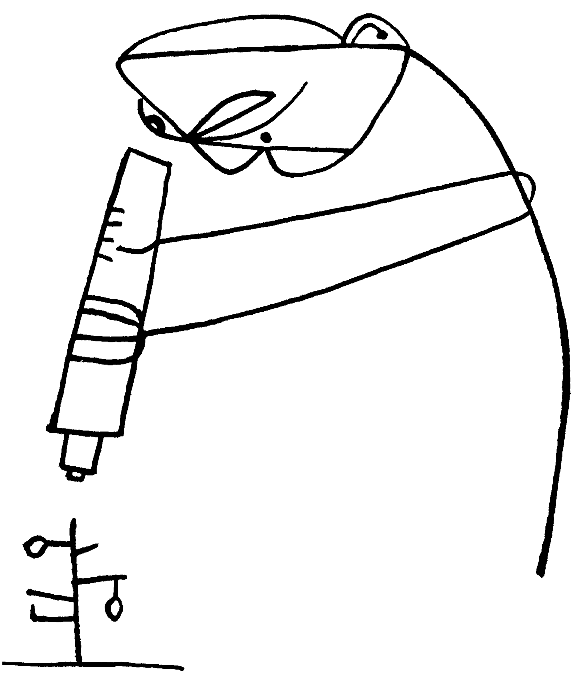
Summoning to mind every advance toward conservation of natural resources that we can, we still must admit that the favorable factors are on relatively small fronts of battle and that the obstacles remaining are so all-embracing as to require a united attack, which depends on comprehensive governmental action and comprehensive education of such magnitude that, when viewed as a world task, it seems highly unlikely of accomplishment. Actually it is a global problem, this balance between population and resources, and neither of the world's granaries—the American one nor the Eurasian one—can long solve the problem alone. The pioneer conditions of our own geographically expanding nation, the abundance and richness of

our natural resources, the expansion of technology, and the development of our patterns of business and industry have all been conducive to policies of exploitation rather than conservation. Our common psychology has been that of expediency and waste, rather than husbandry and frugality. Our mores, and consequently our laws, have favored the exploitative activities of vested interests, allowing them to batten on the natural resources that rightfully belong to the people, as has been pointed out by Bernard DeVoto in discussing Western grazing interests. Even when large sections of the public and government have felt that something has been wrong with such a process, we have rationalized away our worries with the assumptions that the country is being developed, jobs are being made, the country is becoming rich and powerful —and anyway the future will take care of itself.

In the end, large governmental planning is needed, in agriculture as in industry. Planning is inherent in industry despite stout claims of rugged individualism and cries against regulation and the bogies of socialism. And planning is as natural to modern land use as it is to other types of industry. Industrial planning is for controlled production, controlled prices, profits, and power. Enlightened industry endeavors to keep these tendencies in check so as to keep consumer markets open and the dollar circuit closed. However, society has nevertheless found it necessary to attempt to institute governmental regulation of industry, and in extreme cases, society takes to itself the functions of planning as well as of partial regulation, for it has not been the nature of industrial economy in general to practice conservation of either natural or social resources But we cannot institute or maintain such governmental steps in a democracy without the positive concerted action of the people



It is difficult for a specialist to see a problem whole.



Production is also increased as a result of a variety of investigations, many of which were initiated originally without thought of their possible practical application.

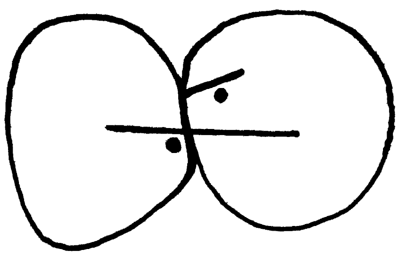
The public apathy, even concerning conservation measures alone, is a reflection of the failure of our educative processes. We have gradually in this country accumulated through the last half century a considerable fund of facts concerning the status of our natural resources. With increasing speed our scientists are learning more and more about plants and vegetation and, more specifically, how we can conduct our agriculture, forestry, grazing, and wildlife management on a sustaining basis. We are discovering the interrelatedness of the elements of the ecosystems, and the futility of attempting to solve human-welfare problems piecemeal. It is becoming clear, for example, that flood control is not just an engineering problem on the lower reaches of the large rivers, but that it involves land-use patterns on the whole watershed; that predator control on range lands is not simply a matter of poisoning or shooting, but is intimately related to overgrazing; that erosion control cannot be ac-

complished by checkdams, or kudzu, or even by individual enlightened farmers alone; and that the fish and game problem is not soluble on a basis of pool- and pen-reared animals. These and all the other conservation problems are too broad for local solution and too complicated for private initiative. They are basically public problems, as are interstate and foreign commerce, war, and the control of atomic energy. Here is where the educatory machinery in conservation has largely failed the general public; and this in spite of the fact that business, industry, and the professions have "sold" the public on technical education.

The failure of general education to date has been the failure to see our problems whole—to see engineering in relation to natural science, natural science in relation to social science, individual problems in relation to population problems and both in relation to resources, and so on, from one class level to another, beyond state and national boundaries, from the present decade to the next century. It is inherent in the human division of labor in our compartmentalized, industrialized economy that it is difficult for a specialist to see a problem whole. But it is just this situation that calls for special effort on the part of all educative agencies interested in public welfare to see the problem whole and to develop a public comprehension at least adequate to allow for a wide public following of enlightened leaders. That is, as Toynbee says, to allow the mimesis by the internal proletariat of the creative minority of society.

It must be remembered that the successful education of the people in conservation is dependent to a large degree on their general education in economic and sociopolitical fields, as well as in a comprehension of the more direct problems of natural resources. Such educative processes are slow, even in countries with a high degree of literacy. They seem overwhelmingly complicated in many countries where the rate of natural-resource destruction is high or where, through past maladjustment, destruction is most advanced, and also where perhaps two thirds or more of the people cannot read.

General education is a concomitant of a high level of living. How can general education come to pass where an abuse of the natural resources is keeping men from reaching a level of living where education about that abuse can be effective? This is the most vicious of chain reactions. We are living at a moment of great and what seems justifiable pessimism, and perhaps man is his own worst enemy. But the dual problems of population control, into the abysses of which we have not looked in this essay, and that of our dwindling resources seem to me ones that have even less likelihood of a solution friendly to man than the control of the atomic bomb.



We are living at a moment of great and what seems justifiable pessimism, and perhaps man is his own worst enemy.

OLD MAN OF THE PRIBILOFS

ALBERT M. DAY

Mr. Day, now director of the U. S. Fish and Wildlife Service, has been with the United States government since his service as a field biologist on the Biological Survey in 1918, and is a national leader in predatory animal and rodent control and in wildlife restoration. He is a national director of the Izaak Walton League and a frequent contributor to magazines and forums.

IN THE heart of Bering Sea, some 300 miles off the mainland of Alaska, lie five dots of land called the Pribilof Islands, named after Gerassim Pribilof, a Russian navigator who discovered them in 1786. Mariners speak of these islands as the "Mist Islands," because they are almost continuously enshrouded in a pea-soup fog

Although rocky, treeless, rain-swept, and wind-lashed, this desolate group of tiny islands is, nevertheless, an exclusive summer resort that annually accommodates nearly 4 million visitors. These summer residents—the Alaska fur seals—come once a year, with unerring regularity, to the Pribilofs to bear their young and to breed, because this fog-bound bit of land is exactly to their liking. They like it so well, in fact, that this is the only place in the world where they have ever been known to set their flippers on land.

From their oceanic wintering grounds, the forerunners of the annual summer migration appear at the islands early in April or May—sometimes as early as March. These are the lordly breeding "bulls," fat and strong from a winter of ease and feasting. They come ashore, sometimes through remnants of the ice along the beach, to fight furiously among themselves for the possession of their favorite places on the "rookeries," or breeding areas. Weighing on an average around 500 pounds, these big animals have been known to top the scales at 700 pounds.

About the first of June the pregnant "cows," or breeding females, begin to appear from the sea and join the family groups, or "harems." Females weigh around 75 pounds and, rarely, attain a weight of 100 pounds. As the newcomers reach shore, each bull seal, or harem master, collects

as many as 40 or more "wives" for his harem. Once a female enters a harem, she cannot depart. Divorces are not permitted among the fur seals. To prevent rivals from kidnapping his mates, the harem master maintains a day-and-night vigil all summer. At this time he is a very dangerous animal and will attack any man or beast that comes near him or his harem.

Within a few days after their arrival, each pregnant female gives birth to one pup, after a gestation period of between eleven and twelve months. Females bear their first pup when three years old, but the males do not mature to the extent that they are able to acquire harems until they are six or seven years old.

At birth the pups are remarkably large in relation to the size of their mothers, averaging about 12 pounds. They are jet black in color, but toward fall they change to the grayish-brown color of the older fur seals. The pup remains on shore for several weeks while the mother makes trips out to sea two or three times a week to seek the fish that will enable her to produce a store of rich milk sufficient to nourish her lusty youngster. Sometimes she has to go 100 miles or more to get small forms of fish life, particularly the squid, which is abundant in these waters.

After these babies are born, and throughout their nursery period, which is also a new mating season, the "Old Man of the Pribilofs" keeps his many mates in his own bailiwick, battles off rivals, sleeps but little, and never eats until the breeding season has ended, subsisting entirely on blubber built up while at sea. During this protracted period of fasting, the bull seals lose greatly in weight and become so thin and emaciated that when the mating season is over they are barely able to make their way across the rock-strewn rookeries to replenish their strength on the rich foods of the sea, which have been almost within a stone's throw of their long and hungry but ever-constant vigil ashore. As his reward for being the head of a large family, however, the "Old Man" escapes with a whole skin, whereas large numbers of the young bachelor seals in the three-year-old class are killed to furnish the basic material for milady's sealskin coat.

From early spring, when the ice floes recede from the shore, until late fall, just before the Arctic ice pack drifts in for the winter, the seal islands are never quiet. The raucous noise made by the roaring and barking of quarrelsome bulls defending their harems from rivals, the bellowing of flirtatious cows seeking to stray from the family circle, and the bleating of newborn pups waiting for food can be heard at sea for miles. Because the Pribilof Islands, in addition to being far off the beaten path of travel, are a special government reservation upon which no person or vessel may land except under stress of weather or by special permission of the Secretary of the Interior, comparatively few persons have ever witnessed this remarkable assemblage of seal life.

When the hectic breeding season finally comes to an end, the summer colony starts to break up as the temperature drops and the violent winds of Bering Sea start their winter wailing. With the season's pups, which have learned to swim, the seals, individually and in small groups, gradually disappear into the depths of the ocean until nature tells them to return. The bleak islands are deserted, and the long migration of the fur seals to warmer winter waters is under way.

Where the seals go on this migration and what they do is now the subject of an intensive investigation by the U. S. Fish and Wildlife Service. Fishery scientists want the answers to such questions as these: What species of marine life do they eat? In what proportion do they consume this food on their migrations? Are they an economic hazard to commercial fishermen? On their migrations do many of the seals travel along the Asiatic coast? To what extent does the killer whale prey upon pup seals when they take to the water?

The answers to these questions are worth considerably more than $64 to the United States government because the potential value of the Alaska fur seals today is well in excess of $100,000,000. They comprise about 80 percent of the priceless fur seals of the world—the raw material for soft, sleek, and expensive coats in which smart women the world over have luxuriated ever since sealskin history began in the days of Russia's Catherine the Great.

The fur seals (*Callorhinus alascanus*), which the United States government manages, are not to be confused with the common hair seals, which are widely distributed over the world. The latter do not have the soft underfur that characterizes the Alaska fur seal and makes it so valuable. The seals that frequent San Francisco's Seal Rocks, to the delight of visitors, are for the most part sea lions—so familiar to circus fans—and hair seals.

Alaska fur seals are mammals that live the greater part of their lives in water. Structurally, they have much in common with bears, except that they are adapted to an aquatic life, whereas bears are terrestrial animals. Instead of feet, seals have

flippers, but when they come ashore they can travel at a fairly rapid rate, at least for a short distance. So much do fur seals resemble bears in their general structure that more than two hundred years ago George Steller, the great German naturalist who accompanied Vitus Bering on his voyage of discovery to Alaska in 1741, described them as "sea bears."

These seals belong to a species distinct from any other fur seals. Other species are found on the Commander Islands, off the Siberian Coast, and on Robben Island, in the eastern part of Okhotsk Sea, now under the jurisdiction of Russia. Fur seals of other species are found also on Lobos Island, Uruguay, off the Cape of Good Hope, Africa, and to a very limited extent in other cold parts of the Southern Hemisphere.

Navigator Pribilof discovered the islands named for him only after a prolonged search had been carried on by the Russians to locate the breeding grounds of the fur seals that were so numerous about the passes of the Aleutian Islands. The Pribilof Islands, at the time of their discovery, were uninhabited, and there is no concrete evidence to indicate that any human being had ever visited them previously. They remained under Russian management for eighty-one years, until 1867, when the United States purchased Alaska and acquired the islands as part of the Territory.

The group consists of five islands, of volcanic origin, three of which are small and relatively unimportant in seal history. St. Paul Island, the largest, is about 14 miles in length. Forty miles away, by water, is St. George Island, 12 miles long.

It is probable that before discovery the Pribilof herd may have contained as many as 4 million animals. Records indicate that prior to 1834 about 2 million pelts were taken under Russian auspices, and by that year the herd had become so reduced in numbers that restrictive measures were applied. From 1835 to 1867, about 600,000 pelts were taken at the Pribilof Islands, and in this period of restricted killing the herd increased to probably 3 million

The number of seals in the herd when Alaska came into the possession of the United States has been variously estimated at from 2 million to 5 million animals. During the seasons of 1868 and 1869, the first two immediately following the purchase of Alaska from Russia, when killing was unrestricted, about 329,000 fur seals were killed by various independents.

For a period of forty years, from May 1, 1870, to May 1, 1910, the right to take fur-seal skins on the Pribilof Islands was leased by the government to private corporations. The annual take of sealskins under the first lease, which ran from 1870 to 1890, was limited to 100,000 skins, and the total for the twenty-year period was 1,977,377. The annual rental and tax brought a total revenue of $6,020,152 to the government for that period.

Under the second lease, however, there was only one year—1896—in which the take amounted to as much as 30,000 skins, and the total obtained by the lessee during the twenty years ending May 1, 1910, was 342,651 skins, with the government receiving a revenue of $3,453,833. In 1910 the leasing system was discontinued, and since that time, under the acts of April 21, 1910, and August 24, 1912, the Alaska fur-seal herd has been administered, first by the Secretary of Commerce and now by the Secretary of the Interior, through the Fish and Wildlife Service.

In August 1948, when the annual census computation was made, the herd numbered 3,837,131 animals. Since 1910, when the government assumed direct control of the fur seals, 1,498,911 sealskins have been taken, worth approximately $40,000,000. Translated into terms of fur-seal coats, which require from six to eight skins apiece, this total has produced around 200,000 coats.

Fur-seal pup, Morjovi Rookery, St. Paul Island.

Early-season fur-seal harem, N. E. Point Rookery, St. Paul Island, Alaska.

Sealing operations as they are conducted today by the Fish and Wildlife Service on the Pribilof Islands are confined exclusively to the killing of surplus immature males, chiefly of the three-year-old class, designated as bachelors. Considering the number of animals available, the size of the skin, and quality of the fur, the three-year-old males yield pelts of maximum value In the older males the pelts are of little value. The habits of the fur seals while on land result in the young males herding by themselves, and this makes it possible to drive and kill three-year-olds without disturbing the breeding animals. No female or breeding bull is ever killed intentionally.

The best season for harvesting the crop lies between June 15 and July 31, a period of 47 days. This is the time when the three-year class is dominant on the bachelor beaches and the fur is prime. Earlier, the older males are more abundant, and, later, toward August, two-year-olds and nomadic females swarm onto the beaches. In practice the season is often extended as much as a week in either direction to compensate for natural variations in the time of arrival of the young seals.

Adjacent to the breeding rookeries on the Pribilof Islands are places known as the "hauling grounds" where the young immature male seals, or "bachelors," as they are called, come ashore to enjoy a change of scenery and to acquire knowledge of their future homesites against the time when they will be ready to set up housekeeping for themselves. Not all these young fellows are destined to join a family circle, however, and it is well that they are not. Because of their great abundance and belligerent nature when they come to maturity at six years, many tremendous battles would result in the trampling to death of numerous pups if the breeding grounds were overcrowded with adult males.

It is from these hauling grounds that the seals selected for killing are driven inland a short distance. They can be driven almost as easily as a flock of sheep, but because extensive land travel is foreign to their habits of life they can go only a very short distance before they must rest. These driving operations, therefore, must be conducted with extreme care so as not to overheat the animals and thus lessen the value of the pelts.

Rainy or humid weather is preferred for the seal killing, which is done under the immediate direction of the Fish and Wildlife Service by the resident Aleuts, descendants of the people moved there in early days by the Russians for the purpose of utilizing the fur resources of the islands. These Pribilof natives, now numbering about 500, are in effect wards of the government. They are paid a fee of $2.00 for each sealskin taken on St. George Island, where operations are on a comparatively limited scale, and $1.20 for each skin taken on St. Paul Island. Their primary compensation, however, is through the provision by

the government of all necessaries of life, including schools and medical aid.

In the fur seal, polygamy is perhaps more highly developed than in any other mammal. This fact makes it possible to kill the surplus bachelor animals without decreasing the number of young that may be born. Although the average harem contains about 40 cows, there are records of more than 100 cows in a single harem. The natural ratio of breeding males to females is about 1 to 26. Under the existing system, where only the young males are killed, the ratio approaches 1 to 50.

A suitable reserve of three-year-old males is made each year for breeding stock. The number of this age class to be reserved is determined from observations as to the increase in the herd, the number of breeding males available, and the average size of the harem.

After the animals selected are killed, the skins are removed, washed, blubbered, and given a thorough curing in salt for at least ten days. They are then rolled singly with a generous supply of salt on the flesh side, which is turned inward. Boric acid also is used as a germicide in preserving the skins. From 80 to 100 of the skins are packed to the barrel for shipment.

Prior to 1913 the fur-seal skins taken on the Pribilofs were shipped to London for sale in a raw, salted condition. In addition to being the world's chief sealskin market at that time, London was the principal center for the dressing and dyeing of fur-seal skins. Most of the Alaska sealskins were returned to the United States for use after being dressed and dyed in London. Today the government has a contract with the Fouke Fur Company, of St. Louis, for dressing, dyeing, and selling the skins at public auction.

The process of preparing these skins is a most difficult one, because more than 125 distinct manipulations or treatments are involved. This work requires about ninety days. After grading, the skins are washed to remove all surplus grease and dirt. Nature has given the Alaska seal a guard hair to protect the fine silk underfur which insulates the cold and dampness from the skin of the animal. It is this soft silk underfur that, when dyed, produces the lustrous fur of very high wearing qualities. The skins are subjected to considerable dry heat until the guard hair is loosened and can be removed without damage to the fur. After this guard hair is extracted, the pelts are put through a chamois tannage—no chemicals are used, just good quality oils, which give the same fine soft feel to the leather side associated with the finest type of chamois glove.

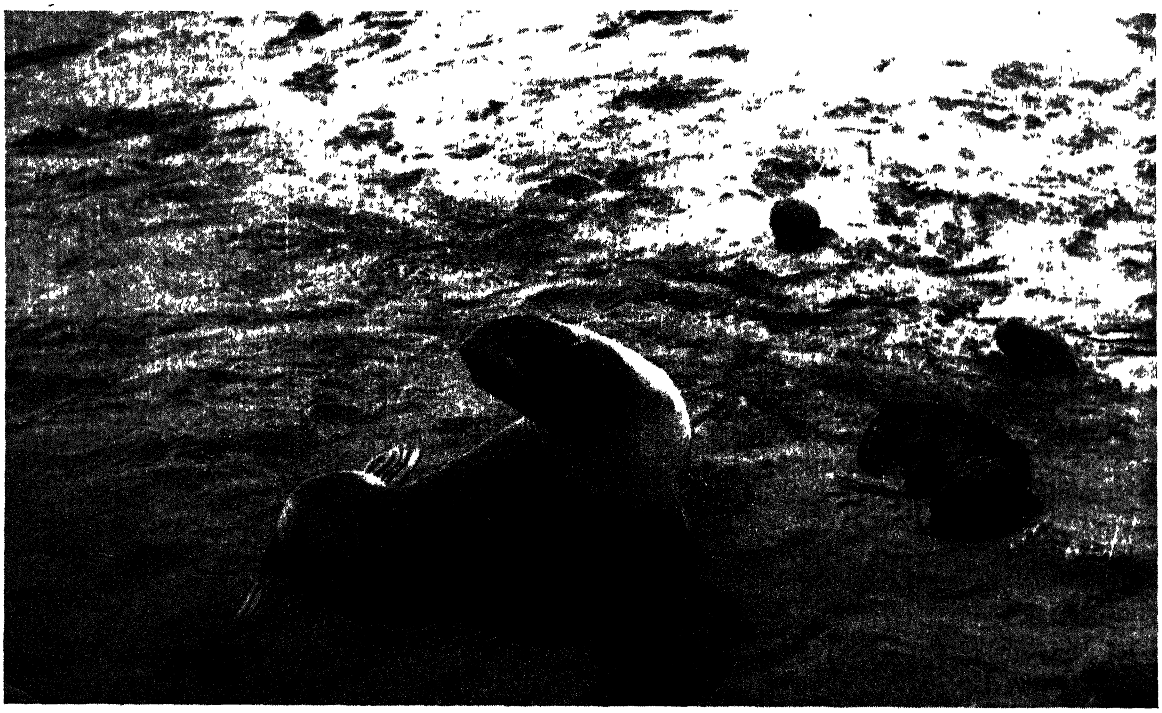
Then comes the dye process. Contrary to general information, the fur of the Alaska sealskin is naturally curly, very much like a lamb, and it is the dyeing process that straightens the fur and gives to it the silky, lustrous glow. A grounding solution and numerous applications of dyes are brushed into the fur, producing a permanently straightened fur of exquisite texture. After the dyeing operation is completed, the leather is buffed down to the required thickness; the finishing operation cleans the fur, exposing the true beauty of Alaskan sealskin, and produces a leather that has such a high degree of pliability that it can be draped and molded as easily as fine cloth.

These skins are literally as temperamental as opera singers and have to be handled with the greatest care and skill. After they are finished, they are segregated into various grades, sizes, and lots and are sold to the highest bidder on each lot at a public auction, held twice a year in April and October at the Fouke plant in St. Louis. The net proceeds from these sales are turned over to the Treasury of the United States.

Through careful management and scientific study, the United States government has built up this great herd of fur seals to its high of some 3,800,000 animals in 1948 from a low of about 132,000 in 1910, when it assumed direct management of the herd. At that time it was evident that something had to be done immediately to save this great natural asset. The fur-seal herd had been brought perilously close to extinction through pelagic sealing—the indiscriminate killing of seals while they are at sea. The practice began as a commercial enterprise about 1882 and reached its height in 1894, when approximately 61,000 skins were taken at sea by pelagic sealers.

Pelagic sealing was both cruel and economically wasteful. Only about one out of five animals killed was actually recovered by the hunters before the carcasses sank and the skins were lost. This practice was destructive of males and females alike. After the young were born and while they were still on the island nourished by their mother's milk, each mother seal killed at sea meant the loss of another seal, its pup, which was left on the island to starve. The mother seal does not nurse any but her own pup. Unborn pups were lost if the female seals were killed on their way northward to the breeding grounds, since these pups are born shortly after the females land on the islands.

Pelagic sealing in the north Pacific Ocean was not confined to the nationals of any one government, and with the increase in operations at sea



Fur-seal mother and pup, Little Polovina Rookery, St. Paul Island.

it was soon realized that only by an international agreement could the Pribilof Islands herd be conserved. Diplomatic negotiations with regard to the matter extended over a period of years, and it was not until July 7, 1911, that effective international protection was given to this herd. On that date a convention was concluded between the United States, Great Britain, Japan, and Russia which became effective on December 15, 1911. For the first time subsequent to the development of pelagic sealing the way was cleared for effective conservation and use of the Pribilof Islands fur seals. This treaty was scheduled to run for fifteen years, and indefinitely thereafter until modified or abrogated. After fourteen years any of the four countries signing the treaty could give one year's notice of a desire to modify or cancel the agreement.

An outstanding feature of the convention was that it prohibited pelagic sealing in waters of the north Pacific Ocean north of the thirtieth parallel of north latitude and including the Seas of Bering, Kamchatka, Okhotsk, and Japan, except for the limited operations by primitive methods carried on by Indians and other aborigines dwelling on the coasts of the protected waters.

This convention, commonly known as the North Pacific Sealing Convention, also afforded protection to the Japanese fur-seal herd at Robben Island, estimated to contain not more than 50,000 animals, and the Russian herd at the Commander Islands, with probably fewer than 100,000 animals.

In return for the surrender of such profits as their nationals had been deriving from pelagic sealing operations, an allotment of 15 percent of the fur-seal skins taken annually on the Pribilof Islands was made to both Canada and Japan. Russia was a signatory of the treaty only in the interest of the seal herd on her side of the Pacific. Throughout the life of the treaty, Japan took her share in cash, which amounted to more than $1,500,000. Until 1933 Canada took her share in cash, but after that date she often elected to take actual delivery of her share in skins.

Under the terms of the Convention of 1911, and the wise management practices employed by the United States government, the seal herd continued to increase year after year. It was not until 1940, however, that the harmonious relations existing among the signatories were disrupted when Japan served notice on our government that on October 23, 1941, it would abrogate the Convention. Japan alleged that the fur-seal herd, at least in part, migrated down the Asiatic coast and had grown so large that it was devouring valuable food fishes essential to the economy of such a fish-eating nation.

The records and findings in the possession of the United States government at that time, developed by the United States and Canada over

many years, indicated, however, that the migrations of the seals were primarily along the eastern side of the Pacific. Moreover, studies of stomach contents disclosed that the fur seals fed largely on squid, pollock, seal-fish—a small deep-water fish—and other noncommercial species; very few salmon were eaten.

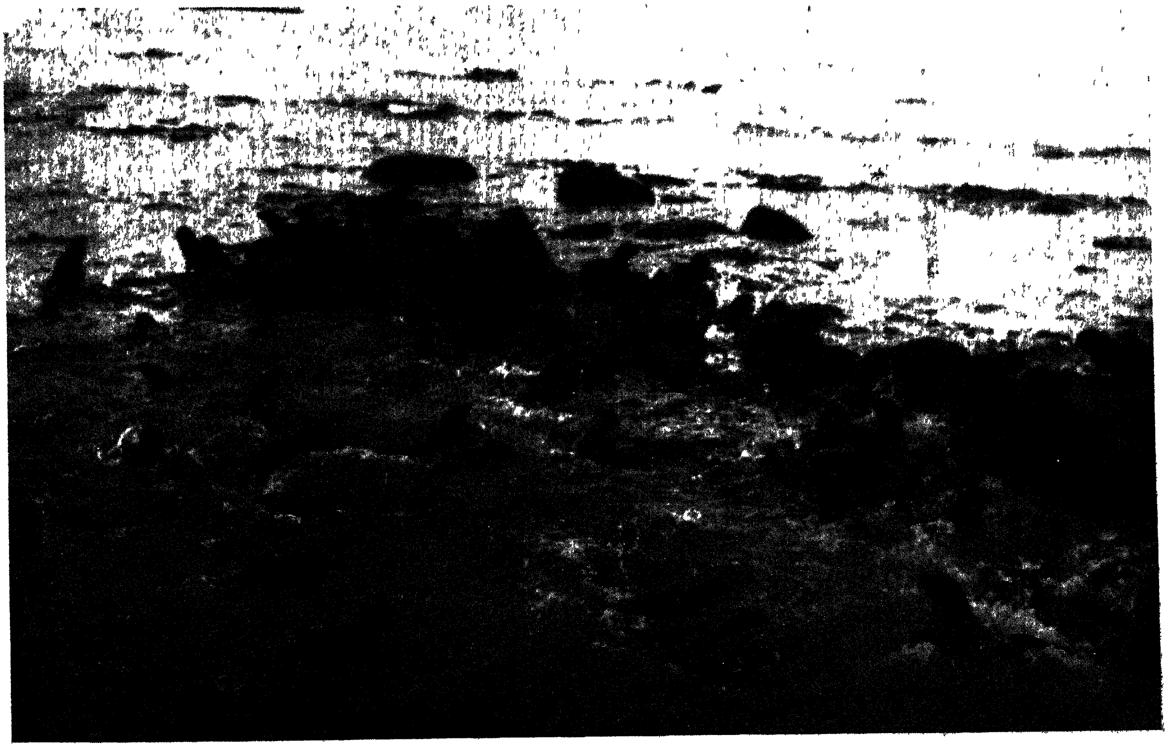
To bring the United States data up to date, the Fish and Wildlife Service began to make plans for an extensive investigation of the migratory and feeding habits of the fur seals, as well as the entire life history of these animals and of their relationship to the fisheries and to other economic interests

On June 30, 1941, the Seventy-seventh Congress appropriated $290,000 to the Service to start the fur-seal investigation. From these funds the Service purchased the three-masted schooner yacht *Black Douglas* and began work at Savannah, Georgia, to equip it as a floating laboratory for studying the seals and their alleged poaching during their long migrations on the high seas. Before the vessel ever reached Seattle, however, war was declared and a submarine in Pacific waters nearly ended the career of the *Black Douglas* and its crew. By blacking out, and cutting its motor so the submarine could no longer trace that sound, the vessel escaped by using sail. When the *Black Douglas* arrived in Seattle it was requisitioned by the Navy, its scientists and crew disbanded and returned to their homes, and the seal investigation shelved "for the duration."

The matter of protecting the fur seals, however, was not neglected during the war. A provisional agreement for their protection was signed by Canada and the United States in December 1942. On February 26, 1944, the President signed a new fur-seal law to give effect to this provisional agreement. With enforcing legislation by the Canadian government, the agreement provides, among other things, that 20 percent of the skins taken on the Pribilof Islands shall become the property of Canada, the remainder to be retained by the United States The Act of February 26, 1944, brings together, with only minor changes, all previous legislation directly affecting the Pribilof Islands fur-seal herd.

In May 1947, re-equipped as a floating laboratory, the *Black Douglas* left Seattle, Washington, for the Pribilof Islands to seek new data on where the seals go when they leave the islands, what food they eat, and whether the young seals are preyed upon by other marine mammals.

As part of their work during the summers spent on the islands, Service biologists attached metal tags to the flippers of thousands of seal pups in



Fur-seal pups playing in the surf and learning to swim Vostochni Rookery, St. Paul Island.

each of the years 1947 and 1948. Recoveries of these tagged seals at sea during the coming years will yield specific information on the migration habits of the seals. In 1950, when the 1947 crop of pups will be ready for killing, a statistical study of the recoveries of marked three-year-olds on the killing grounds will make possible a check on the accuracy of seal census methods now in use.

From observations made to date of tagged seals that return to the islands, the fur-seal experts have already obtained valuable data. For example, we know that as seals mature they tend to return in increasing proportions to the exact area of their birth. We also know that seal cows may produce pups when they are twenty years old. We have been able to get exact figures on the rate of growth of seals, as well as a variety of other facts that are essential in the management of the herd.

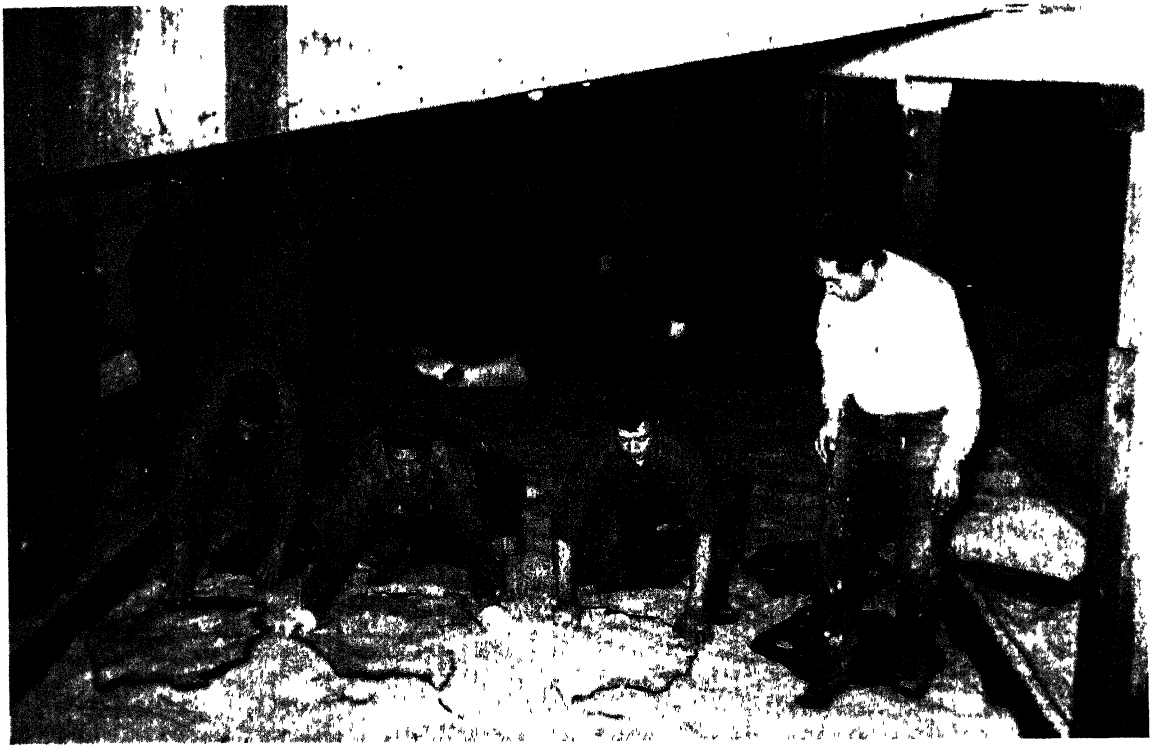
Observations on fur seals in captivity have contributed information obtainable in no other way. Such observations have indicated that the daily food requirement of a medium-sized seal is about 10 pounds. The only seals in captivity now are six in the Balboa Park Zoo, in San Diego, California. On August 8, 1948, one of them gave birth to a healthy youngster, the first ever to survive birth in captivity, after a gestation period of at least 374 days.

The degree to which the fur-seal herd may be further developed is another point scientists are seeking to determine. The causes of mortality, also, are not well known, since most of them occur while the seals are at sea. The losses are probably greatest when the young pups, at the age of four months, leave the islands in November and venture into the stormy and treacherous waters of the Pacific. Many of them are thought to be destroyed by killer whales. In the stomachs of two whales stranded on the island, 18 young fur seals were found in one and 24 in the other. Hookworm is also causing the death of an increasing number of newborn seals.

Wringing excess sea water from freshly blubbered skins; action is from right to left.

When these current investigations, which will cover a period of years, are concluded and the results analyzed, the Service expects to have available a wealth of authentic data on the migrations and food habits of the fur seals and their relation to other fishery resources of the north Pacific to use as the basis for its work of protecting and perpetuating these valuable natural resources.

Fortified with new scientific data, the United



Salting fur-seal skins on St. Paul Island

States government will be in a position to combat any demands for the return of the days of unbridled exploitation of the fur-seal herd. Without the immunity from pelagic sealing which the North Pacific Sealing Convention gave to the fur seals, the herd might be commercially extinct today instead of being an important source of revenue to the government. When the Convention became effective in 1911, the government was enabled to manage the herd in accordance with a scientific program of conservation and utilization. That the herd is nearly thirty times as large today as it was in 1911 is proof of the success of this program. To return, then, to the former destructive practice of pelagic sealing would undo what is regarded as the world's outstanding achievement in the restoration of a great wildlife resource through international cooperation.

AN AMERICAN PASCAL: JONATHAN EDWARDS

RUFUS SUTER

After receiving his Ph.D. from Harvard in 1932, Dr. Suter went to the Library of Congress as cataloguer in philosophy and, later, as a Fellow in the Asiatic Division. He is now with the Army Map Service, Washington, D C. Dr. Suter has written on Francis Bacon, Blaise Pascal, and Descartes for THE SCIENTIFIC MONTHLY.

AS paradoxical a personality as any to be found in the novels of Dostoievski was Blaise Pascal. A founder of modern experimental physics, he belonged to the distinguished company of Galileo, Torricelli, Gilbert, and Boyle, and as a mathematician he was the peer of Descartes. Yet at the height of his powers he allowed his scientific interests to be smothered by what some of us today would call a pathological obsession with the morbid side of religion.

America also had a Pascal. Although his scientific achievements were not equal to those of the young man from Clermont, he showed in his boyhood signs of an aptitude that might have flowered if a religion as lugubrious as that of his French predecessor had not nipped it in the bud. The resemblance is the more striking since the theology that monopolized the energies of both men was the same. Pascal became the champion of a community, now extinct, within the Church of Rome, known as Janssenism. Its teachings were indistinguishable from those of Protestant Calvinism, because it derived from the same source: a somber convert of the ancient world, Saint Augustine. The American Pascal was the last great defender of the abstract principles behind New England Calvinism, known more familiarly as Puritanism, the religion of the Pilgrim Fathers and the Mathers. This American Pascal, grandfather of Aaron Burr and grandfather-in-law of Eli Whitney, missionary to the American Indians and president of Nassau Hall, was Jonathan Edwards. He was born in 1703 at East Windsor, on the banks of the Connecticut.

Edwards' power as a psychologist and psychoanalyst is comparatively well known. What makes the resemblance between him and Pascal striking is that in his case, as well as in that of the Janssenist, a precocious early insight into the problems of physical science was sacrificed to the religious passion. That Edwards, while still in his teens, wrote eight sheets of foolscap treating brilliantly, among other topics, of physics, meteorology, and astronomy,* is little known. He did not have the genius of Pascal. But his youthful scientific aptitude may be compared favorably with that of many another eminent man, such as Franklin Kant, or Swedenborg, whose early bent toward science was later eclipsed by other interests. An examination of some of the high lights of these eight sheets of foolscap is worth the attention of any student of the history of American science.

I

The method of inquiry Edwards adopted was after the geometrical model. He uses definitions, axioms, postulates, corollaries, lemmata, etc. He introduces geometrical diagrams. His technique of proof is either straightforward deduction, or a technique he subsequently applied in his theological writing: demonstration that the contradictory of the proposition to be proved is untenable. There is evidence of meticulous observation, but no controlled observation in the sense of laboratory experiment. Many of his ideas are put forward as hypotheses, to be investigated later.

As with some modern fundamentalists, Edwards' astronomy is uninfluenced by the infallibility of Genesis. This is astonishing, since, as the literalistic critics of Galileo (Protestant as well as Catholic) knew, Genesis supports the geocentric view of stellar and planetary motions, by implication if not explicitly. But the opinion of Urban VIII, Luther, Calvin, and Wesley made no impression upon the Yaleman from East Windsor. who shows not the least hesitation to accept the astronomical system of Galileo and Newton. In a paragraph about the frail human tendency to set

* This material was collected and published under the heading "Notes on Natural Science" as part of an appendix to an anonymous biography of Edwards. The biography is Vol. 1 of *The Works of President Edwards: with a Memoir of his Life*. In ten volumes. Vols. 1, 3–6, 9–10, pub. by G. & C. & H. Carvill, New York, 1830; Vols. 2, 7–8, by S. Converse, New York, 1829–30.

up familiar experiences of everyday life as standards of possibility and impossibility, he points out that even among the learned are some who, to ease their imagination, are ready to fall back into the antiquated system of Ptolemy, merely because they cannot conceive how the fixed stars can be so distant "as that the earth's annual revolution should cause no parallax among them."

His knowledge of the remoteness of the fixed stars led him to a proof of the impossibility of their diurnal revolution around the earth. If they are to encircle the earth in twenty-four hours, their speed must be at least ten times the speed of light (in fact, it must be several thousand times). Edwards then illustrates by a diagram that the light emitted from the stars would be tossed off at a tangent to their orbit and would miss the earth. If the Ptolemaic notion were true, nobody on the earth would ever have seen a star.

This knowledge of the vast distance of the fixed stars also convinced the youthful Yaleman that the stars are suns. At their distance they cannot shine by the reflected light of our sun. Old Sol is, conversely, a star.

In view of the preoccupation of many astronomers of the past twenty years with the sidereal system, of which our family of planets and even the galaxy are a small part, another line of astronomical reasoning by Edwards is of interest today. He suggests the possibility that the universe, or "Starry World," is a spheroid. The proof, he believes, would be forthcoming from observation of the Milky Way. We could determine our position within this spheroid by observing how far the galaxy departs from being a great circle This would give us our vertical distance from the galactic plane. We could, then, observe the ratio of brilliance of opposite sides of the Milky Way. This, compounded with several other ratios, would give us our horizontal distance from the center of the plane we occupy.

Outside this spheroid, the matter must be evenly placed in order that the gravitational pull should not upset the nice adjustments within the system. The necessity for this uniform distribution might be avoided if the spheroid rotated; but we must outlaw this rotation, because it also would cause disturbances in the perfectly symmetrical and regular motions of the planets, comets, and other journeying, whirling masses of matter within the spheroid.

Edwards' cosmological ideas, thus far, are in tune with the modern note. We know that our local sidereal system is lens- or watch-shaped, and not a spheroid. But the difference is not too great; and the determinations have been made, as Edwards foresaw, by study of the Milky Way. We have our so-called Island Universes, such as the Nebula of Andromeda, which are matter outside the spheroid of our universe, although they are not evenly distributed. In view of this modernity it is by surprise that we finally catch a weird echo from the medieval concert. Our future Calvinistic theologian wonders whether there may not be an enclosure to the spheroid, and whether this shell, if it were absolutely solid, could not withstand violent shocks by gigantic bodies—as if Lucifer might declare war on the universe.

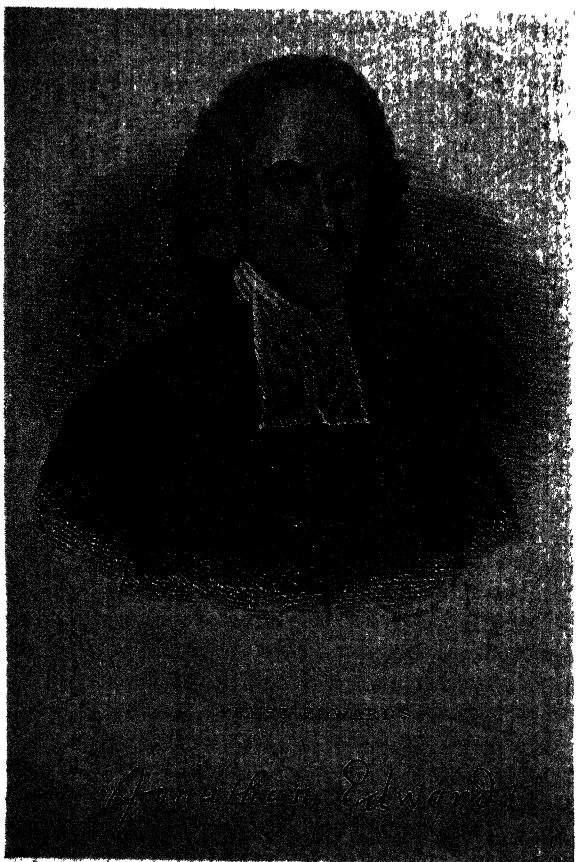
The idea of a spheroid of the Starry World suggests the image that the sidereal system may be a particle—a drop of water, say—in a universe at a higher level of magnitude; or that a particle on a blade of grass may be a sidereal system at a lower level of magnitude. This fancy Edwards unceremoniously rejected. The same speculation fascinated Pascal, who saw an infinite series of telescoping universes. The reason for Edwards' objection is not clear, but we surmise that his general stubborn resistance to read infinity into the size of the physical world was at the bottom of it. Infinity in the mathematical sense of approach toward a limit was beyond Edwards' horizon. He seems to have thought of infinity as a concrete fact, a degree of being actually real. To attribute infinity to the dimensions of the physical world may have appeared to him a kind of blasphemy, although he had no sensitiveness about seeing infinite forces acting in the physical world.

The young man at Yale had several other astronomical insights. The precession of the equinoxes he understood to be the effect of a gyration of the earth's poles. He sets the period of this gyration at 25,200 years, instead of at 25,800 years, which is the period stated in modern texts. He had some inkling of the physical condition, vast size, and terrific temperature of the sun, and he shared in the eighteenth-century literary fancy that the moon is inhabited.

We shall find that Edwards' meteorological insights are often on the right track. The grand problem: Why is winter colder than summer? he met with four solutions, three of them clarified by geometrical diagrams. The principle is that the relative nearness of the sun to the horizon in the winter, hence the obliqueness of its rays, accounts for the winter coolness. The sun's high altitude, on the other hand, in the summer, hence its chance to hit the ground with perpendicular beams, explains the summer heat. Two of the solutions are ingenious enough to be looked at in detail. The

first of them presupposes the theory that heat is a violent agitation of atmospheric particles. In the summer, when the perpendicular rays of the sun strike the earth's surface, the reflex rays retrace. more or less, the lines of their incidence, in accordance with a well-known law of optics. With this much beating back and forth along the same road, the atmospheric particles joggle each other into a fury. Temperature, in other words, is high. In the winter, with the sun shining low on the horizon all day long, the situation is different. The oblique rays, even if they struck a smooth surface, would be reflected at an obtuse angle, so that most of the reinforcement of the struggling atmospheric particles would be lost. As facts are, the surface of *terra firma* is rough, and reflex rays are struck off indiscriminately at every which angle. Summer's reinforcements are lacking; the particles beat each other into only a relatively thin, lethargic turmoil. Winter temperatures, consequently, are low.

Edwards' other ingenious explanation is one to be found in modern textbooks. A sheaf of parallel rays striking a surface oblique to them touches it at points further apart—hence over a larger area—



than the same sheaf of rays would if impinging upon a surface perpendicular to them. This is a simple fact of projection. To express the same phenomenon otherwise: Struck horizontally, a unit area on the earth's surface receives fewer rays than when struck perpendicularly; hence winter is colder than summer.

Another happy meteorological thought of Edwards has to do with why thunder starts with a clap near at hand, then rumbling and muttering seems to roll away into the more distant reaches of the sky. The lightning, on the other hand, is over in a flash. Edwards' analysis of this phenomenon is as neat as any to be found among the recorded boyhood cogitations of Pascal The lightning, to be sure, is over in a flash; but the flash extends through a space, one end of which is likely to be further from us than the other. Sound travels more slowly than light. It is thus to be expected that while out there, where the lightning happens, the thunder is over as quickly as the flash, the sound reaches us successively, in accordance with the difference in distance from us of the various points along the flash. Inevitably, in fact, we occasionally hear thunder backwards. If the start of the flash is further out in the sky than the end, the thunderclap which accompanies the violent climax of the flash may take less time to reach us than the noise of any of the antecedent parts of the flash; so that the pandemonium seems to be in reverse order to the actual events: the clap apparently sounds first, and the din of Rip van Winkle's rolling pins. seems to recede subsequently into the distance.

Edwards explained lightning as an almost infinitely fine combustible matter, floating in the air, that becomes incandescent by a "sudden and mighty fermentation." Once aglow, this "fluid matter" divides the air as it darts, each moment receiving new impulses by its continued fermentation. The zigzagging directions of its flight are determined by the differences in atmospheric temperature. Its particles are "so fine, and are so vehemently urged on, that they instantaneously make their way into the pores of earthly bodies, still burning with a prodigious heat, and so instantly rarifying the rarifiable parts."

Our Yale pupil here is struggling to express the identity of lightning and electricity a generation before Franklin's kite experiment. It is interesting to note that the continued usage today of such terms as current, direction of flow, resistance, etc. is a throwback to the days when electricity was generally considered a "fluid matter."

Edwards understood correctly the nature of

clouds and rain. He recognized that the twinkling of the stars is an atmospheric phenomenon, and he explained the fact that the stars twinkle though the planets do not as a result of the comparative slimness of the parcel of rays from bodies so distant that they are points of light. The least disturbance in the earth's atmosphere is bound to interrupt what would otherwise be as steady a glow as that of Venus. Edwards, however, was confused in his understanding of the atmosphere. He appreciated that it is, in part, composed of vaporous exhalations from the earth (tiny bubbles) drawn up by the sun and, perhaps, by the force of attraction of the clouds, but he thought that an important part of it is also the particles of the ether, packed more thickly near the surface of the terrestrial globe (or of any planetary globe) than in empty space, because of the power of attraction.

In our survey of Edwards' astronomy and meteorology we have already mentioned, in passing, several of his inspirations in general physics. He knew that light travels at a finite speed. He evidently had adopted Newton's corpuscular theory of light. He also knew that sound travels, and vastly more slowly than light. He recognized that sound is a vibration in the air. He knew that heat is a violent agitation of particles. He was deeply impressed by Newton's law of gravitation. He alludes once (a point we have not mentioned) to Newton's third law of motion, that every action has an equal and opposite reaction. He understood (another point we have not mentioned) that air has weight, and that bags from which the air has been sucked do not collapse because of any horror of the void, but because the weight of the surrounding air pushes them in. He was probably convinced of the doctrine of the conservation of matter.

II

Edwards is most exciting, however, as a theoretical physcist. His discussion of the atom is a fascinating bit of scientific curiosa.

The conventional picture of the atom as the particle having the quality of *least reducibility* (a purely mathematical attribute) he rejects and substitutes in its stead an entity having the quality of what he calls *indiscerptibility* (a mechanical attribute). An atom, that is to say, is a thing that cannot be fractured by the existence of any finite force. Or, to put the distinction in another way: Conventionally, when a person says that an atom is a body so small that it cannot be divided, he means by "cannot" the inability in a geometrical sense to halve any further. The last term in a series of halvings has been reached; space proves to be geometrically incapable of further division. The difficulty with this notion, of course, is that extension may be infinitely divisible, in which case there could be no atoms. For Edwards, however, "cannot" means "not enough strength" to halve any further. Any finite force of pressure, torsion, collision, attraction, or whatnot would be too weak to break the atom. The question of whether space is or is not infinitely divisible is irrelevant to the problem of the existence of the atom. An atom may be as large as the universe. On the other hand, millions upon millions of submicroscopic atoms doubtless exist. Size has no bearing upon their nature.

Edwards goes into detail in the analysis of indiscerptibility. An indiscerptible body is a *plenum*. That is, it is absolutely full and perfectly solid. This does not mean, however, that an atom may not be honeycombed with pores—or even that there may not be more empty than filled space in it. A thing is *plenum* when every part within it is in "contact by surface" with some other part, all of which parts may run up and downways and crisscross between the pores.

The term "contact by surface" is significant. Edwards emphasizes a distinction between "contact by surface," which is true physical contiguity, and "contact by point or line," which is an abstract, ideal, mathematical contact to be found in the conceptual world of textbooks on geometry. Countless millions of points or lines of contact would not generate a single contact by surface. But whenever surface contiguity happens in nature, the touching bodies become indiscerptible, become one *plenum*, parts of one self-identical atom.

We might suppose that eventually an inconveniently large number of bodies would touch by surface, so that the universe would gradually coagulate. Probability makes this unlikely. The chances are only one in many millions that any two atoms would have such shapes that any part of the surface of one could perfectly dovetail into the surface of another; or that, if so, these fortunate ones should happen to meet; or, if they should happen to meet, that the proper faces should be turned toward each other. We may assume, however, that such unions do happen in immense numbers at submicroscopic levels.

Edwards betrays the sound scientific instinct to generalize, by trying to identify gravity with the indiscerptibility aspect, or the aspect of perfect solidity, of the atom. One step in this process is almost bizarre, though its precocious ingenuity cannot be denied. The indiscerptibility of the atom, he

attempts to show, is the cumulation of an infinitely large quantity of gravitational force, generated by the mutual pulls of the infinitude of material parts within the atom. The proof is an application of Newton's law of gravitation, F equals Mm/d^2, to a geometrical representation of an atom. The infinitesimal qualities of F are supposed to add up to an infinite quantity of F, or cohesive force holding the atom intact.

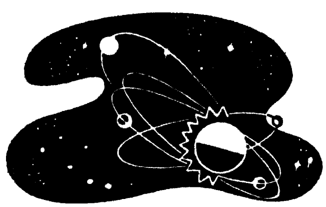
The concept of the indiscerptibility of the atom is an interesting logical toy to play with; but if Edwards were alive today the recent splitting of the atom would force him to revise his views.

The last step of Edwards' theoretical physics is a transition to metaphysics—a dénouement occasional among modern physicists. He concludes that matter is solidity. If some thinkers have conjectured that matter is extension (he may have Descartes in mind), the rebuttal is: Extension of what? Obviously, extension of solidity. By making the appropriate substitutions we may reach the strictly equivalent proposition that body *is* gravity. Newton thus has the first say in metaphysics as well as the last say in physics.

There is an analogy between the European students of Plato and the older school of Edwards' scholars. The former have commonly shown an antipathy toward regarding Plato, or any of the other Greeks, as indebted to any culture prior to ours outside of Hellas—to India, for example. Similarly, there has been a tendency to regard Edwards as either wholly original in his nontheological thought, or at most as deriving it from a purely New England background. Much has been written to prove that his boyhood metaphysical idealism was not suggested by Berkeley, but sprang full-blown from his own mind. It is, indeed, difficult to reach any certain conclusion about how much of Edwards' scientific and philosophical speculation was original In the very foolscap we have been reviewing he states as an ethical rule never to appear to be widely read by alluding to learned authors. It is a paradox that this rule, intended as a discipline against boasting, may have had the effect of causing Edwards to appear to later generations as more of a genius than he was. We know at least that he was familiar with Locke's *Essay*. In the eight sheets of foolscap the only authors he names are Newton and the materialist Hobbes, whom he dismisses.

Whether Edwards' scientific opinions were mostly suggested by reading of European authors or not, it should impress the student of the history of American science that within little more than a century since the death of Gilbert, and within less than a generation since the death of Boyle, the essentially modern scientific world view should have seeped down to a boy in a college in a remote colonial town.

The world no doubt lost in Edwards a philosopher and possibly a great scientist. The tragedy in his case is more complete than in Pascal's for whereas the religious community to which the Janssenist sacrificed his science has become defunct, Pascal's tract, the *Pensées* still has something to offer the devout soul. But the harrowing theology of Edwards, which swallowed up both his natural philosophy and his science, is dead even in the Congregational churches that nurtured him and the other Puritans.



ANTHROPOLOGY: A BRIEF SURVEY

ROBERT W. EHRICH

Dr. Ehrich, of the Department of Sociology and Anthropology, Brooklyn College, first prepared this article to inform European colleagues of trends and developments in American anthropology since 1939, when the international exchange of information was interrupted. In its original form it appeared in the Polish annual Lud, *1948,* XXXVII. *In a somewhat revised form, it is presented here for an American audience.*

THIS article is an attempt to synthesize some of the trends and developments in the field of anthropology during the past ten years. Intense activity in the various specialties has added much new material, contributed many new concepts, and drastically revised many older ones. Limitations of space preclude a detailed treatment of any one subject, and only those aspects that seem of outstanding importance will be mentioned. Even so the list is far from complete. The discussion and bibliography have for the most part been limited to American work, but some studies not of American origin are included. This is particularly true of some phases of archaeology and physical anthropology.

The term anthropology is here employed in its broadest sense and includes physical anthropology, ethnology, ethnography, social anthropology, and archaeology, as is customary in British and American usage. In this paper the divisions into cultural anthropology, archaeology, and physical anthropology only are made.

Although not often stated, and sometimes explicitly denied, there is implicit in much of the modern work the assumption that the dynamic processes and laws attributed to man's institutions cannot be divorced from those pertaining to man himself. The highest common denominator in the analysis of culture is man, and the present tendency is to examine the cultures of living groups in terms of people. Although there are marked schisms in the various schools of thought, it is increasingly obvious that the preoccupation of many social anthropologists with the psychological approach is a recognition of this principle. The consideration of physiological, biological, and constitutional factors must follow as a matter of course.

The concept of man and his works must therefore be regarded in the nature of a continuum, and consequently culture cannot be analyzed in terms of abstractions and laws apart from the human animal. In order to attain predictability, the anthropologist must not only strive to obtain a cross-sectional view of modern man and his ways of life, but he must also find out what has happened to man in the past, how he has developed, how he has reacted to various stimuli, and what he has produced. In these terms, recourse to known history and to archaeology is vital, for we must obtain the case history of man if we are to understand him at present and throw light on his future.

The three major topics to be discussed are interrelated in terms of this broader view, and the specific approaches and schools of thought mentioned illuminate various facets of the larger objective. The specialist will find some of the interpretations open to challenge, ample grounds for disagreement with the writer's assessments, and various omissions and disputable classifications in the bibliography. This paper, however, represents an effort to provide the nonspecialist with a brief orientation in the type of thinking and research now current.

CULTURAL ANTHROPOLOGY[1]

In recent years there has been a marked tendency toward the breaking down of formal barriers between the various social sciences. This has been carried to such an extent that it is almost impossible to define the limits where sociology and social psychology stop and social anthropology begins. Indicative of this trend has been the establishment of the Institute of Human Relations at Yale University and the Department of Social Relations at Harvard University, in both of which social scientists from several fields collaborate. At the present time there is a great deal of collaboration, cooperation, and coordination among specialists. Not only have anthropologists been called to participate in the solution of problems in such fields as mental health, family life, child care and development, and hygiene, as well as those

of administration and group relations, but volumes incorporating the various aspects of the social sciences are also appearing.[2]

In research of both ethnographic and cross-cultural ethnologic character, emphasis shifted some time ago from the purely descriptive approach to a concern for isolating the fundamental and dynamic processes that shape both the individual and his culture. The most recent trends in cultural anthropology fall into two main areas, which can be further subdivided. The first concerns itself with the determination of cultural forms and with the techniques for analyzing the dynamic implications of their components. The second deals with the problem of the individual in culture, particularly with the influence of culture upon the personality of the individual. Although conceptually distinct, these two fields of interest are by no means mutually exclusive, and both approaches are usually incorporated within single studies.[3]

The present tendency in American cultural anthropology is not so much to eschew theoretical approaches as it is to avoid the overschematized positions of the more extreme schools. Both in theory and in operational method the trend has been toward an eclecticism and synthesis of the several approaches that are selected and applied to the problem in hand.

One of the chief efforts of those representing the so-called structuralist school lies in describing cultures *in toto* and in determining the forms of their analyzable components relative to group value attitudes. Their basic position is closely akin to that of the gestalt psychologists in that they recognize in specific cultures individual flavors somewhat different from the sum of the recognizable parts. Like the personality of the individual, this ethos seems, in part at least, to stem from the projective systems and personality-forming traits within the culture of the group. A variant approach is Opler's view that the value system of a society is organized around several themes which are not necessarily harmonious or consonant with one another.

Basic concepts of this school are expressed in the use of terms such as ethos, configuration, pattern, and theme, but the struggle for clarity continues. The data are clear enough, as are the interpretations of individuals working in this field, but the semantics of this approach are not yet established. Kluckhohn (in Spier, bibliographical note 2) has suggested that the term "pattern" be used to denote structural regularity in overt culture, and that a sharp distinction be made between ideal and actual behavior patterns. He would restrict the term configuration to express structural regularity in covert culture. There is, however, no uniformity as yet in the exact definition of these labels, nor is there standard practice as to the levels of abstraction for which they are used.

Essentially the treatment of culture in terms of structural analysis resolves itself into a study of group values and group expressions of behavior. In order to assess a cultural structure from this viewpoint, historic, functional, and personality analyses must be fused. Implicit in this approach is its direct applicability to specific problems, such as the processes and effects of acculturation among specific groups, adjustment problems resulting from the retention of cultural heritage by minorities, and the relation of the individual to his social and cultural environment. In this regard interest is centering on the cultural patterning of larger groups such as nation and tribe and also on that of smaller divisions such as communities[4] and minorities of specific national origins.

The study of personality formation, on the other hand, is the analysis of group norms in behavior, psychology, and the like within the structural framework and the recognition of deviants and their place within a given culture. This approach involves the isolation of basic personality as well as the classification of personality types within a community or cultural group and the recognition and classification or labeling of the conditioning factors. Techniques employed include the structural and functional analyses of the specific cultural background; concentration upon the periods of infancy and childhood in regard to treatment, and attitudes and relationships between adults and children, between children, and between adults; and the use of projective techniques such as the analysis of childplay, games, and the use of Porteus, Rorschach, Thematic Apperception, and other tests. Many of the recent group studies combine these approaches. Conversely, but in the same vein, Kardiner[5] is attempting to apply psychiatric interpretation to projective systems within cultural data and to define the reciprocal relationships between culture and personality.

Although several works long antedate the period under discussion, native autobiographies and accounts and semifiction which competently fuses ethnographic data into consistent, patterned life histories as seen through primitive eyes are other means employed by these schools.[6] These devices bring into sharp focus the character of a given culture, the value attitudes of the people, and, by implication at least, the projective systems, the basic personality norms and deviants, and the proc-

esses and degrees of acculturation of the individuals and of the group.

On a practical level this school is throwing a great deal of light on problems of acculturation, cultural change, administration, and on industrial and class relations. They are also testing many generalizations on human behavior which were propounded in the past from viewing our own culture alone.

A somewhat different approach to similar objectives appears in the work of G. P. Murdock's cross-cultural survey, conducted by the Institute of Human Relations at Yale.[7] The object of this study is to build up as complete an ethnographic picture as possible of a large number of diverse cultures and then to test generalizations of presumed association and correlation by statistical analysis of the recorded phenomena.

Another method of culture analysis is based on the definition of cultural anthropology as the science of human relations. In an attempt to reduce such a study to an objective technique, Chapple and Coon[8] offer an analytic tool of research in the objective definition and quantitative measurement of interaction between individuals and among groups. Patterns of relationship thus established throw light on the functional significance of institutions, on the forms of specific institutions in response to their contexts, and on the processes of culture continuity, diffusion, and change.

For new material of the strictly functional school, attention should be called to two posthumous publications by Malinowski. *A Scientific Theory of Culture*[9] presents a clear formulation and dissection of his theoretical approach, and *The Dynamics of Culture Change,* based largely upon African material, tests the applicability of his concepts to administrative problems. Although not strictly comparable in approach, other acculturation studies express the same concern and inquire into the causes, dynamics, and forms of culture change and explore the problems in human relations and cultural disintegration that arise from it.[10]

As a modified survival of the older evolutionistic approach the neoevolutionist school of thought should be mentioned. Headed by L. A. White,[11] this group is still struggling with an over-all theoretical formulation and explanation of culture and its growth. They base their approach on per capita consumption of energy.

Although less sharply defined as a clear-cut anthropological attack, the work of one other group should be mentioned. This is the so-called sociological school,[12] which concerns itself with the analyses of communities within our own civilization. In theory and in operational technique, however, its approaches are largely ethnographic and ethnologic in that it combines the same considerations of material culture, functional institutions, structural configurations, group values, and personality formation that characterize modern studies of primitive and nonliterate cultures. Much of this work to date is primarily concerned with class and caste, but other themes are also analyzed or are implicit in the group's general treatment.

In America the present trends of cultural anthropology indicate that the field, in spite of its lack of distinct boundaries, is coming of age. Although there is still much to be done in the formulation and elucidation of theory and methodology, much that has been learned in the laboratory of the simpler cultures is now being applied directly to the understanding of problems within our own civilization. On the evidence afforded by the universality of culture borrowing, configurations, personality structures, and the like, the psychic unity of man is generally accepted, and this premise is implicit in practically all work being carried on today. When we add to this the concept of a culture as a system of learned designs for living, the study of culture becomes an analysis of individual and group reactions to forces which cause development and change and to forces which limit the possibilities of choice and thus determine the ultimate form of cultural expression. The maturity of the present approach is attested by the steadily growing applicability of its results to a better understanding of ourselves and of the problems of our Western civilization.

ARCHAEOLOGY[13]

During recent years there have been certain healthy trends developing in the archaeological field. Although archaeologists are still painfully aware of the inadequacy of their data and are consequently prone to be ultraconservative in their attempts to avoid overfacile and unfounded generalizations, they show a growing tendency to interpret their results against a broader background than that of their strictly local and technical operations.

These attempts fall into two main classes. On the one hand, there is the recognition of archaeology as a technique of historical research, ably presented by Childe,[14d,e,f] in which legitimate deductions as to the development of culture and civilization are made according to the evidence at hand. Further extensions of this approach lead directly into the analysis of protohistoric periods and the

consequent linking of prehistoric and historic cultures into a broad and understandable pattern.

On the other hand, there is the recognition of archaeology as a technique of ethnology. This view is implicit in the concern which several archaeologists have exhibited in their examination and discussion of the theoretical implications of their procedures.[14b,k,p]

In specific areas steady advances have been made. Most of these are specialized in nature and need not be treated here. For the most part they represent a refinement and more precise understanding of patterns and relationships already blocked out or postulated. In the Near East, for example, the chief interest centers in the attempts to establish chronologies and correlations between the sequences that have been worked out for the different areas. Perhaps the most significant excavations of recent date are those of Tel Hassuna[14m] in Northern Iraq, where several stratified levels of pre-Halaf date have been found under Halaf deposits. Pre-Halaf material has long been known from several sites, but this is its first occurrence in circumstances that furnish considerable data on the earlier phases of the Neolithic in the Near East.

For European archaeology the steady revision downward of the Iron Age dating has perhaps the most significance from the standpoint of history.[14l] A detailed monograph on the Neolithic cultures of Bulgaria has just been written by the late Dr. James Gaul and published by the American School of Prehistoric Research.

An excellent study of Pleistocene archaeology in southern and eastern Asia has been published by Movius.[14n] This not only gives geological correlations but also recognizes a large area of chopping-tool cultures contemporaneous with, and distinct from, the biface industries of the West.

In the American field there have been a few important developments and a steady increase in the elaboration of patterns already established. Of particular significance are the equating of the Teotihuacan culture with that of the Mayan Old Empire, or classic period, and the recognition of Tula not only as the Historical Toltec but also as the source of Mexican influence in northern Yucatan, thus affording a double correlation of the Mexican and Mayan sequences.[14j,q,r]

On a broader scale is the concept of a Meso-American archaeological sphere. Although this concept has not yet been clearly defined, it involves the recognition of an archaeological culture area, including the northern part of Central America and Mexico, which remained relatively constant throughout the changing cultures of the different periods. (*See* Kidder *et al.,* note 14.) The recently isolated Olmec complex is regarded by some not only as very early but also as one of the bases from which the Meso-American cotradition developed.

In Peru[14a] the coordinated study of the Viru Valley undertaken by six North American and two Peruvian institutions provides intensive data on a limited region. Archaeological sequences and settlement shifts are being definitively established, and a long period of preceramic culture has come to light, pushing the archaeological record back to still earlier dates.

In North America the formation of the Committee for the Recovery of Archaeological Remains represents an important step.[14c] This Committee was organized in 1945 to meet the emergency created by government flood-control and irrigation programs involving extensive dam-building and reservoir systems. Since major population movements, intensive occupation, and cultural development have always taken place along the world's waterways, the salvaging of archaeological remains from the areas to be flooded is of paramount importance. The evidences of culture history from many significant localities must be unraveled before they are rendered inaccessible or destroyed. The Smithsonian Institution is the agency in charge of this program, and its position is strengthened by a series of interdepartmental agreements with such interested parties as the National Park Service, the Bureau of Reclamation, and the Corps of Engineers. Cooperation with state and local organizations is being developed. and several archaeological surveys of threatened areas have already been made or are under way.

Although not a part of the River Valley Project, the excellent synthesis of eastern archaeology by Ford and Willey[14h] points up the significance of river valleys and geographical routes in understanding the development and diffusion of archaeological cultures and of the movement of peoples.

Other advances lie in the geologic dating of the Lindenmeier and other Folsom sites to the late Pleistocene or immediately post-Pleistocene epoch. This, taken in conjunction with the Minnesota and Tepexpan skeletons (mentioned below under "Physical Anthropology"), clearly establishes an early date for the peopling of America. Recent newspaper articles report finds in Nebraska which are dated to the middle of the last glaciation; authentic scientific information is still lacking.

In the Southwest the definition of subareas and regional differentiation continues, and the separation of Anasazi, Hohokam, and Mogollon cultures

seems well established. Migration drifts and continuities with the cultures of historically known peoples are being traced.

Just published work by Larsen and Rainey at Ipiutak, Point Hope, Alaska, has yielded new material and is pushing back the dates for the earliest recognizable influxes from Siberia.[14q,l,o]

PHYSICAL ANTHROPOLOGY[15]

A) Fossil man:[16] The discovery and publication of much new fossil material has thrown considerable light on the evolution of man and added new points of controversy. In Java additional finds of several *Pithecanthropus* specimens have clearly defined the general type and established its close relationship with the *Sinanthropus* specimens from Choukoutien. The appearance of an exceptionally large jaw fragment in lower Pleistocene deposits in Java called *Meganthropus paleojavanicus* by Weidenreich and von Koenigswald, and three huge primate teeth, presumably Pleistocene in date but recovered from the shelf of a Hong Kong drugstore and labeled *Gigantopithecus blacki* by the same authors, have brought up the question of giantism among man's early ancestors. This is a provocative view which is not accepted by all anthropologists, but which does have an element of possibility.[16c,m,n,o]

Homo soloensis from the upper Pleistocene Ngandong beds of Java can now be fairly interpreted as a later and more advanced form of the *Pithecanthropus-Sinanthropus* group. The view that Neanderthal man is an offshoot of the east Asiatic types has been advanced and is under discussion.

In South Africa the series of fossil finds of adult members of the *Australopithecus* group, begun by Broom in 1936 and still continuing, has illuminated the question of transitional forms in human evolution.[16a] The exact dating of these Australopithecinae is still uncertain; although many statements attribute them to the mid-Pleistocene, Broom maintains that it is still possible that they are Upper Pliocene in date. These adult specimens come from Sterkfontein (*Plesianthropus transvaalensis*) and Kromdraai (*Paranthropus robustus*) and exhibit both human and apelike characters in the dentition, face, and skull. Long bones and more recent unpublished material indicate an upright posture, and the circumstances of their finding suggest a troglodytic and cooperative form of life. The range in cranial capacity is from 480 cc for *Plesianthropus* to 650 cc for *Paranthropus*. The actual significance of these Australopithecinae must wait until some specimens are found in deposits that can be accurately dated. If a Pliocene date should be established, it might not be necessary to eliminate them completely from the human line or to treat them merely as instructive and analogous offshoots.

In western Europe little new has been added to the alleged finds of the Early Palaeolithic since the discovery of the Swanscombe skull in 1935.[16b,g] This specimen came from an Acheulean context, and, in the view of many, its close resemblance to the Piltdown and Galley Hill skulls lends these earlier and disputed finds an increasing aura of respectability. This interpretation is, however, not universally accepted. The general implication is that a morphologically advanced type of early man existed in western Europe at a time level in which the inhabitants of eastern Asia were in the *Pithecanthropus-Sinanthropus* stage. While more such material is required for the elucidation of this interpretation, in the view of this writer the geographic distribution of core- and chopping-tool cultures lends added credibility. Recent reports that Lower Palaeolithic man has been found in France may indicate that clarifying evidence is at hand.[16k] At the cave of Fontéchevade, Charente, a modern-type skull was found associated with Tayacian implements sealed beneath a stalagmitic layer.

Neanderthal remains have now been reported from Baisun in Uzbekistan[16l] and from Tangier in Morocco. The divergent Neanderthaloids found in 1932 at Mount Carmel in Palestine have been published in detail.[16h] Whether these represent transitional forms or hybrids is still in question, but the tendency at present is to regard them as a hybridization between progressive and conservative Neanderthal forms or between conservative Neanderthaloid and more advanced types.

In America the recent find of an essentially Indian type at Tepexpan[16d] in Mexico is dated as Upper Pleistocene. In conjunction with the Minnesota[16e,f] skeleton, it not only adds confirmation to the archaeological evidence for an early date for the peopling of America, but it also supports the interpretation drawn from the finds of Asselar, Grimaldi, the Upper Cave at Choukoutien, and the like, that the differentiation of modern races was well under way during the late Pleistocene period.

B) Special studies: Some work is continuing along the lines of racial analysis in an attempt to define physical types and their distribution. Perhaps the most important recent work of purely descriptive character is the identification by Field of the Iranian Plateau variant of the Mediterranean race.[17]

Most racial studies at the present time, however, are more concerned with stability and inheritance patterns of the types present; populations are treated more as units, and the racial aspect is being studied in relation to historical, environmental, and genetic considerations.[17] Partly in reaction to the misapplication and abuse of the racial concept in recent years, many anthropologists are challenging the importance and significance of racial difference and are turning their attention to other facets of interest in the biology of man.

Among present trends is an effort to throw more light on human genetics.[18] This is being done through family studies of specific characters and by analysis of anthropometric data recorded for populations, the studies of blood types and their inheritance,[19] and by laboratory experiments which test the limits of genetic capability for development in bone growth as they are affected by muscular conditioning. One of the subsidiary goals of this approach is its application to the history of human development and the extension backward in time of our understanding of genetic processes.

Another area of research interest is in the arena of growth and ageing studies.[20] Normal development and variations caused by special conditions such as diet, environment, and pathology are receiving special attention. This field is making contributions to both pediatrics and orthodontics.

Perhaps the most highly controversial field in physical anthropology at this time is that of constitutional anthropology, or the anthropology of the individual.[21] The Kretschmerian view that there is a correlation between recognizable morphological and psychological types has been reassessed and elaborated by Sheldon and others. In its present form this concept is expressed in terms of three physical components which stem from the embryological layers known as the endoderm, mesoderm, and ectoderm. Labeled endomorphy, mesomorphy, and ectomorphy, all three components are present in varying degrees in each individual. The chief contribution of this new approach is the development of a rating scale and technique by which the relative strength of each component is assessed for each individual, thereby permitting the recognition and definition of intergrades between the rarely-occurring extreme types. A similar scale has been devised for rating temperament, and a high correlation between the two has been reported.

Although considerable effort is still being expended on clarification and refinement of technique and although this approach has not been adequately tested in cross-racial, cross-cultural, and other contexts, it is already being used operationally in its application to clinical medicine and to some extent to personality adjustments in industry, to problems of military equipment, and to sociological and demographic data.

This whole school is being subjected to violent criticism by many anthropologists who believe that its concepts are untenable or that its techniques are unsound. If the approach can be validated, however, its potential utility and general implications are far-reaching and of the utmost importance.

Less significant but also worthy of mention is one further aspect of physical anthropology. This is the application of anthropometric techniques to specific problems of design. During the war anthropometry was utilized to define range of variability, averages, and size frequencies of bodily measurements as a basic element in the design and production of clothing and equipment. It is being used in a similar manner by various industries today.

LITERATURE

The following bibliography is by no means complete and is intended merely as an orientation in the type of work now being done. Articles of special interest have appeared in numerous journals dealing with anthropology, archaeology, sociology, psychology, genetics and heredity, biology, physiology, and medicine; in the transactions of various academies; in the bulletins of numerous institutions; and in various monographs and special studies. The bulk of the recent literature is such that no adequate summary is possible.

There are several new works of an introductory nature. Of these the most satisfactory is Kroeber's masterly revision of his *Anthropology*. *The Ways of Men*, by John Gillin, emphasizes a functionalist approach, and *Man and His Works*, by M. J. Herskovits, stresses the cultural relativist point of view. Both are comprehensive. *Social Organization*, by R. H. Lowie, although more limited in scope, is thorough and valuable.

For physical anthropology the revised edition of *Up From the Ape*, by E. A. Hooton, is the most complete up-to-date survey.

Although the various items have been arranged in the bibliography by topics, many of them properly belong under more than one heading. This statement applies particularly to those titles which are grouped under Structure, Personality Formation, and Community Studies.

NEW JOURNALS

Applied Anthropology Problems of human organization. Chapple, E. D., Ed. Boston: Society for Applied Anthropology, Vol 1, 1941. (Quarterly)

Journal of Near Eastern Studies. Journal of the Department of Oriental Languages and Literature of the University of Chicago Univ. of Chicago Press, Vol 1, 1942. (Quarterly)

Southwestern Journal of Anthropology. Albuquerque: Univ. of New Mexico and Laboratory of Anthrop., Vol. 1 (I), Spring 1945. (Quarterly)

Yearbook of Physical Anthropology New York: The Viking Fund, Vol. 1, 1945 (Annual)

1 GENERAL NEW WORKS OF MORE OR LESS GENERAL INTEREST

COON, C. S., Ed. *A Reader in General Anthropology*. New York: Henry Holt, 1948.

GILLIN, J. *The Ways of Men: An Introduction to Anthropology*. New York D. Appleton-Century, 1948

HERSKOVITS, M. J. *The Economic Life of Primitive Peoples*. New York, London. Knopf, 1940.

HERSKOVITS, M. J. *Man and His Works*. New York. Knopf, 1948.

KLUCKHOHN, C. *Mirror for Man. The Relation of Anthropology to Modern Life*. New York: Whittlesey House, 1949.

KROEBER, A. L. *Cultural and Natural Areas of Native North America*. Berkeley: Univ of Cal. Press, 1939.

KROEBER, A. L. *Anthropology*. (2nd ed.) New York. Harcourt, Brace, 1948.

LOWIE, R. H. *An Introduction to Cultural Anthropology*. (2nd ed.) New York: Farrar & Rinehart, 1940.

LOWIE, R. H. *Social Organization*. New York: Rinehart, 1948.

MURDOCK, G. P. *Ethnographic Bibliography of North America*. Yale Anthrop. Stud., 1. New Haven: Yale Univ. Press, 1941.

STEWARD, J., Ed. *Handbook of South American Indians*. Bur. Amer. Ethnol., Bull 143. Washington, D. C. 6 volumes are projected, published are
- Vol. 1. *The Marginal Tribes*, 1946.
- Vol. 2. *The Andean Civilizations*, 1946
- Vol. 3. *The Tropical Forest Tribes*, 1948
- Vol. 4. *The Circum-Caribbean Tribes*, 1948.

SWANTON, J. R. *Indians of the Southeastern United States*. Bur. Amer. Ethnol., Bull. 137. Washington, D. C., 1946.

VAILLANT, G. C. *Aztecs of Mexico: Origin, Rise and Fall of the Aztec Nation*. Garden City, N. Y.: Doubleday, Doran, 1941.

WIESCHHOFF, H. A. *Anthropological Bibliography of Negro Africa*. Amer. Oriental Ser. 23, New Haven: Amer. Oriental Soc., 1948.

Human Origins. An Introduction to Anthropology. Chicago: Univ. of Chicago Bookstore, 1945.

The Maya and Their Neighbors. Essays presented to Alfred M. Tozzer New York: D. Appleton-Century, 1947–8.

2. INCORPORATION OF VARIOUS ASPECTS OF THE SOCIAL SCIENCES

GOTTSCHALK, L., KLUCKHOHN, C., and ANGELL, R. *The Use of Personal Documents in History, Anthropology, and Sociology*. Soc. Sci. Res. Council, Bull. 53. New York, 1945.

✓ HUNT, J. McV., Ed. *Personality and the Behavior Disorders: A Handbook Based on Experimental and Clinical Research*. New York: Ronald Press, 1944.

✓ KLUCKHOHN, C. K., and MURRAY, H. A., Eds *Personality in Nature, Society, and Culture*. New York: Knopf, 1948

LINTON, R., Ed. *The Science of Man in the World Crisis* New York Columbia Univ Press, 1945. (Articles on cultural anthropology [structural], psychology, and sociology.)

MURDOCK, G. P., Ed. *Studies in the Science of Society: Presented to A. G. Keller*. New Haven: Yale Univ. Press; London: Oxford Univ. Press, 1939.

NEWCOMB, T M., HARTLEY, E. L., *et al. Readings in Social Psychology*. New York: Henry Holt, 1947

SPIER, L., HALLOWELL, A. I, NEWMAN, S. S., Eds. *Language, Culture, and Personality* Menasha, Wis Sapir Mem. Pub. Fund, 1941.

3. PROCESS, STRUCTURE, AND PERSONALITY FORMATION

BATESON, G., and MEAD, MARGARET. *Balinese Character: A Photographic Analysis*. New York: N. Y. Acad. Sci., 1942. (A personality study.)

BENEDICT, RUTH F. *Patterns of Culture*. Boston: Houghton Mifflin, 1934; reissued, New York: Pelican Ed, Penguin Books, 1946. (Deals primarily with ideal patterns of culture and personality among Zuni, Kwakiutl, and Dobu.)

BENEDICT, RUTH F. *The Chrysanthemum and the Sword*. Boston: Houghton Mifflin, 1946. (Treats of nationality structure and formation of national personality types in Japan.)

DAVIS, A. *Father of the Man*. Boston, 1947.

DAVIS, A., and DOLLARD, J. *Children of Bondage: The Personality Development of Negro Youth in the Urban South*. Washington, D C.: Amer. Council of Educ., 1940.

DENNIS, W. *The Hopi Child*. Univ. of Virginia, Inst. for Res. in Soc Sci., Inst. Monogr. 26 New York, London: D Appleton-Century, 1940.

DUBOIS, CORA. *People of Alor: A Social Psychological Study of an East Indian Island*. With analyses by A. KARDINER and E. OBERHOLZER. Minneapolis: Univ. of Minn. Press, 1944.

EVANS-PRITCHARD, E. E. *The Nuer*. Oxford, 1939. (A structuralist analysis.)

GORER, G. *Himalayan Village*. London: Michael Joseph, 1938. (A community study dealing with structure and personality.)

GORER, G. Themes in Japanese Culture. *Trans. N. Y. Acad. Sci.*, Ser. 2, 1943, 5, (V), 106–124. (Article dealing with structure and personality formation.)

HARING, D., Ed. *Personal Character and Cultural Milieu*. Syracuse, N Y.: Syracuse Univ. Bookstore, 1948.

HOEBEL, E. A. *The Political Organization and Law-Ways of the Comanche Indians*. Amer. Anthrop. Ass. Mem. 54, Menasha, Wis., 1940.

HSU, F. L. K. *Under the Ancestors' Shadow*. New York: Columbia Univ. Press, 1948.

KLUCKHOHN, C., and LEIGHTON, D. *The Navaho*. Cambridge: Harvard Univ. Press, 1946. (Structure and personality formation.)

KROEBER, A. L. *Configurations of Culture Growth*. Berkeley: Univ. of Cal. Press 1944. (A structural approach to the history of civilization. Literature, art, philosophy, and science are grouped as products and expressions of culture patterns within larger configurations; the men are treated as symptomatic of the cultural compulsives.)

LEIGHTON, D., and KLUCKHOHN, C. *Children of the People*. Cambridge: Harvard Univ. Press, 1946. (Personality formation and acculturation among Navaho children.)

LIN, Y-H. *The Golden Wing: A Sociological Study of Chinese Familism*. New York: Oxford Univ. Press, 1948.

LINTON, R. *The Study of Man*. New York, London: D. Appleton-Century, 1936. (The point of departure upon

which Linton and Kardiner's later work on personality formation is based.)

LINTON, R. *The Cultural Background of Personality.* New York, London. D Appleton-Century, 1945.

LLEWELLYN, K N., and HOEBEL, E. A. *The Cheyenne Way.* Norman: Univ. of Okla. Press, 1941. (A structural study.)

MACGREGOR, G., and others. *Warriors without Weapons.* (*See* Acculturation, Note 10.)

MEAD, MARGARET. *And Keep Your Powder dry. An Anthropologist Looks at America.* New York: Morrow, 1942.

OPLER, M. E. *An Apache Life Way The Economic, Social, and Religious Institutions of the Chiricahua Indians.* Chicago: Univ. of Chicago Press, 1941.

OPLER, M. E. Themes as Dynamic Forces in Culture. *Amer. J. Sociol.,* 1945, 51, 198–206. (Article on a structural concept.)

OPLER, M. E. Some Recently Developed Concepts Relating to Culture. *Southwestern J. Anthrop.,* 1948, 4, (II), 107–122.

RAUM, O. F. *Chaga Childhood: A Description of Indigenous Education in an East African Tribe.* London. Oxford Univ. Press, 1940.

RICHARDSON, JANE. *Law and Status among the Kiowa Indians.* Amer. Ethnol. Soc. Monogr. 1. New York: J. J. Augustin, 1940.

THOMPSON, LAURA, and JOSEPH, ALICE. *The Hopi Way.* Chicago: Univ. of Chicago Press, 1944. (A structure and personality formation study.)

WHITING, J. W. M. *Becoming a Kwoma: Teaching and Learning in a New Guinea Tribe.* New Haven: Yale Univ. Press, 1941. (A personality formation study.)

4. COMMUNITY STUDIES

BEALS, R. L. *Cherán: A Sierra Tarascan Village.* Smithsonian Inst., Inst. of Soc. Anthrop., Publ. 2, Washington, D. C., 1946.

EMBREE, J. F. *Suye Mura: A Japanese Village.* Chicago: Univ. of Chicago Press, 1939.

FOSTER, G. M. *Children of Empire The People of Tzintzuntzan.* Smithsonian Inst., Inst. of Soc. Anthrop., Publ. 6, Washington, D. C., 1948.

GILLIN, J. *Moche: A Peruvian Coastal Community.* Smithsonian Inst., Inst. of Soc. Anthrop., Publ. 3, Washington, D. C., 1947.

GOLDSCHMIDT, W. R. *As You Sow.* New York: Harcourt, Brace, 1947.

HERSKOVITS, M. J., and FRANCES S. *Trinidad Village.* New York: Knopf, 1947.

MINER, H. *St. Denis: A French-Canadian Parish.* Chicago· Univ. of Chicago Press, 1939.

PARSONS, ELSIE CLEWS. *Mitla, Town of Souls, and Other Zapoteco-speaking Pueblos of Oaxaca, Mexico* Univ. of Chicago Publ. in Anthrop., Ethnol. Ser., 1936.

PARSONS, ELSIE CLEWS. *Peguche, Canton of Otavalo, Province of Imbabura, Ecuador: A Study of Andean Indians.* Univ. of Chicago Publ. in Anthrop., Ethnol. Ser., 1945.

REDFIELD, R. *Tepoztlán, a Mexican Village.* Univ. of Chicago Publ. in Anthrop., Ethnol. Ser., 1930.

REDFIELD, R. *Folk Culture of Yucatan.* Chicago: Univ. of Chicago Press, 1941.

REDFIELD, R., and ALFONSO, V. R. *Chan Kom, a Maya Village.* Carnegie Inst. of Washington, Publ. 448, Washington, D. C., 1934.

SPICER, E. H. *Pascua, a Yaqui Village in Arizona.* Univ. of Chicago Publ. in Anthrop., Ethnol. Ser., 1940.

WARNER, W. L. *Yankee City Series,* 6 vols. (*See* Note 12.)

WEST, J. *Plainville, U.S.A.* New York: Columbia Univ. Press, 1945.

YANG, M. *A Chinese Village: Taitou. Shantung Province.* New York: Columbia Univ. Press, 1945.

5. BASIC PERSONALITY FORMATION

KARDINER, A. *The Individual and His Society.* New York· Columbia Univ. Press, 1939. (Psychiatric interpretations of projective systems within cultural data and the relations of those systems to basic personality formation.)

KARDINER, A. *The Psychological Frontiers of Society.* New York: Columbia Univ. Press, 1945. (Further development of technique and application as laid down in *The Individual and His Society.*)

6. AUTOBIOGRAPHY AND SEMIFICTION

ARSENIEV, V. K. *Dersu the Trapper.* New York: Dutton, 1941.

COON, C. S. *Flesh of the Wild Ox.* New York: Morrow, 1932; London: Cape, 1932. (Semfictionalized tribal study of the Moroccan Rif.)

DYK, W. *Son of Old Man Hat.* New York: Harcourt, Brace, 1938. (Navaho autobiography.)

DYK, W. *A Navaho Autobiography.* Viking Fund Publ. in Anthrop., 8. New York, 1947.

FAST, H. *The Last Frontier.* New York Reader's Club, 1942.

FORD, C. S. *Smoke from Their Fires: The Life of a Kwakiutl Chief.* Inst. of Human Relations. New Haven: Yale Univ. Press, 1941.

HAMBLY, W. *Jamba.* Chicago: Pellegrini & Cudahy, 1947. (Fictionalized biography of an Ovimbundu.)

MARRIOTT, ALICE. *The Ten Grandmothers.* Norman: Univ. of Okla. Press, 1945. (Personalized, semifictionalized study of Kiowas.)

MARRIOTT, ALICE *Maria, the Potter of San Ildefonso.* Norman: Univ. of Okla. Press, 1948

OPLER, M. E. *Dirty Boy.* Amer. Anthrop. Ass. Mem. 52., 1938. (Apache, partial autobiography.)

PRUITT, IDA. *A Daughter of Han: The Autobiography of a Chinese Working Woman, from the Story Told Her by Ming Lao T'ai-T'ai.* New Haven: Yale Univ. Press; London: Oxford Univ. Press, 1945.

RADIN, P., Ed. *Crashing Thunder, Winnebago Indian.* Berkeley: Univ. of Cal. Press, 1920; New York: D. Appleton-Century, 1926.

REICHARD, GLADYS A *Dezba: Woman of the Desert.* New York: J. J. Augustin, 1939.

SIMMONS, L. W., Ed *Sun Chief: The Autobiography of a Hopi Indian.* New Haven: Yale Univ. Press, 1942.

UNDERHILL, RUTH M. *Hawk over Whirlpools.* New York: J. J. Augustin, 1940.

7. CROSS-CULTURE STUDIES

FORD, C. S. *A Comparative Study of Human Reproduction.* Yale Univ. Publ. in Anthrop. 32. New Haven: Yale Univ. Press; London. Oxford Univ. Press, 1945.

HORTON, D. The Functions of Alcohol in Primitive Societies: A Cross-Cultural Study. *Quart. J. Stud. Alcohol,* 1943. 4, 199–320.

MURDOCK, G. P Correlations of Matrilineal and Patrilineal Institutions, in *Studies in the Science of Society: presented to A. G. Keller,* MURDOCK, G. P., Ed. New Haven: Yale Univ. Press; London: Oxford Univ. Press, 1937.

MURDOCK, G. P. The Cross-Cultural Survey. *Amer. Sociol. Rev.,* 1940. 5, 361 ff.

MURDOCK, G. P. Bifurcate Merging, a Test of Theorems. *Amer. Anthrop.,* 1947, 49, 56–59.

MURDOCK, G. P (and FORD, C. S., HUDSON, A. E., *et al.*). *Outline of Cultural Materials.* Inst of Human Relations. New Haven: Yale Univ. Press, 1945.

SIMMONS, L. W. *The Role of the Aged in Primitive Society.* New Haven: Yale Univ. Press, 1945.

8. INTERACTION

CHAPPLE, E D. *Measuring Human Relations.* Genet. Psychol. Monogr. 22, 1940.

CHAPPLE, E. D. Personality Differences as Described by Invariant Properties of Individuals in Interaction. *Proc. Nat Acad. Sci.*, 26, Washington, D. C., 1940.

CHAPPLE, E. D. Anthropological Engineering, Its Use to Administrators. *Appl. Anthrop*, 1943, 3, (I).

CHAPPLE, E. D., and COON, C. S. *Principles of Anthropology*. New York: Henry Holt, 1942.

9. FUNCTIONALISM

MALINOWSKI, B. *A Scientific Theory of Culture and Other Essays* Chapel Hill: Univ. of North Carolina Press, 1944 (The theory of functional analysis.)

10. ACCULTURATION

EMBREE, J. F. *Acculturation Among the Japanese of Kona, Hawaii* Amer. Anthrop. Ass. Mem. 59, 1941.

GOLDFRANK, ESTHER. *Changing Configurations in the Social Organization of a Blackfoot Tribe during the Reserve Period.* Amer. Ethnol. Soc. Monogr. 8. New York: J. J. Augustin, 1945.

HAWTHORNE, H. B *The Maori: A Study in Acculturation.* Amer. Anthrop. Ass. Mem. 64, 1944.

HERSKOVITS, M. J. *Acculturation, the Study of Culture Contact.* New York. J J. Augustin, 1938.

KEESING, F. M. *The South Seas in the Modern World.* New York: John Day, 1941.

LAFARGE, O., Ed. *The Changing Indian.* Norman: Univ. of Okla. Press, 1942.

LEWIS, O. *The Effects of White Contact upon Blackfoot Culture.* Amer. Ethnol. Soc. Monogr. 6. New York: J. J. Augustin, 1942.

LINTON, R, Ed. *Acculturation in Seven American Indian Tribes.* New York, London: D Appleton-Century, 1940.

LOCKE, A., and STERN, B. J., Eds. *When Peoples Meet.* New York, Philadelphia: Hinds, Hayden & Eldredge, 1942, 1946.

LORAM, C. T., and MCILWRAITH, T. F., Eds. *The North American Indian Today.* Toronto: Univ. of Toronto Press, 1943.

MACGREGOR, G., HASSRICK, R. B, and HENRY, W. E. *Warriors without Weapons: A Study of the Society and Personality Development of the Pine Ridge Sioux.* Chicago: Univ. of Chicago Press, 1946.

MALINOWSKI, B. *The Dynamics of Culture Change.* New Haven: Yale Univ. Press, 1945.

THOMPSON, LAURA. *Fijian Frontier.* Amer. Council, Inst. of Pacific Relations, 23. Studies of the Pacific, 4. San Francisco, 1940.

THOMPSON, LAURA. *Guam and Its People: A Study of Culture Change and Colonial Education.* Amer. Council, Inst. of Pacific Relations. San Francisco, New York, 1941; Princeton, N. J., 1947.

11. NEOEVOLUTIONIST

WHITE, L. A. Energy and the Evolution of Culture. *Amer. Anthrop.*, 1943, 45, (III), 325 ff.

WHITE, L. A. Evolutionary Stages, Progress, and the Evaluation of Cultures. *Southwestern J. Anthrop.*, 1947. 3, (III), 165–92.

12. SOCIOLOGICAL SCHOOL

ARENSBERG, C. M. *The Irish Countryman.* New York: Macmillan, 1937.

ARENSBERG, C. M., and KIMBALL, S. T. *Family and Community in Ireland.* Cambridge: Harvard Univ. Press, 1940.

DAVIS, A., GARDNER, B. B., and GARDNER, MARY R. *Deep South: A Social Anthropological Study of Class and Caste.* Chicago: Univ. of Chicago Press, 1941.

EMBREE, J. F. *The Japanese.* War Background Studies 7. Smithsonian Inst., Washington, D. C., 1943.

EMBREE, J F. *The Japanese Nation: A Social Survey* New York: Farrar & Rinehart, 1945.

FEI, H. *Peasant Life in China.* London: Routledge, 1939.

LEIGHTON, A. H. *The Governing of Men: General Principles and Recommendations Based on Experience at a Japanese Relocation Camp.* Princeton: Princeton Univ. Press, 1945.

POWDERMAKER, HORTENSE. *After Freedom: A Cultural Study of the Deep South.* New York: Viking, 1939.

WARNER, W. L. *Yankee City Series*, 6 vols. New Haven. Yale Univ. Press, 1941–47. (Anthropological study of a modern New England community over a period of ten years.)

13 ARCHAEOLOGY OF GENERAL INTEREST

BREW, J. O. *Selected Bibliography of American Indian Archaeology East of the Rocky Mountains.* Papers of the Excavators' Club, 1943, 2, (I), Cambridge, Mass.

CHILDE, V. G. *Prehistoric Communities in the British Isles.* London, Edinburgh. W. and R. Chambers, 1940.

CHILDE, V. G. *Dawn of European Civilization.* (4th ed., rev.) London: Kegan Paul, Trench & Trubner, 1947.

JOHNSON, F., Ed *Man in Northeastern North America.* Papers of the Robert S. Peabody Foundation, 3. Andover, Mass., 1946.

MARTIN, P. S., QUIMBY, G. I., and COLLIER, D. *Indians before Columbus·Twenty Thousand Years of North American History as Revealed by Archaeology.* Chicago: Univ. of Chicago Press, 1947.

TAYLOR, W. W. *A Study of Archaeology,* Mem. 69. *Amer. Anthrop.*, 1948, 50, (III, Pt. 2).

ZEUNER, F. E *Dating the Past, an Introduction to Geochronology.* London: Methuen, 1946.

ZEUNER, F. E. *The Pleistocene Period, Its Climate, Chronology and Faunal Successions.* London: Ray Society, B. Quaritch, Ltd., 1946.

14. ARCHAEOLOGY FOOTNOTES

BENNETT, W. C., Ed. *A Reappraisal of Peruvian Archaeology.* Mem. Soc. Amer. Archaeol., *Amer. Antiquity,* 1948, 3, (IV, Pt. 2). (a)

BREW, J. O. *Archaeology of Alkali Ridge, Southeastern Utah.* Peabody Museum Papers, 21 Cambridge, Mass., 1946. (Especially the chapter on The Use and Abuse of Taxonomy, 44–66) (b)

BREW, J. O., Chairman, and others. Symposium on River Valley Archaeology. *Amer. Antiquity,* 1947, 12, (IV), 209–225. (c)

CHILDE, V. G. *What Happened in History.* New York: Penguin Books, 1946. (d)

CLARK, G. *Archaeology and Society.* (Rev. ed.) London: Methuen, 1947. (e)

CLARK, G. *From Savagery to Civilization.* London: Cobbett, 1946. (A compressed summary.) (f)

DELAGUNA, FREDERICA *The Prehistory of Northern North America as Seen from the Yukon.* Mem. Soc. Amer. Archaeol. 3. Suppl. *Amer. Antiquity,* 1947, 12, (III, Pt. 2) (g)

FORD, J. A., and WILLEY, G. R. An Interpretation of the Prehistory of the Eastern United States. *Amer. Anthrop.*, 1941, N. S., 43, (III), 325–63. (h)

HENCKEN, H. The London Conferences on Archaeology, 1943 and 1944. R. Archaeol. Inst., *Archaeol. J.*, 1945, 102. (i)

KIDDER, A. V., JENNINGS, J. D., and SHOOK, E. M. *Excavations at Kaminaljuyu, Guatemala.* Publ. 561, Carnegie Inst. Wash., 1946. (j)

KLUCKHOHN, C., REITER, P., *et al. Preliminary Report on the 1937 Excavations, Bc., 50–51, Chaco Canyon, New Mexico.* Univ. of New Mexico Bull. 345. Anthrop. Ser., 3, (II). (Especially Kluckhohn's theoretical discussion of interpretation and taxonomy.) (k)

Larsen, H., and Rainey, F. B. *Ipiutak and the Arctic Whale Hunting Culture* Anthrop. Papers, Amer. Mus. Nat Hist. 42. New York, 1948. (l)

Lloyd, S., and Safar, F. Tel Hassuna, Excavations by the Iraq Government Directorate General of Antiquities, in 1943 and 1944. *J. Near East. Stud.*, 1945, 4, 255–89. (m)

Movius, H L., Jr. *Early Man and Pleistocene Stratigraphy in Southern and Eastern Asia*. Peabody Museum Papers, **19**, (III), Cambridge, Mass., 1944 (n)

Rainey, F. B. The Ipiutak Culture at Point Hope, Alaska. *Amer Anthrop.*, 1941, **43**, (III), 364–75. (o)

Rouse, I. *Prehistory in Haiti. A Study in Method.* Yale Univ. Publ. in Anthrop. **21**, 1939. (Especially the chapters on conceptual technique and procedure, 9–36.) (p)

Ruz, L. A. *Guia Arqueologica de Tula* Mexico, D. F , 1945. (q)

Stirling, M. W *Stone Monuments of Southern Mexico.* Bur. Amer. Ethnol., Bull. 138, 1943. (r)

15. PHYSICAL ANTHROPOLOGY OF GENERAL INTEREST

Goldstein, M. *Demographic and Bodily Changes in Descendants of Mexican Immigrants, with Comparable Data on Parents and Children in Mexico.* Austin, Tex.: Inst. of Latin American Stud., Univ. of Texas, 1943.

Hooton, E. A. *Man's Poor Relations.* Garden City, N Y.: Doubleday, Doran, 1942. (Excellent review of anthropoid and lower primate studies.)

Hooton, E. A. *Up from the Ape.* (2nd ed.) New York: Macmillan, 1946. (The most comprehensive and up-to-date survey of the general field of physical anthropology.)

Howells, W. W. *Mankind So Far.* Garden City, N. Y.. Doubleday, Doran, 1945. (A readable introduction to physical anthropology.)

Keith, Sir Arthur. *A New Theory of Human Evolution* London: Watts, 1948.

Krogman, W. M. *A Bibliography of Human Morphology, 1914–1939.* Chicago: Univ. of Chicago Press, 1941.

Shapiro, H. L. *Migration and Environment: A Study of the Physical Characteristics of the Japanese Immigrants to Hawaii and the Effects of the Environment on Their Descendants.* London, New York, Toronto: Oxford Univ. Press, 1939.

16. FOSSIL MAN

Broom, F. R. S., and Schepers, G. W. H. *The South African Fossil Ape-Men. The Australopithecinae.* Transvaal Mus. Mem., 2. Pretoria, 1946. (a)

Clark, W. E. LeGros, and Morant, G. M. Report on the Swanscombe Skull. *J. R. Anthrop. Inst.*, 1938, **68.** (b)

DeTerra, H. Pleistocene Geology and Early Man in Java. *Trans. Phil Soc.*, 1943, N. S. **32**, 437–64. (c)

DeTerra, H. Preliminary Note on the Discovery of Fossil Man at Tepexpan in the Valley of Mexico. *Amer Antiquity*, 1947, 13, (I), 40–44. (d)

Jenks, A. E. *Pleistocene Man in Minnesota, a Fossil Homo Sapiens.* Univ. of Minn. Press, 1936. (e)

Jenks, A. E. *Minnesota's Browns Valley Man.* Amer. Anthrop. Ass. Mem. 49. 1937. (f)

Keith, Sir Arthur. A Resurvey of the Anatomical Features of the Piltdown Skull with Some Observations on the Recently Discovered Swanscombe Skull. *J. Anat.*, 1938–39, 73 (g)

McCown, T. D., and Keith, Sir Arthur. *The Stone Age of Mount Carmel.* Vol. II. *The Fossil Human Remains from the Levalloiso-Mousterian.* Oxford: Clarendon Press, 1939 (h)

MacCurdy, G. G., Ed. *Early Man.* Philadelphia: Lippincott, 1937. (A symposium.) (i)

Mahoney. D. J. *The Keilor Fossil Skull: Geological Evidence of Antiquity.* Also, *The Problem of Antiquity of Man in Australia* Both in Mem. Nat. Mus. Melbourne, 13, 1943. (j)

Movius, H. L. Tayacian Man from the Cave of Fontéchevade (Charente). *Amer. Anthrop* 1948, **50**, (II), 365–67 (with further bibliography). (k)

Okladnikov, A P. Neanderthal Man and His Culture in Central Asia *Asia*, 1940, 40. (l)

Weidenreich, F. *The Skull of Sinanthropus Pekinensis, a Comparative Study on a Primitive Hominid Skull.* Paleont Sin , N. S 10, whole ser. 127, 1943. (m)

Weidenreich, F *Giant Early Man from Java and South China* Amer Mus Nat. Hist , Anthrop. Papers, 40, (I), 1945. (Morphological description) (n)

Weidenreich, F. *Apes, Giants, and Man* Chicago: Univ of Chicago Press, 1946. (Expanded public lectures.) (o)

17 RACE

Benedict, Ruth F. *Race, Science, and Politics.* New York: Modern Age Books, 1940, revised, Viking, 1943

Coon, C. S *Races of Europe.* New York: Macmillan, 1939 (Extremely important. A comprehensive survey, description, and analysis of physical types in Europe from paleolithic times to the present, with special reference to historical and archaeological background.)

Field, H. *Contributions to the Anthropology of Iran.* Anthrop. Ser , Field Mus. Nat. Hist , **29**, (2), Publ. 459, 1939.

Lewis, J. *The Biology of the Negro.* Chicago: Univ of Chicago Press, 1942.

Shapiro, H. L. *Migration and Environment.* New York, London: Oxford Univ. Press, 1939

Steggerda, M. *Maya Indians of Yucatan.* Washington, D C : Carnegie Inst., 1941.

18. GENETICS

Brues, Alice M. Sibling Resemblances as Evidence for the Genetic Determination of Traits of the Eye, Skin, and Hair in Man. Unpublished Doctor's thesis, Harvard Univ., 1944.

Brues, Alice M. A Genetic Analysis of Human Eye Color. *Amer. J. Phys Anthrop.*, 1946, N. S. **4**, 1–36.

Dobzhansky, Th. *Genetics and the Origin of Species.* (2nd ed. rev.) New York: Columbia Univ. Press, 1941.

Dunn, L. C., and Dobzhansky, Th *Heredity, Race, and Society.* New York: Pelican Ed., Penguin Books, 1946.

Gates, R. R *Human Genetics.* New York: Macmillan, 1946.

Gates, R. R. *Human Ancestry. From a Genetical Point of View.* Cambridge: Harvard Univ. Press, 1948.

19. SEROLOGY

Boyd, W. C. *Blood Groups.* Tabulae Biologicae, **10**, 1939

Candela, P. B. The Introduction of Blood-Group B into Europe. *Hum. Biol.*, 1942, **14.**

Schiff, F., and Boyd, W. C. *Bloodgrouping Technic.* New York: Interscience Publishers, 1942.

Wiener, A. S. *Blood Groups and Transfusion.* (3rd ed) Springfield, Ill.: Charles C Thomas, 1943.

20. GROWTH AND AGEING

Cowdry, E. V., Ed. *Problems of Ageing: Biological and Medical Aspects.* (2nd ed.) Baltimore: Williams & Wilkins, 1942

Glasser, O., Ed. *Medical Physics.* Chicago: Year Book Publishers, 1944.

Greulich, W. W., and Thoms, H. The Growth and Development of the Pelvis of Individual Girls, Before, During, and After Puberty. *Yale J. Biol. and Med*, 1944, **17**, (I).

Krogman, W. M. *Growth of Man.* Tabulae Biologicae, **20**, 1941.

Simmons, K. *Brush Foundation Study of Child Growth and Development.* Monogr. Soc. Res. Child Developm., 9 (I) 1944

WOODWORTH, R S *Heredity and Environment A Critical Survey of Recently Published Material on Twins and Foster Children.* Soc. Sci Res. Council, Bull. 47, 1941.
Adolescence 43rd Yearbook Nat. Ass. for Child Study, 1944.

21. CONSTITUTIONAL ANTHROPOLOGY

DRAPER, G., DUPERTUIS, C. W., and COUGHEY, J. L., JR. *Human Constitution in Clinical Medicine.* New York, London: Hoeber, 1944.
HEATH, C. W. *What People Are* Cambridge: Harvard Univ. Press, 1945. (The Grant Study.)
HOOTON, E. A. *Young Man, You Are Normal.* New York: Putnam, 1945. (A popularly written report on the Grant Study)
HOOTON, E. A. *Up from the Ape.* New York: Macmillan, 1946 (Summary on pp. 663–98.)
LESSA, W. A *An Appraisal of Constitutional Typologies.* Amer. Anthrop. Ass Mem. 62, 1943. (A comparative and historical study of this field as a whole.)
SHELDON, W H., STEVENS, S. S., and TUCKER, W. B. *The Varieties of Human Physique.* New York: Harper, 1940. (With *The Varieties of Temperament* this is the foundation of the present interest and work in the field of constitutional anthropology.)
SHELDON, W. H, and STEVENS, S. S. *The Varieties of Temperament.* New York: Harper, 1942.

THE FREEDOMS

Speak not to us of freedoms in the large,
Nor chisel words to just the proper bevel;
Assume your closer and more pressing charge:
Let us have freedoms at the local level.
Do you find malice in your quiet town?
Is there compassion in your hustling city?
Men of good will, or tyrants trampling down?
Anonymous aid, or patronizing pity?

The things we need are very simple things:
Respect for human life and honest labor,
A proffered hand—no prompting from the wings,
A better casting in the role of neighbor.
Freedom has fled from many a hostile quarter,
Blurred are the freedoms written on the water.

KATHARINE O'BRIEN

BOOK REVIEWS

THE RACE BETWEEN EDUCATION AND CATASTROPHE

No Place to Hide. David Bradley. xviii + 182 pp. $2.00. Atlantic-Little, Brown. Boston.

ONE OF the skull-cracking problems of this atomic age is to assess properly its hazards. The devastation that an atomic bomb can cause has been emphasized over and over again. It has been said that the atomic age has made war intolerable because atomic bombs are too terrible to be used. If fear alone could prevent atomic wars, then the post-Hiroshima tradition of stressing atomic-bomb damage should be continued. But the problem of atomic energy control cannot be solved through fear. Like all other large problems, it can be met successfully only through level-headed study and discussion. The need that some have felt for stressing the potential disaster of atomic bombs is now clearly overshadowed by the necessity for clear thinking in the entire atomic energy field. Our success not only in avoiding the possible destructiveness of atomic energy but also in realizing its possible benefits will be measured by the extent to which the general public can accurately appraise its potential in either direction.

In *No Place to Hide,* Dr. Bradley unfortunately continues the tradition of stressing the hazards—in this instance, the radiation hazard during and following an underwater atomic-bomb explosion. Imbued with an admirable desire to let the public in on Bikini, he has written an absorbing chronicle of his experience as a "radiological monitor" with the task force that made the tests. If it were no more than that, the book would be superb. His account of the moments in a PBM-5 during the explosions and his flights over the target area as soon as possible thereafter makes good reading. There is much interesting detail about the islands of Bikini, Kwajalein, and Ebeye, on which he was based, interspersed with the routine of preparations and rehearsals.

The book is, however, more than a chronicle. It is also a continuation of part of his mission at Bikini: to convince people of the dangers of radiation which are "evident only upon the dial of some 'Geiger machine'." If the readers of this book are not already convinced after the stories of Hiroshima, they will be before they finish it. But even though Dr. Bradley states quite properly that "The danger from radiation, like the danger from sunburn, snake poison, strychnine, or almost any other hazard, is merely one of degree," readers are likely to feel that any radiation is to be avoided at all costs under any circumstances. For example, they will have read of a sailor who almost had a high amputation of his arm because of a suspicion of radioactive contamination in a minor laceration at the base of the thumb. They will have been led to believe incorrectly that such drastic action would be justified had the wound shown the slightest effect on a Geiger counter. This exaggeration of the seriousness of a radioactively contaminated wound makes it more difficult to acquire the level-headed perspective that is so urgently needed.

It is to be hoped that subsequent literature in the atomic energy field will be equally readable, but will contain as accurate an appraisal as possible of the various kinds of hazards about which readers of this book are left to wonder.

PHILIP N. POWERS
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THE CONTRIBUTION OF LYSENKO TO BIOLOGY

The Science of Biology Today. Trofim Lysenko. 62 pp. $1.25. International Publishers. New York.

IN THE past few months, many articles have appeared, both here and abroad, in the newspapers, scientific journals, and magazines concerning the address of T. D. Lysenko before the Soviet Academy of Agricultural Sciences on July 31, 1948. This book is a translation of that address, and it is not hard to see why so much controversy has been stimulated as the result of its circulation, coupled with the many rumors of the change in viewpoint on genetics in the Soviet Union. It is a familiar phrase to say that any book is an absolute essential for biologists, but to apply such terminology in this instance is an understatement. Nothing in recent years has stirred up so much controversy, and it is important that this address be read by anyone who claims to be informed on trends in biology, particularly students of any phase of evolution.

It will be difficult for scientists to cope with the political dogma that occupies so much of the address. The familiar precise language is intermingled with political philosophy, and a careful reading is necessary in order to sift out the desirable material. Basically, the address contends that the materialistic Michurin trend in genetics is to replace the idealistic reactionary Mendel-Morgan-Weissman theory which is so deeply entrenched throughout the world. Not only is this Lysenko's contention, but by decree it is now the official Soviet viewpoint. It sounds fantastic that our whole concept of heredity, that the efforts of generations of geneticists, that the results observed on thousands of plants and animals, are all set aside by such action, but it is a fact. It will come as a surprise to many geneticists to hear their efforts designated as idealistic, reactionary, and the like.

Stripped of its trappings, the Michurin-Lysenko doctrine is what has been called the inheritance of acquired characters or, as Lysenko expresses it, "changes in the conditions of life bring about changes in the type of development of vegetable organisms. A changed type of development is thus the primary cause of changes in heredity." Expressing it again, he maintains that "Heredity is the effect of the concentration of the action of external conditions assimilated by the organism in a series of preceding generations." Lysenko asserts that this concept is based on Darwinism by saying,

> The Michurinists, in their investigations take the Darwinian theory of evolution as their basis. But in itself, Darwin's theory is absolutely insufficient for dealing with problems of socialist agriculture. That is why the basis of contemporary Soviet agrobiology is Darwinism transformed in the light of the teaching of Michurin and Williams and thereby converted into Soviet creative Darwinism

When asked if such ideas had official approval, he replied, "The Central Committee of the Party examined my report and approved it." That statement explains more than anything else why experimental evidence can be treated with such great abandon.

The necessity for reading this book is not wholly that is represents another viewpoint of genetics, at least from its sponsor's standpoint, but because of the way in which it is being supported. The conclusion shows that very fully (author's italics):

> Progressive biological science owes it to the geniuses of mankind, Lenin and Stalin, that *the teaching of I. V. Michurin has been added to the treasure-house of our knowledge, has become part of the gold fund of our science.*
>
> Long live the Michurinian teaching, the teaching on how to transform living nature for the benefit of the Soviet people!
>
> Long live the party of Lenin and Stalin, which discovered Michurin for the world and created all the conditions for the progress of advanced materialist biology in our country.
>
> *Glory to the great friend and protagonist of science, our leader and teacher, Comrade Stalin!*

R. BAMFORD

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THE SEARCH FOR TRUTH

Economic Man. C. Reinold Noyes. 2 vols. xxiii + 1,443 pp. $15.00. Columbia Univ. Press. New York.

THE reviewer would like to stay around for another half-century to observe the fate of Mr. Noyes' *Economic Man,* to which fate he must perforce now make his small contribution. For the two stout volumes belong to the curious category of the immensely learned, the more or less original, and of works which, except to a Hindu philosopher or perhaps the mythical Martian observer, would seem to depart abruptly from the expected in our culture. Some of such books eventually win over their public; others, because of their very eccentricity and the genius or open-mindedness they demand of their readers, fall into permanent neglect. It would be a shame if the fruit of seventeen years of labor, of masterly synthesis and keen analysis, were to be fated to such neglect, but one would not care to bet heavily that it will not.

The basic objection of the author to classical economics is that it has not even loyally stuck to the elaboration of its formula "wants, efforts, satisfactions," but has allowed itself to give less than its due to process, and more attention than is proper to static physical objects, such as those lumped together under the rubric "land." His basic plea is for marshaling everything that all the pertinent sciences can contribute to a real understanding of man's "wants, efforts (or, alternatively, "means"), and satisfactions." In reality, his two series of terms differ only as to whether means are viewed as given, or as involving an effort necessary to provide them or bring them to the point where they can be effortlessly used.

If one starts (naïvely or pedantically, as you wish) with man, the creature with wants, it is evident that economics is in a sense a biological science. So are all the social sciences, and also linguistics, literature, the fine arts. It may be asserted that if they remain tied to their biological starting point they will never develop very far. Mr. Noyes' reply to this criticism is that if one is to arrive at an adequate explanation of man, one must start at the starting point, with man as organism. Such a start has the additional merit of helping us to be objective, analytic, and quantitative about ourselves.

At any rate, the data of the biological sciences have impressed the author with their certainty and exactness, and he spends his first 500 pages in summarizing them, to the dismay, doubtless, of his readers in economics. For this reason, the book should be reviewed by a committee representing neurology, physiology, psychology, etc., as well as economics. It is, however, precisely Noyes' point that all these matters *must* be brought together in a single mind, and that the compartmentalization which brings it about that biologist and economist do not speak the same language, do not understand each other's jargon, is totally nefarious.

In so far as economics rests on biology, "the data . . . must be accepted as they stand. The facts are unchangeable." In the technology and the social organization which man builds within the limits of the given and the possible, there is more freedom, and economics tends to become "the useful art of economics."

The necessarily bulky evidence of the author's long work, with its burden of quotations, footnotes, graphic representation, and appendices, begins with the "solid rock" of man's physiological wants and mechanisms, and progresses to the study of preferences, of costs. value, and demand. Sherrington, Woodworth, and Pavlov play as important roles as Pareto, Walras, and Clark. It is in homeostasis of the blood that Noyes finds the key to wants, for wanting is essentially the

search to recover a balance that has been somehow disturbed. Wants then produce "central states" (others have called them drives or urges or impulses or emotions), which require integrated action with the end of correcting variations from homeostasis.

The external observations of experimental psychologists add their proof that "mature animals have learned to suppress the consummatory reaction to any internal want which requires an external object for satisfaction until that object is present," a fact which is allied to "the gradual diminution in the relative importance of the repertory of instinctive behavior, as we proceed along the phylogenetic scale." In spite of this learning and suppressing we are, when we base ourselves on modern physiology, very far from the old hedonistic psychology and from the marginal utility theory related to it.

The changes called for in theories of psychological motivation and economic behavior Noyes says he cannot explain in less than his 1,443 pages, so certainly no review can do justice to them. Perhaps his own briefest and most illuminating phrase is that "the pattern of behavior . . . is . . . one determined by the relative intensity of future wants." The emphasis throughout is not on environmental stimuli to which we respond but on internal variations from homeostasis which set up corrective responses and so energize behavior. In pursuing the satisfaction of the wants that arise from blood and brain, we meet the resistances imposed by nature. Resistance is a truer concept than either "land" or "scarcity," and in Noyes' view is his most original contribution. It can be made quantitative, and Noyes affirms its usefulness in his own economic analyses; it leads to better understanding and to a limited degree of predictability.

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ON PINIONS FREE

The Flight of Birds. John H. Storer. xv + 94 pp. 176 figs. $2.50. Cranbrook Inst. of Science. Bloomfield Hills, Mich.

THIS pretty little book, despite many limitations, is the best discussion of bird flight available, in print, in the English language. The photographs of birds in flight and the diagrams of wing and feather structure should appeal to a wide audience with biological interests. For there is scarcely any attempt in the whole array of contemporary "bird books" to explain how a bird manages to fly. The sections devoted to soaring are the strong point of this book, especially Storer's descriptions of his own observations of soaring condors and hawks. It is one of the first books where an ornithologist accepts wholeheartedly the fact that a bird can soar only by means of rising air currents or velocity gradients. Gone are the nostalgic hints at a belief in levitation or some mysterious force which the older naturalists postulated to lift the birds they observed flying on motionless wings. Woodcock's important observations of soaring birds over the ocean are ably summarized, and so is the subject of slots and flaps in bird wings and their action as antistall devices.

The first portion of the book, however, is less clearly an asset to biological literature. On the basis of 16-mm motion pictures, an explanation is offered of the aerodynamics of the flapping wing, a thorny field where angels might well fear to tread even though armed with the best of data. As stated in the book, "In a feather at rest, we see not the shape that will be used in flying but a design that will automatically achieve (far different) shapes in response to different pressures from the air." Yet in this short treatment several rather dogmatic statements are made without qualification. For instance, "The outer half of a bird's wing, starting at the wrist, constitutes the propeller," and "the inner wing . . . is merely the handle that moves the propeller." These and other statements *may* be valid generalizations, but the figures do not establish them as such, nor does any other evidence known to this reviewer. Beyond the danger that the average reader will interpret many of the statements as established facts, when they can scarcely be more than plausible guesses, there is some confusion of thought and an apparent neglect of the European literature.

On page 28 one reads that "On the upstroke the wing tip moves upward and backward. By thus thrusting backward against the air it still drives the the bird forward." This implies that the wing tip moves backward on the upstroke not only relative to the bird's body, which is obvious, but also *relative to the air*. The latter is not true for most birds in forward flight, as can be seen in the figures on page 32. Thus the phrase "thrusting backward against the air" is misleading. At cruising speeds the whole bird is usually traveling fast enough so that relative to the air its wing tip always moves forward along a wavy path roughly like a sine wave. Pettigrew made this point eighty years ago, and Marey confirmed it by photographic methods. Perhaps the purpose of a discussion of the aerodynamics of bird flight might better have been served by translating selections from Stolpe and Zimmer's book, *Der Vogelflug* (Leipzig, 1939), or even by reprinting the remarkably lucid and accurate material in Pettigrew's *Animal Locomotion* (New York, 1874) or Marey's *Le Vol des Oiseaux* (Paris, 1870).

But the truly regrettable inadequacy is not in this little book, which scarcely pretends to be a thorough analysis of bird flight (except in the jacket blurb); rather it is the fact that so important a biological phenomenon as bird flight has received so little and such casual attention. Small birds at least could probably be trained to fly in a wind tunnel and the details of wing action photographed at close range. With clear pictures to determine the actual shapes and velocities of various parts of the wing throughout its cycle of movement, the aerodynamics of both flap-

ping and gliding flight could be described and analyzed far better than will ever be possible from motion pictures or visual observation of birds flying in the open. Here is a real opportunity for some experimental zoologist to break important new ground.

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UNIVERSAL RAW MATERIAL

The Coming Age of Wood. Egon Glesinger. 279 pp Illus. $3.50. Simon & Schuster. New York.

WOOD," asserts Dr. Glesinger, "will become the characteristic raw material of our civilization because it has three attributes which make it unique among all raw materials." It is universal. It is abundant. And it is inexhaustible.

He should know. Forests and their products have not had a more eloquent and enthusiastic champion in our time than Egon Glesinger. Now chief of the Forest Products Branch of the Food and Agriculture Organization, he came to the United States in 1941, having been forced by the Nazis to leave Europe, where since 1933 he had served as secretary-general of the International Timber Committee, originally sponsored by the League of Nations. Although a keen student of modern wood technology, his conception of the possible contributions of wood in all its physical and chemical aspects to man's welfare is that of an economist.

Fairfield Osborn's *Our Plundered Planet* and William Vogt's *Road to Survival* have conditioned us to think of land and resources from a global viewpoint. Glesinger's book beautifully supplements these splendid works by directing our attention to a particular kind of land and a particular kind of resource. Forests cover about 8,000 million acres, one fourth of the land surface of the earth. Although not all the world's forest resources are usable in a commercial sense, still only a fraction is utilized even after centuries of exploitation.

That wood as a raw material has satisfied many human requirements since earliest antiquity is a fact which it were almost platitudinous to repeat. But that wood as a raw material is capable of satisfying almost every requirement of existence is one of Glesinger's statements that will doubtless inspire some disbelief. But no scientist, or intelligent layman for that matter, should challenge the author's assertion without reading him and thus giving him an opportunity to prove his thesis.

We have long known that wood supplies shelter and fuel, that it produces food for men and animals. It is the world's second most important source of textile fibers. It is capable of supplying enormous quantities of motor fuels and lubricants. From wood come plywoods, plastics, paper products whose number is legion, and chemicals whose diversity and possible application are almost beyond our calculation.

> Tree extraction [says Glesinger], though practiced since the dawn of history, is still in its infancy. Yet of all the processes of forest chemistry, none is more suited to the nature of the growing tree than the extraction of the substances trees produce as living organisms. If all the extracts from all the world's trees were captured, they would add untold variety, color, and wealth to the arsenal of chemicals upon which mankind must be able to draw to achieve higher standards and well-being.

The shocking waste of wood, both in logging and in manufacture, by American forest products industries is an extravagance about which the author, with his European background of conservative utilization, is understandably critical. He makes a convincing case for integration of industry to end unreasonable competition for resources and to obtain closer utilization of raw material, including so-called waste products. He insists that in the interest of more equitable distribution of the wealth derivable from mass production of our forest resources for the benefit of all people, industry must rely more on technology and less on cutthroat competition. To obtain the complete utilization he deems necessary would require such radical reorganization of the forest industries as may be wholly impracticable in this era of still fairly abundant, though declining, resources. Still, he is able to show examples of such integration, not only in Scandinavia under the impact of economic and technologic development and in the Soviet Union under political five-year plans, but in the United States as well, where several large, financially secure, and family-dominated concerns have successfully integrated their production and forest-management operations. In short, it can happen here because it is happening.

That forest destruction is manifestly against the public interest is demonstrable fact, and it has been studied, and inveighed against, by the forestry profession for more than half a century. The solution of this problem, as Glesinger sees it, is public, that is to say Federal, regulation of private forest management by law. In this proposal he echoes the efforts and agitation of a long line of public officials, beginning with Gifford Pinchot and including Franklin D. Roosevelt, the last three chiefs of the U. S. Forest Service, and, most recently, President Truman. That public regulation of private forest management has been successful in certain European nations cannot be adduced as a reason for its imposition here. This is not to say that it won't work here; this reviewer merely desires to point out that under our democratic system the principle of Federal regulation has simply not become a Congressional issue, and not entirely because of opposition by the so-called forestry industry lobby. Dr. Glesinger is a distinguished forest products economist, not a professionally trained forester, which are two different things. Thus it is no disparagement whatever of his book to suggest that he is more convincing in his advocacy of intensi-

fied research and industrial integration than in his plea for silviculture by law.

This book is worthy the earnest attention of every American who may have the least interest in the role which modern technology applied to forest resources may play in increasing industrial prosperity and human comfort.

HENRY CLEPPER
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THE MEANING OF ANTHROPOLOGY

Mirror for Man. Clyde Kluckhohn. xi + 313 pp. $3.75. Whittlesey House. New York.

THE title of this book, which took first prize ($10,000) in a publisher's contest for scientific books last year, is especially apt. Anthropology, as the science of man, includes a vast range of material. Professor Kluckhohn has superbly presented a reflection of the scientific picture of man in his totality. Like any mirror image, there is a surface completeness, with very limited detailed. This is, however, necessary to the purpose of the presentation. As clearly stated by the author in his preface, "This book is intended for the layman, not for the carping professional. . . Had I entered all the qualifications and reservations required in a technical study, the intelligent layman would stop before the end of the first chapter."

Any professional anthropologist who has tried to convey to the layman the meaning of anthropology as a science, and its application to the world of today, must appreciate the clarity and easy communicability of the author's presentation. The teacher who has tried to summarize an introductory course in general anthropology will find this book an outstandingly desirable one for reference to his students. It is an ideal companion to any text in the field.

The first seven chapters which present summaries of ethnology, archaeology, physical anthropology, and linguistics, together with their applications in the past and present, and their interrelations with one another, are outstanding in this latter connection.

The eighth chapter, entitled Personality in Culture, presents a conservative picture of the time-honored anthropological concern of the interinfluences of the individual and the group. There is also some attempt to give a picture of the various psychological approaches being used in this special field today. The reviewer feels that the author has perhaps been overcautious with respect to this latter portion of the subject.

The two final chapters, An Anthropologist Looks at the United States and An Anthropologist Looks at the World, are statements of the potentialities of the science in analyzing contemporary cultures on broad bases. Here we find a picture of what anthropology has done in this field in the past, what it is doing today, and what it may be expected to contribute in the future. They are the least satisfactory chapters in the book, because many of the points made explicit here have been more strongly presented by implication in the preceding portions of the volume. Nevertheless, they are correctly included in a book of this kind.

The book is interestingly written, absorbing, and eminently successful in achieving its purpose. It fills a long-standing need in scientific literature for the layman.

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FANCY AND FACT

The Lungfish, the Dodo, and the Unicorn. (Rev. ed.) Willy Ley. xi + 361 pp. Illus. $3.75. Viking. New York.

IN THE introductory chapter to this "excursion into romantic zoology," Willy Ley outlines the literature of natural history from Aristotle and Pliny, through Gesner and Linnaeus, to Cuvier, Buffon, and Darwin. But in tracking down legends of animals that never existed, such as the unicorn and the dragon, he has gone through an enormous amount of classical and medieval writing, and assembled his clues with as much suspense and thrill as would the writer of a modern "who-dun-it." The unicorn can be blamed, in part, on a mistranslation of the Old Testament from Hebrew into Greek; paleontology has substituted the woolly mammoth for some of the giants of legend; and philologists have offered interesting linguistic explanations for such myths as those of birds that grow on trees.

The first part of the book deals with animals that never existed; the second part tells of animals that actually lived but are now extinct; and the third part, Witnesses of the Past, treats of bizarre creatures of ancient origin which, had they followed the pattern of their kind, would be extinct today. The chapter As Dead as the Dodo points out that the bird really became well known only after its extinction; for about a hundred years Dutch and Portuguese navigators brought stories of it, and occasional specimens, back to Europe, but when naturalists went to Mauritius to look for it, it had vanished so completely that residents of the island doubted that it had ever occurred there.

New Zealand Interlude is a delightful chapter on the fauna of a region where ancient species have maintained themselves by being isolated from invading enemies. The Ituri forest in the Congo, another lost world, is the home of creatures that really should be fossils by this time, such as the okapi and the pangolin. In contrast to these survivors, who have been protected either by island or forest isolation, Ley points out that Limulus, the "horseshoe crab" of the North American Atlantic Coast, has three claims to fame: "First, it is one of the oldest living animals; second, it is probably the most numerous

living fossil; and third, it has survived not on some carefully isolated and protected island but in the open sea."

Out of the mass of data that Ley has presented with contagious enthusiasm, I have found one or two minor inaccuracies. The orangutan is not extinct in Sumatra—I obtained a fine large specimen there in 1937. And in 1948 I saw a sizable group of Heck's reconstructed "wild forest horses," which managed to survive the repeated bombings of the Munich Zoo.

In Rumors and Shadows Ley has collected many fascinating native tales of animals unknown to science. As he retells the story of the comparatively recent findings of the okapi and the pigmy hippopotamus, and of Jimmy Chaplin's melodramic search for, and discovery of, the African peacock just a few years ago, one realizes that in African or South American jungles there may still be even larger animals yet to be found.

This new and enlarged edition of a previously published book is so delightful and well done that the reader is glad to have it again available.

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SCIENCE AT THE SOURCE

A Source Book in Greek Science. Morris R. Cohen and I. E. Drabkin. xxx + 597 pp. Illus. $9.00. McGraw-Hill. New York.

Studies in Philosophy and Science. Morris R. Cohen. 278 pp. $4.50. Henry Holt. New York.

IT HAS been the glib intellectual fashion for some time to award the ancient Greeks the palm for preeminence in art, literature, and philosophy, but to say that Greek science, because the Greeks lacked machines and precision instruments, was a rudimentary and retarded affair. The *Source Book,* a selection of Greek writings on mathematics, astronomy, mathematical geography, physics, geology and meteorology, biology, medicine, physiological psychology, and chemistry, refutes that view. It also serves as a positive reminder that the basic requirements of science are a seeing eye, a hearing ear, and a talent for speculative theory—attributes which the Greeks had in surprisingly modern degree.

Professor Cohen, one of the two or three most dynamic influences in recent American philosophy, was an ideal choice for co-editor of the *Source Book.* As appears from his *Studies in Philosophy and Science,* he is primarily in the Greek philosophical tradition; and, interested though he was in all facets of learning, his basic predilection was for the Greek-born disciplines of mathematics and the natural sciences. In fact, Professor Cohen's concurrent grasp of recherché philosophy and mundane scientific matters is in the best Hellenic tradition. Aristotle, it may be remembered, contributed to Western civilization not only the classic canons of deduction and incidental insights on such matters as the sphericity of the earth, and the role of slipping of earth masses and subterranean gases in earthquakes, but also astounding observational detail, classificatory technique, and the teleological approach to human and animal biology, zoology, and psychology.

Dr. Drabkin, co-editor of the *Source Book,* who has translated the hitherto untranslated passages in this compilation, is a professor of mathematics.

Unlike the technical virtuoso in a special field, Professor Cohen is interested in the basic problems of scientific method rather than, let us say, some dazzling notational exercise, an interest which lends stimulation both to the *Source Book* and the collection of *Studies.* The *Source Book* implicitly, and the *Studies* explicitly, demonstrate the simple and elementary nature of the fundamental ideas and advances of science. Such a realization should give the scientist a humility that is vitally needed in our braggart and technically proud civilization. Furthermore, no student of Greek culture can escape, even in the scientific field, the feeling that Greek thinkers and scientists were in the service of the Greek political states, voluntarily as free men and not in the servile Nazi or Soviet fashion. (I wonder how the formulator of the Hippocratic oath would vote on the Compulsory Health Insurance Bill?)

There are excluded from the *Source Book* the myths, superstition, and astrology that occasionally characterize even some of the soberest Hellenic chroniclers of science. The main effort of this volume is to achieve comprehensive coverage of topics: in 558 pages of text, it is the rare selection that takes up more than 5 pages, and a great many occupy only a half page or page of print. Furthermore, the authors have shown commendable asceticism in confining their notes and footnotes to the essential task of clarifying their texts and putting them in their historical setting, and have refrained from overexercising their philosophic erudition. Readers who desire to gain broader or more intensive insights can, however, consult the valuable 10-page bibliography at the end.

Professor Cohen's views on the nature of the physical world and the mathematical and physical sciences are set forth more comprehensively in his earlier volume *Reason and Nature.* In the current *Studies,* students of science will enjoy his slashing attack on Francis Bacon as a shallow scientific amateur who propagated the fallacy that observation unaided by theory can result in knowledge; and his respectful but firm and meticulous analysis of why he—as a man primarily interested in the nature of the world—must needs differ from the anthropomorphically minded John Dewey. Unlike John Dewey, Professor Cohen's criterion of "truth" is not pragmatic success in achieving human ends, but a logically consistent system setting forth with maximum simplicity and directness relationships among objective entities. Thanks to Charles Peirce, these relationships are not invariant, but admit of chance deviations. They are subject to the principle of "polarity"—i.e., the physical universe can largely be described as an equilibrium between contradictory principles or concepts.

The *Source Book* series is sponsored by the AAAS, among others, and it can be congratulated upon this volume on Greek science as a worth-while and durable work of scholarship and reference. Scientists without philosophic training can find food for thought even in the solely technical yet lucid philosophic essays in *Studies in Philosophy and Science*.

SIGMUND TIMBERG

Department of Justice
Washington, D. C.

EXPLORING THE OCEAN DEPTHS

Submarine Geology. Francis P. Shepard. xvi + 348 pp. Illus. $6.00. Harper. New York.

DURING the second world war, submarine exploration in the coastal waters of the various war zones extended knowledge of the sea floors. In the course of these investigations, new methods for determining positions at sea and improved equipment for exploration were developed.

Submarine Geology summarizes the available information in a brief and readable text. The first five chapters survey the methods of exploration, waves and currents, shore lines and beaches. The descriptive classification of shore lines presented by Dr. Shepard is no more satisfactory than those he condemns, and its position in the volume breaks the continuity of the discussion of marine processes. If the whole purpose of shoreline classification is to enable a student to differentiate configurations due primarily to nonmarine and marine agencies, then the proposed classification may have some merit. Unfortunately, the geologist expects something beyond such a goal. A classification should not only differentiate between various features, but also should indicate interrelationships, possible antecedents, and future developments of a land form. The proposed classification fails to fulfill these requirements. Furthermore, Dr. Shepard's inclusion of irregular shore lines with small bays, prominent points, and stacks in a group entitled "sea cliffs made irregular by wave erosion" might well be challenged. As he himself so carefully illustrated in an earlier chapter on waves and currents, the headlands bear the brunt of wave action, whereas the estuaries suffer but minor attack. Does this fact not indicate that the irregular shore line is inherited from some other agency, and that it is but an ephemeral feature in geologic time? The sea's role is smoothing out irregularities, not making them.

The last seven chapters describe the principal features of the sea floors of the world. This portion of the book should be of interest to the landlubber geologist, who has little time to keep up with submarine exploration. It is to be regretted that the author did not begin this section with a description of the shore lines of the world, treating them in the same order as the material in later chapters—that is, by tracing them around the continents and islands. Such a treatment of shore lines might have made a desirable substitute for his dubious classification, harmonized with the balance of the volume, and provided some transition between the first and second parts of the book.

Although Dr. Shepard's interpretation of the origin or significance of the features described may in some instances be open to question, he is to be congratulated for briefly summarizing a large mass of data and for providing numerous references for those who wish to pursue any aspect of the subject in greater detail The illustrations are generally adequate, despite the limited geographic range of the photographs. Unfortunately, the depths on many of the charts have suffered so much reduction in size in the process of reproduction that they are illegible.

Students and geologists will find *Submarine Geology* useful as a reference, though perhaps too brief to serve as the text for a course. It is hoped that future texts in this field will be more detailed than this pioneer endeavor and that integration between descriptions of different portions of the ocean floor and processes of formation will be more thorough. Brevity, if carried to extremes, may border on the superficial.

ELIZABETH W. OLMSTED

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MAN IN THE ANDES

Acclimatization in the Andes. Carlos Monge. xix + 130 pp. $2.75. Johns Hopkins Press. Baltimore.

CARLOS MONGE, one of America's outstanding scientists, has made the field of altitudinal adaptations in man peculiarly his own. His work is a fine example of the importance of exploiting the advantages of a local situation. Monge lives in Peru, where man has achieved the greatest vertical distribution in permanent habitation known on our planet, and it occurred to him to make detailed investigations of this situation, with results that are of interest to physiologists everywhere—that should be of interest, indeed, to all biologists and to students of the social sciences as well.

Unfortunately, I think, Monge makes only passing mention of contemporary work, whether his own or other people's, in the present book. It consists almost entirely of a study of the records and observations of the Incas and of colonial Spanish officials on the effects of change of altitude on individuals and populations, and on the recognition of these effects in folk customs and in social legislation. He is chiefly underlining the neglect of this subject in Peru since the country achieved independence, which gives the book a slant that was important for the local audience, but that has less relevance for the readers at whom the translation is aimed.

The accounts of the Incan customs are, however, fascinating enough in themselves, and serve as vivid evidence of the ways in which that complex culture

was adapted to its distinctive environment. A chapter on Altitude and Military Operations shows the disastrous effects that can result from ignoring altitudinal adaptations, as happened during the colonial wars. Monge has a slight tendency to use big words, like "climatic aggression" and "bio-climatic determinism," that have, to me, a confusing effect; and I wish that the historical account had served as an introduction to an account of his own observations and experiments in which his phraseology and theories could have been more clearly defined.

The book has a foreword by Isaiah Bowman, and the English translation by Donald F. Brown reads very smoothly.

MARSTON BATES

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THE EUROPEAN FLAMINGO

Die Flamingos der Camargue Etienne Gallet. 127 pp. 56 figures. Verlag Werner Krebser & Co. Thun, Switzerland.

IT IS strange that the flamingo, one of the most local and most striking in appearance of all the birds of Europe, has never been made the subject of a special monograph. This omission is now rectified by the appearance of this little book, and a very satisfactory account of the species it is. The flamingos are birds of great interest to naturalists from several angles, and have been the direct goal of many ornithological pilgrimages to the desolate marshes of the Camargue, the only part of France where these large fowl may be sought with any assurance of success.

The detailed account of the habits of the European flamingo given in Gallet's book and the excellent photographs that illustrate it make the volume a companion piece to the late F. M. Chapman's well-known paper on the life history of the American flamingo, published in 1905 by the American Museum of Natural History. As might be expected, the two species are essentially similar in their habits, although it appears that the truncated conical mud nests built by the American species are higher structures on the average than are those of the European one; it also seems that the young of the latter tend to gather in flocks earlier than do the chicks of the former species These are, however, very tentative conclusions from a comparison of Gallet's and Chapman's papers, and may not be upheld by additional data of future observers.

The book is divided into two sections, one dealing with the life of the flamingos and the other with supplementary observations on special topics. The former section treats of the habitat requirements, the appearance and wanderings of the birds, their food, their pairing and family life, the duration of incubation, the growth and development of the young, and the enemies of the species. The second part, much shorter than the first, gives additional data on food, nests, eggs, a list of the known breeding colonies in the Camargue from 1914 to 1947, measurements of a series of specimens of the birds, and a useful bibliography. There is a short foreword by Professor Hediger, of the Zoological Park at Basel. The volume is handsomely made and attractive to look at. There is no index, but as the whole book is quite small this is not a great handicap.

HERBERT FRIEDMANN

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SMITHSON'S WHIM

Sons of Science. Paul H. Oehser. xvii + 220 pp. Illus. $4.00. Schuman. New York.

SO far-flung is the renown of the Smithsonian Institution, not only as a landmark in our nation's capital, but also as an international symbol of the importance and value of science, that it comes as rather a surprise to have it called to one's attention that were it not for the whim of a wealthy but lonely Englishman—who had never seen the United States—the institution would never have come into existence.

In 1829 James Smithson's bequest of money for the founding of an institution at Washington "for the increase and diffusion of knowledge among men," as stipulated in his will, was dropped so suddenly and unexpectedly into the lap of the Congress of the United States that it took this august body ten years to decide what to do with it. What has finally materialized is not a single institution but a whole family of them, far exceeding the dream of the original donor—and far exceeding the funds he provided as well. At present the Smithsonian family, partly supported by Congressional appropriations, consists of the United States National Museum, the International Exchange Service, the Astrophysical Observatory, the National Collection of Fine Arts, the Bureau of American Ethnology, the National Zoological Park, the Freer Gallery of Art, the National Gallery of Art, the National Air Museum, and the Canal Zone Biological Area.

In *Sons of Science,* Paul H. Oehser, since 1931 editor for the United States National Museum, and since 1946 assistant chief of the Smithsonian's Editorial Division, tells the colorful story of this cultural and scientific center. Through a description of the lives and activities of the men who directed its progress since its founding he relates how the institution was founded, how it was housed, how its various divisions were conceived, and how they thrived, reached maturity, and sired still other offspring.

In chronological order of services rendered, these men are as follows: Joseph Henry, famous American physicist and first director of the Smithsonian; Spencer Fullerton Baird, biologist, who is often called the

father of the National Museum; George Brown Goode, another biologist and a museum expert; Samuel Pierpont Langley, world-famous astronomer and pioneer in "flying machines;" Charles D. Walcott, geologist; Charles G. Abbot, astrophysicist; and Alexander Wetmore, well-known ornithologist and present Smithsonian secretary. Our national house of science, the Smithsonian, has indeed been the home of illustrious Sons of Science.

Slanted for the general reader, the volume should do much to eliminate our lacunae concerning this absorbing bit of American history. It constitutes a welcome addition to the "Life of Science Library."

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NONINDUSTRIALIZED MAN

Man and His Works. Melville J. Herskovits. xviii + 678 + xxxvii pp. Illus. $6.75. Knopf. New York.

LIKE Herskovits' previous writings, this volume is an excellent job, well written, and represents a real contribution to the social sciences. The purpose of *Man and His Works* is summarized by the author as follows:

> As a scientific discipline, anthropology has amassed an impressive body of materials, and has reached substantial conclusions about the nature, processes and functioning of human groups and their modes of existence. It is these conclusions, and such of the factual materials as are necessary to document them, that form the core of this book. It is hoped that a unified treatment of the entire field of anthropology will be of aid in giving insight into the kind of world we live in, and why it is that kind of world (p. ix).

Separate sections are devoted to the presentation and interpretation of anthropological findings in the fields of the nature of culture; the materials, structure, and aspects of culture (including separate chapters on such topics as Technology and the Utilization of Natural Resources, Political Systems, Religion, etc.); cultural dynamics; and cultural variation. The last chapter, Anthropology in a World Society, attempts to portray the role which cultural anthropologists can play in an understanding of our present-day industrialized society. Practically all the anthropological materials introduced relate to "primitives" or "nonliterate" peoples from Africa, the Western Hemisphere, and Oceania; very few materials relating to Oriental cultures are introduced.

The title of this book, its stated purpose, and the chapter Anthropology in a World Society suggest an all-inclusiveness, so that the reader expects to find at least two subject areas covered more adequately than they are. The first is our modern industrialized Euro-American culture, in particular, as it is similar to and differs from nonindustrialized cultures; the second consists of the writings and findings of social scientists other than professional anthropologists. The author has followed the custom of most anthropologists by limiting himself to anthropology as conventionally taught in our universities. However, in so doing, it should be noted that the relationship between nonindustrialized cultures and our modern industrialized culture has not been adequately established, despite the author's statement that study of cultural anthropology ". . . will be of aid in giving insight into the kind of world we live in, and why it is that kind of world." He does attempt to differentiate cultural anthropology from sociology and the other social sciences; it appears, however, that he has demarcated cultural anthropology as a separate and distinct field of research and study only by limiting himself to societies other than our modern industrialized one.

By so limiting himself, he has written a book which discusses nonindustrialized man rather than *all* men and *all* cultures. The considerable amount of insight into the problems of our modern society that might result from a comparison of industrialized and nonindustrialized societies is largely missing. Also missing are many relevant findings from sociology and other social sciences. As a volume in the field of anthropology as commonly taught in our universities today, this book is excellent; in so far as it is claimed that cultural anthropology as distinct from the other social sciences has significance for our modern industrialized society, cultural anthropologists have yet to prove their case.

A. J. JAFFE

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MICROBIOLOGY

Microbes Militant. A Challenge to Man. Frederick Eberson. ix + 401 pp. Illus. $4.50. Ronald Press. New York.

The Biology of Bacteria. (3rd ed.) Arthur T. Henrici and Erling J. Ordal. xiv + 577 pp. Illus. $5.50. Heath. Boston.

THE common subject matter of these two books happens to be microbiology. Here their similarity ends.

The first of the books, by Dr. Eberson, assistant chief of the laboratory service in the Veterans Administration Kennedy Hospital in Memphis, Tennessee, is a revision of his earlier work, *The Microbe's Challenge,* published in 1941. It is essentially a popularized discussion of the rise of medical bacteriology and its role in preventive medicine.

In his opening chapter the author describes in simple terms what bacteria are, how they live and breed, their relation to disease, and how the human body attempts to ward off microbial attacks. Having thus introduced his subject, Dr. Eberson tells in lively fashion the now familiar story of how bacteria were discovered with homemade lenses by Antony van Leeuwenhoek in 1676 and how this discovery languished for more than a century and a half, until such men as Henle, Cohn, Pasteur, Koch, Lister, Tyndall,

Smith, and a host of other workers established the new science of bacteriology.

Still in a historical context, the story continues with a discussion of parasitism, in which the reader is introduced to a few nonmicrobial parasites, such as *Trichinella spiralis,* some insect vectors of disease, and certain protozoal and rickettsial agents of human misery. Then follow chapters on bacterial variation, bacterial chemistry and serology, viruses, bacteriophages, and antibiotics. The book concludes with a historical discussion of epidemiology.

This new version of Dr. Eberson's book has been enlarged to include many of the advances in our knowledge of infectious agents resulting from the war. The author is at pains to point out, however, that our ever-increasing stock of knowledge concerning microbes is no cause for complacency—that the problem of infectious disease is still very much with us and offers a continuing challenge to the human race. Dr. Eberson is to be commended for having produced an interesting and readable book useful to the general reader as well as to students of bacteriology and of medicine.

One last comment: On page 25, line 11, Leeuwenhoek is described by the author as having quit his job as a draper's assistant to become "*Kamerbewaarder der kamer von Heeren Schepenen van Delft.*" This Dutch sentence Dr. Eberson has loosely translated as "In other words, he was custodian or janitor of Mr. Schepenen's rooms." This is not quite fair to the Father of Protozoology and Bacteriology. A more precise rendition of this phrase indicates that *Heeren Schepenen van Delft,* means the Aldermen of Delft. Mr. Leeuwenhoek was in fact the custodian of the Council Chamber for the Aldermen of Delft, a somewhat different position from that of personal valet to the mythical Mr. Schepenen. This not only appeared in the first version of this book, it is repeated in this second edition. It is permissible to hope that Mr. Schepenen's ghost will have been laid to rest when a third edition of *Microbes Militant* appears.

The Biology of Bacteria, in contrast to *Microbes Militant,* is a straightforward textbook of general microbiology. Those who are familiar with the earlier editions of this book by the late Professor Henrici, of the University of Minnesota, will scarcely need an introduction to this revised edition. Suffice it, therefore, to point out that the new version admirably preserves the spirit of the original work. There are 83 more pages in the new edition as compared with the last; this does not indicate the extent of revision adequately, since the entire book has been redesigned and reset, so that there is actually more textual material than the increased number of pages might suggest.

The chapter on the microscope and microscopy now includes a discussion of the phase microscope, the electron microscope, and of ultraviolet and fluorescent microscopy; and it must be mentioned incidentally that some excellent electron micrographs appear as illustrations. Nine pages devoted to fungi in the second edition have been expanded to 21 pages in this one, reflecting the growing importance of this group of organisms. In so far as it could be done in an introductory text, the newest available data on every phase of microbiology, from bacterial metabolism to viruses and rickettsias, have been included.

Despite its general excellence, one gentle criticism of this book must be voiced. It is in regard to the absence of any list of additional readings. Any book written as this one is—on a beginner's level—tends to become dogmatic. In an effort to be all-inclusive, the authors fail to leave room for qualifying remarks. In such a case, one remedy seems to be to expose the student to a wider view of the subject through well-selected lists of references. Thus the student can more readily see the subject as in a state of flux and growth rather than as a static body of accepted dogma useful chiefly for passing examinations.

Morris C. Leikind

Library of Congress
Washington, D. C.

MARINE BIOLOGY

Between Pacific Tides. (Rev. ed.) Edward F. Ricketts and Jack Calvin. xxvii + 365 pp. Illus. $6.00. Stanford Univ. Press

IN THE decade between the first publication of *Between Pacific Tides* (1939, reviewed in The Scientific Monthly, 50, 274) and the appearance of this revised edition, the public has had unusual opportunity to learn something of the character of its senior author. As "Doc" in John Steinbeck's *Cannery Row* and as collaborator on that same writer's *Sea of Cortez,* the late Edward F. Ricketts has become known to a far wider circle of readers than most marine biologists ever do. In that same decade the original edition of *Between Pacific Tides* proved to be a stimulating introduction to intertidal invertebrates for the amateur and beginner, which is the avowed purpose of the volume, but in addition it has also become established as a useful handbook for the student of invertebrate zoology and ecology.

The changes appearing in the revised edition will, in the main, increase the book's effectiveness from the biologist's point of view by some additions and by bringing the references up to date. The bulk of the text, photographs, and drawings are nearly identical with the 1939 edition. Their arrangement, too, on a faunistic ecological plan, likewise remains unchanged. For this reason certain material in which one might have hoped for revision is still in its original state. In particular, some of the drawings, such as those of coelenterates, are scarcely deserving of the fine presswork and bookmaking that have gone into their presentation. The same is true of a few of the otherwise excellent photographs, mainly the work of the junior author.

The new material in the revised edition consists of a frontispiece in color; a new section of 34 pages,

including 17 additional figures, on plankton; and 12 pages of interpolations in the systematic and bibliographic appendix, which bring the latter up to 1947 in coverage. Most of the volume is written in a series of faunistically arranged paragraphs, each dealing with specific organisms. The additional material on Pacific marine plankton, however, forms a highly readable continuous account emphasizing the importance of the plankton community for the largely benthonic communities dealt with in the rest of the book. This section, with its bibliography and the new material in the appendix, should add considerable value to a book already proved to be very useful as an introduction to our Pacific coastal fauna.

TALBOT WATERMAN

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AVOIDABLE MORTALITY

How to Live Longer. Justus J. Schifferes. 255 pp. $3.00. Dutton. New York.

HEALTH education is a field in itself. The author of this excellent discussion of preventable and premature death has been active in this field for many years and, although not a physician, is fully qualified to write upon his subject. There are many approaches: statement of fact, pleading, and explaining the meaning of facts are three of the most useful. In the present volume, J. J. Schifferes has employed all three. The book is crammed full with facts, especially statistical and mortality data concerned with the major causes of death today. There are also much pleading to avoid unnecessary suicide by living more wisely, and some rules to follow. The facts are sound, the pleading is sincere, but the author, trying as he is to simplify the problem of avoidable mortality, cannot escape generalization and oversimplification.

There is too little explanation of the meaning and implications of some of his statements. It is true, he points out, that if more people avoided obesity by controlling their appetites, we would have less diabetes mellitus and therefore fewer deaths from diabetes. But this does not suffice. To be successfully guided, the obese person must know *why* he eats too much and to be assisted in resolving the anxieties which create the need for an escape mechanism in stuffing himself. Individualization and the personal touch are lacking. Throughout the excellent factual text one feels the author is thinking and dealing with "paper" men and women, digits in statistical tables. Similarly, his recommendations are largely on the "wholesale" level—public health agencies and organizations are to be the major factors in developing the utopia of longevity with health. Though there is adequate and repetitive emphasis of the need for assuming personal responsibility for one's health, it lacks conviction and one feels the author has an exaggerated faith in the intelligence, will, and emotional maturity of mankind. I do but wish mankind were sufficiently grown-up to follow intelligently the excellent and fundamental advice offered, even in general terms, but this is only a wish, and wishes are not convincing in science The "scare" technique herein employed has repeatedly fallen short of expectations, tried as a crime deterren , severe penalties have not reduced the incidence of crime; horror pictures have failed again and again to appreciably reduce promiscuity and therefore venereal disease. The author has studied the basic facts about disease and presents them well, but his understanding of men and women is remote, vague, idealized, and unrealistic.

There is much to be learned by a careful study of this book, and both good and bad can come of this work. Some few people will be stimulated to take better care of themselves and seek individualized personal guidance; others will be frightened, and their hypertensive disease or ulcers exacerbated. It is, however, recommended reading for those who seek facts.

EDWARD J. STIEGLITZ, M.D.

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FRESH FISH FOR THE ARMY

Fishing in Troubled Waters. Wilbert McLeod Chapman. 256 pp. $3.00. Lippincott. Philadelphia.

THIS is a narrative of the adventures of the vessel and the crew which undertook to provide fresh fish for the United States Army, behind the battle front in the Southwest Pacific, in the closing period of the war—written by the scientist in charge of operations. Though far from a scientific record, it provides interesting side lights on physiography, weather, and potential fisheries of the islands, not to mention personalities involved in the military organization, and of native islanders.

Bits of natural history are unobtrusively scattered through the book. There is an exceedingly interesting description of the tridachna ("maneater") clams in life, the larger of which might easily prove disastrous for one stepping by mischance into an open shell. The fish fauna in these waters is abundant and tremendously varied. By spreading derris root along the coral in shallow water at one point, diving after and securing the small fish as the poison began to affect them, 472 fishes belonging to 121 species were collected "in one small spot, no more than half an acre in area." We read that "Many of these were most peculiar, and they were generally much more highly colored than the fish with which we were acquainted in northern seas."

Food fishes of the reefs are notably difficult to secure in quantity, but it was such that were immediately available and had to be drawn on, especially surgeon fishes and parrot fishes, it seems. Though taken with nets, the former had to be handled with spears. "Because of the sharp folding spine on the base of their tail it is not possible to touch these fish while they are alive." "Almost all of the many kinds of parrot fish carry some bit of brilliant color about their persons, and many are a blaze of brightly con-

trasting hues all over. . . . Contrary to some medical opinion their flesh is not only not poisonous but is delectable."

Though the war was barely out of sight below its horizon, the book is a fascinating tale of adventure, replete with color, human interest, and typically American humor, whereof some exaggeration no doubt is a part.

J. T. NICHOLS

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BIOGRAPHICAL NOTES

The Life Story of the Fish, His Morals and Manners. Brian Curtis. xii + 284 pp. Illus. $3.75. Harcourt, Brace. New York.

THERE is probably no group of animals about which more questions are asked and answered less properly, or about which more misinformation is in general circulation, than fish. This unfortunate state of affairs comes about because man has always fished for one reason or another and because fishes, by the very nature of their medium, appear to be mysterious. Certainly they cannot be studied easily without more equipment than is readily available.

Brian Curtis' book should do a great deal to remedy these matters, for he has brought a tremendous amount of knowledge together and written it in as simple and engaging a manner as seems possible, all without compromising in the least the scientific principles by which the information was assembled.

It is hard to conceive how the whole feeling of the living of a fish could be better explained, from how a fish is born, feeds, grows, reacts and adapts to its environment, how it sleeps, and why, for many groups, it does what it does. Even such highly technical aspects of fish living as what it sees, and how, are treated completely and simply.

The principles and practices of conservation measures in fish management, of fundamental importance to the millions of anglers, and the effects of water conservation measures on both game and commercial fisheries are all brought under understanding scrutiny.

The absence of footnotes and reference marks makes for easy reading, and the inclusion of a "Selected References" section, by chapters, directs the curious to the proper authority for detailed information.

One or two small slips—for instance, the somewhat slack use of the terms "oxygenation" and "aeration"—do not detract seriously from the excellence of the book. It is by far the best popular exposition of fishes and their living we have yet seen.

CHRISTOPHER W. COATES

New York Aquarium
New York Zoological Society

REVISED CLASSIC

An Introduction to Mathematics. A. N. Whitehead. (First published in 1911. 12th impression. First American ed., revised and reset, 1948.) v + 191 pp. $2.00. Oxford Univ. Press. New York.

AS THE jacket claims, "this is one of the few contemporary works that can with safety and justice be called a classic." It was originally written for the Home University Library, and was better known a third of a century ago than it is today. The revisions are limited to the redrawing of diagrams and the correction of "a few inaccuracies and typographical errors . . . , but in no instance has the author's original wording been altered." It may be noted that not all the desirable corrections have been made. To cite only one of several, Abel is assigned to Sweden, which will not be exactly pleasing to Abel's compatriots. Again, the author states that "for the detailed historical facts relating to pure mathematics, I am chiefly indebted to *A Short History of Mathematics,* by W. W. R. Ball." Ball's *History* was outdated even in 1911. In the thirty-seven years since Whitehead's *Introduction* was first published, the detailed historical facts relating to pure mathematics have been radically revised.

The purpose of this classic of 1911 was to give the general reader an appreciation of the spirit of mathematics and to indicate how mathematics is applicable to natural phenomena. Some of the material is still adequate, if rather dull, but as a whole the book is hopelessly dated. The spirit of mathematics has not remained stationary in the past thirty-seven years, nor have its applications been confined to the problems of the eighteenth and nineteenth centuries. As the book has been available for over a third of a century, there is no need here to summarize its contents. In treatment it is too elementary and too sketchy for a college mathematics major in his first term, and probably too difficult for the general reader who has not thoroughly assimilated a comprehensive course in secondary-school mathematics. Need even an introduction to mathematics dwell almost exclusively on the antiquated and the obsolescent? Possibly; but it seems a pity that this first American edition of Whitehead's popular classic was not drastically revised and amplified in the spirit of 1948.

Before turning to philosophy as his major interest, Whitehead did much memorable work in mathematics, both pure and applied. It is interesting to observe the changing cast of his thought in this book, written when he was about to abandon mathematics. Prophetically—when some passages in his later writings are recalled—he observes (p. 170) that "It is a safe rule to apply that, when a mathematical or philosophical author writes with a misty profundity, he is talking nonsense." Ipse dixit.

E. T. BELL

California Institute of Technology

CORRESPONDENCE

AESTHETIC

In thumbing through my husband's copies of THE SCIENTIFIC MONTHLY, I have been both intrigued and delighted by the rare appearance of exceptionally fine poetry. It all possesses both vigor and beauty, and I feel is the one thing that completely rounds out your magazine, giving it the aesthetic touch, or a touch of the "science of the beautiful," in contrast to what, in most minds, are the great obscurities. We delighted in "The Copperhead" and "Telescope on Mount Palomar," to mention only two.

MRS. T. D. URBAHNS

Live Oak, California

USEFUL

The article by M. K. Bennett, "Population and Food Supply: The Current Scare" (January 1949), has recently been brought to my attention as representing an authoritative statement on the subject, counteracting some of the less well-founded opinions being expressed by some writers.

It is my hope to present in a course in Food Economics, which I am currently teaching, a number of the viewpoints held today on this problem, and I feel that Dr. Bennett's article would make an excellent contribution in this regard.

ODIN WILHELMY, JR.

Department of Agricultural Economics
Cornell University

SCIENCE AND TECHNOLOGY

Relative to your press release item on Snow-melting Highway in the January 1949 issue, I am sending you a clipping from *The Detroit News* (February 4, 1949, p. 40) which might interest you. The 500-foot strips of highway mentioned here are heated by electricity:

> FOR $100 YOU CAN HAVE A SNOWLESS WALK
>
> "Householders can look forward to ice-free sidewalks and driveways all year for about $100 annually, and the average store owner can expect to forget about ice and snow on his sidewalk for $50 a year.
>
> "These are the figures Harold Wall, assistant superintendent of the Public Lighting System, says it would take to heat the walks with radiant heat on the basis of the experiment with Michigan's 'warm highway' on Eight Mile road between Livernois and Wyoming avenues.
>
> "For an average of $250 each for the two 500-foot strips all of the snow that has fallen on them since they were constructed in November has been melted, Wall said. That will make the annual cost of heating the highway about twice the cost of lighting the same area, he said.
>
> "'A storeowner with a sidewalk 20 feet long and six feet wide could keep the walk clear of ice for $50, based on the rate for heating the highway,' Wall said.
>
> "'The sidewalk in front of the average home and a track on a 100-foot driveway could be heated for $100 a year,' he added."

GEO. P. LOWEKE

Wayne University

FEDERALESE

In the contest to translate English into Federal Prose, first prize, a coat of arms showing a stuffed bureaucrat rampant on a bound volume of the *Congressional Record,* the gift package, neatly done up in red tape, goes to Ellwood H. McClelland, who has just retired from the Carnegie Library of Pittsburgh. He has translated "A rolling stone gathers no moss," and, for conciseness, has held it to one sentence:

"A detached fragment of the terrestrial lithosphere, whether of igneous, sedimentary, or metamorphic origin, and whether acquiring its approximation to sphericity through hydraulic action or other attrition, when continuously maintained in motion by reason of the instrumentality of gravitational forces constantly acting to lower its center of gravity, thus resulting in a rotational movement around its temporary axis and with its velocity accelerated by any increase in the angle of declivity, is, because of abrasive action produced by the incessant but irregular contact between its periphery and the contiguous terrain, effectively prevented from accumulating on its external surface any appreciable modicum of the cryptogamous vegetation normally propagated in umbrageous situations under optimum conditions of undeviating atmospheric humidity, solar radiation, quiescence, and comparative sequestration from erosive agencies."—*The Pleasures of Publishing,* issued semimonthly by Columbia University Press.

LIFE CYCLE

Biosis
Meiosis
Zygosis
Mitosis
Morphosis
Eclosis
Heterosis
Macrosis
Adiposis
Pediculosis
Psychosis
Diagnosis
Thrombosis
Sclerosis
Prognosis
Necrosis

JULIAN D CORRINGTON

University of Miami

THE

SCIENTIFIC MONTHLY

JUNE 1949

WALPURGIS WEEK IN THE SOVIET UNION

ROBERT C. COOK

Mr. Cook has been managing editor of the Journal of Heredity *since 1922. He was treasurer of the Ithaca Genetics Congress (1932), where Nicolai Vavilov participated as the last Soviet delegate to attend an International Genetics Congress, and he is secretary of the Committee on Displaced Geneticists of the Genetics Society of America. Mr. Cook's article is a condensation of a much longer and more detailed discussion that will appear in the July issue of the* Journal of Heredity.

AT THE Sixth International Congress of Genetics at Ithaca, in August 1932, the late Nicolai Vavilov included in his address to the Congress the following "flash" regarding a discovery made by a Ukrainian colleague:

> The remarkable discovery recently made by T. D. Lysenko of Odessa opens enormous new possibilities to plant breeders and plant geneticists of mastering individual variation. . . . The essence of these methods, which are specific for different plants and different variety groups, consists in the action upon the seeds of definite combinations of darkness (photoperiodism), temperature and humidity. This discovery enables us to utilize in our climate for breeding and genetic work tropical and sub-tropical varieties . . . This creates the possibility of widening the scope of breeding . . . to an unprecedented extent, allowing the crossing of varieties requiring entirely different periods of vegetation.

In the light of what has happened since, Lysenko's modest acorn of observation on plant physiology has grown amazingly over a period of seventeen years. It has now borne a very strange fruit: an allegedly new "Marxist-Michurinist" genetics. This is the latest thing in science, or the oldest, depending on how we look at it. Whatever status may be assigned Lysenkoism in the mature hindsight of history, it is unique in one respect: it is the only scientific discipline in existence today whose validity depends, not on experiment, but on certification as to purity and truth, in content and concept, by government fiat.

HISTORICAL BACKGROUND

At Ithaca, delegate Vavilov had extended to the geneticists of the world a cordial invitation to hold the next International Genetics Congress in Moscow in 1937, and preliminary plans for the conference in Moscow were well under way by 1935. About a year later it became clear that something was amiss with these plans when a dispatch to the *New York Times* on December 14, 1936, carried the news that the Genetics Congress had been postponed and that Professors Agol and Vavilov had been arrested. Vavilov's arrest was later denied, but the Congress had been postponed "at the request of a number of scientists who had expressed a wish to extend their preparations for the Congress."

The International Organizing Committee was convinced by May 1937 that the "postponement" constituted in effect a cancellation. It was stipulated by the Soviet government that if the Congress were to be held, no papers on human genetics could be presented. Under such circumstances a free scientific meeting was hardly possible, and arrangements were made to hold the Congress instead at Edinburgh, Scotland in 1939. Vavilov was elected its President, which position he did not fill, as no Russian delegates were present. Then in September 1939, while the Congress was in session, the nonaggression pact between Germany and Russia was signed and World War II began.

In the light of what has happened since, it may be significant that the "genetics furore" in the USSR toward the end of 1936 marked the beginning of the blood purge of generals and officials that continued through the following year. In 1938, a genetics mass meeting in Moscow had developed into a runaway Lysenko lovefeast. It is now clear that Vavilov was then standing on the brink of a precipice. Two years later he was suddenly relieved of his post as head of genetic research in the USSR. He died mysteriously in Siberia in 1942.

Vavilov, his prestige based on a superb job of analyzing the origin of cultivated plants, was chosen to organize genetic research by none other than Nicolai Lenin himself. Even this could not save him. Nor was he the only martyr. Among the distinguished geneticists who have disappeared are Agol, convicted in 1939 of "Menshevist idealism," and Levit, Director of the Human Genetics Institute. That other prominent "classical" geneticists were able to continue their work was due, Lysenko has made it very clear in his recent speeches, to forces he was not yet powerful enough to cope with. The battle of genetics in the USSR has not been fought so much in the laboratories as in the Congresses, dominated by laymen, and in the executive meetings of the Politburo.

Vavilov's successor as President of the All-Union Lenin Academy of Agricultural Sciences was Trofim Lysenko, one of the youngest members of that august Academy, and surely one of the least well educated. A practical plant breeder, with a minimal knowledge of experimental genetics, Lysenko still faced serious opposition. Science in the Soviet Union was at that time under control of the Academy of Sciences, which was then at the peak of its prestige. The mean age of the academicians, Eric Ashby tells us, averages about sixty-five years. These "conservative aristocrats of Soviet science" have always looked askance at Lysenko's weird theories. Ashby was convinced in 1945 that "Lysenko and his school were quietly tolerated" because of his popularity with the politicians, but that he had already passed his zenith. Ashby seems to have underestimated the extent to which Lysenko's ideas appealed to the top Communists, as well as to the farmers. His power was growing, and, sponsored by No. 3 Communist Malenkov, great things were in store for him.

The next step in the drive for "Marxist-Michurinism" was the publication in 1946 of Lysenko's *Heredity and its Variability*. Following closely on definite information that Vavilov was dead, this book inspired several critical articles in British and American scientific journals. The reply to these appeared on September 2, 1947, in a leading article in *Pravda* attacking certain "reactionary" biologists in and out of the Soviet Union. Academician Zhebrak, taken to task for a lack of loyalty to "Soviet biological ideas" and for consorting with such "open enemies of the Soviet people as Dobzhansky and Timoffief-Ressovsky," was replaced as President of the White Russian Academy of Science by N. I. Grashechenkov, a party propaganda expert.

Genetics Congresses seem to be nodal points in the history of Michurinist genetics. To the Seventh International Congress of Genetics in session in Stockholm, July 7–14, 1948, came word from the Soviet Academy of Sciences that "The Russian geneticists are too busy to leave their work;" hence, they would not be present at Stockholm.

The nature of this "work" was revealed three weeks later when the Lenin Academy of Agricultural Sciences met in Moscow on July 31–August 7. Like that historic meeting in 1939 when Vavilov suffered his first defeat, this gave the illusion of being a valid scientific discussion. Professor Zavadovsky, *doyen* of Russian animal geneticists, let the cat out of the bag when he outspokenly criticized the organization of the meeting. A sick, ailing old man, going from one sanatorium to another, Zavadovsky had heard of the meeting by accident.

> Insufficient opportunity was given those who are rightly, and especially for those who are wrongly considered among the Weissmann-Morganists, to prepare and to have the possibility to express themselves freely and fully. . . . We are making a big mistake [when] those who dare not agree with Lysenko are in a wholesale manner put by Lysenko's supporters in the odious category of "formal geneticists" . . . I consider that this narrow, limited, one-sided line of slandering not only the methods but the people who are not working under the approved plan is an inadmissible thing. . . . Who gives the right to include under the name of Darwinism that context which contradicts his teachings?

Others—notably Zhebrak, already in disgrace and under heavy fire—spoke eloquently in support of their views. The minority of Mendel-Morganists were frequently interrupted and ruthlessly heckled, mainly by Lysenko and Prezent. Those who disagreed in any respect appeared to be cast in the role of defendants in a court rather than of scientific workers engaged in a search for the truth. A reading of the transcript of this meeting makes it clear that a Thomas Committee on Un-American Activities and a Lysenko scientific gathering have many things in common. Lysenko had the advantage of Thomas, who might say to a witness: "The rights you have are the rights given you by this committee." Thomas' on-the-spot "rules" were subject to review by the courts. Before the chairman opened the first session of this "scientific" gathering in Moscow, an important

decision had already been made, and was reposing in Lysenko's pocket. What this was the assembled academicians would not know until they had committed themselves. Then an announcement would be "forced" from a coy Lysenko on the tenth and last session, to explode the bomb.

The discussions were over, and Lysenko again had the floor to sum up In sixty words—and in one of the most straightforward and understandable sentences Lysenko ever penned, the pay-off was announced:

> Before I pass on to my concluding remarks I feel it my duty to make the following statement:
>
> The question is asked in one of the notes handed to me, "What is the attitude of the Central Committee of the Communist Party to my report?" I answer that the Central Committee of the Party examined my report and approved it.

The answering of these questions so conclusively will surely have a place in history. Political authority, technically incompetent to evaluate the data of scientific research, had decided vital questions of fact on which a scientific discipline depended. Many basic biological tenets, proved and accepted elsewhere in the world, were false and "heretical" in Russia. Lysenko was making his own rules.

What happened to Zavadovsky we do not know: we hope he was allowed to continue his trip to the new sanatorium. A chasm had opened before more active and prominent scientists, Zhukovsky, Schmalhausen, and others, who still held places of prominence in the scientific hierarchy. Immediately after Lysenko finished speaking a strange parade of recanters began. Three academicians recanted on the spot. P. M. Zhukovsky, who had expressed an ardent belief in the importance of chromosomes and had doubted the validity of Lysenko's "experiments," now volunteered "to fight—and sometimes I am capable of fighting—for Michurin's teachings. I am working for the Committee for Stalin Prizes, and in the Council of Ministers, and therefore I think that I have a great moral duty—that is to be an honest Michurinist, to be an honest Soviet biologist." Said Academician S. L. Alikhanian: "As a Communist, I cannot and must not pit my personal views and understandings against the course of development in biological sciences." And Academician I. M. Poliakov: "The only thing for party and non-party workers to do is to say right out that Michurinist direction is the general road of development of biological science. . . . You must understand that this [foreign] rottenness has influenced some Soviet scientists, and it is necessary to eradicate it to the end. I will work for Lysenko."

renunciation of a lifework by some of the great names in the once fine and respected field of Soviet biology. Then on August 26 the All-Union Academy of Sciences of the USSR met, and the official heads began to roll. In an open letter to Stalin, the Presidium of the Academy promised "resolutely to correct the mistakes we have made, to reorganize the work of the section on biological sciences and its institutes, and to develop the biological sciences in a genuine Michurinist direction."

None of these changes in attitude were predicated on the question of the validity of the evidence. The basic data never even came up for discussion. Faith, as defined by a perplexed schoolboy as "belief in something you know is impossible," became the basis for biological science in the USSR. "Personal views" congruent with the relative data, must now bow before the official line of the party, however absurd that line might be. "It still moves," tradition tells us the recanting Galileo whispered as they released him from the stake. "The genes still segregate at random, in spite of Lysenko and Malenkov," we can hear these frightened people whisper.

Into official limbo went the Secretary of the Biological Sciences Section, L. A. Orbeli, an outstanding student of Pavlov, a Stalin Prize Laureate, and the leading physiologist of the Soviet Union; and Academician I. R. Schmalhausen, distinguished Director of the Palaeontological Institute. M. B. Dubinin, the eminent *Drosophila* geneticist not only was discharged as Director of the Institute of Cyto-Genetics—the institute itself was abolished amidst sarcastic screamings in *Pravda* against "such incompetents playing with fruit flies"!

Orbeli's successor, A. I. Oparin, promised that all experimenters in natural science would reconstruct their work in a fundamental fashion and cease "fawning and servility before foreign pseudo-science."

This resounding defeat to the "aristocracy of science in the Soviet Union" was administered to an Academy headed by Sergei Vavilov, the brother of geneticist Vavilov, liquidated in 1942. Said physicist Vavilov: "Our mistake has been primarily to fail to see that one of the conflicting trends, the Michurinist teaching, is genuinely materialistic and progressive. The organistic type of Mendelian trend is idealistic and reactionary."

On August 27, 1948, a *Pravda* editorial, quoted in the official *Soviet News*, put the capstone on this weird nonsense in explicit and all-inclusive terms:

important principle in any science—the party principle. They pegged themselves to a position of political indifference and "objectivity." The USSR Academy of Sciences forgot the instructions given by V. I. Lenin that "partisanship" is inherent to materialism, and that materialism, whatever phenomena are being considered, must stand openly and directly on the viewpoint of a definite public group.

Walpurgis Week was over. The "pseudo-scientific," "reactionary," "idealistic" biology of the capitalist world was dead.* *Pravda* and *Izvetzia* screamed approval. The country boy from the Ukraine who came to Moscow less than ten years before had made very good indeed!

BASIS OF LYSENKO'S CLAIMS

What, precisely, is this amazing new genetics of Lysenko? It is said by many leftish writers in the United States that Lysenko's "calmness of tone, scholarly approach, and patiently marshalled facts" demand recognition for his views. A feature article in the leftish *Masses and Midstream* in March 1949 says, "This controversy will affect biological science as profoundly as did Darwin's theory of natural selection, which was also highly controversial in its time."

Is the universal outcry against Lysenko's views by competent geneticists motivated by a desire to conceal the truth and to prevent the emergence of a powerful new organon? Is it possible, merely by paying $1.25 for a copy of Lysenko's *The Science of Biology Today* to "read the facts in the Lysenko controversy," as claimed in an advertisement of this book?

We do not have space here to consider the details of Lysenko's claims. They have been reviewed most carefully and fully by two competent geneticists.† Lysenko makes twelve didactic basic claims regarding genetics which are false, unprovable, or unproved. For example, he makes the flat assertion that "a hybrid F_1 plant is never later than the early parent." This is untrue, and is so proved by a mass of evidence. When Lysenko states in his latest book that "any character may be transmitted from one strain to another by means of grafting as well as by the sexual method" his only "evidence" is some puerile "experiments" that would shame a high-school boy. When Lysenko says "You need but change the type of metabolism in a living body to bring about a change of heredity," he adduces no clear evidence to support this claim, and he ignores the overwhelming weight of evidence against the proposition. This evidence stands solidly athwart his claim that "We can change heredity so as fully to meet the effect of the action of conditions of life." When Lysenko says "We must firmly remember that *science is the enemy of chance* [italics his] . . . "That all the so-called laws of Mendelism-Morganism are based entirely on the idea of chance . . . does not deserve to be called science," that "physics and chemistry have been rid of fortuities. That is why they have become exact sciences," he is obviously talking utter nonsense.

The words may be "calm in tone," they may have a "scholarly" ring to the layman, but they are the words of magic, of ex cathedra assertion, uttered by one who at best is a sincere and misguided bigot and at worst an utter charlatan. But they are not the words of science. To take Lysenko's statements one by one, and to evaluate them and extract the iota of truth from the few that are not completely false, would take pages. As Richard Goldschmidt puts it: "Such elementary facts as the chance assortment of chromosomes Lysenko considers to be mythical nonsense. . . . How is it possible he has never taken the trouble to see with his own eyes what thousands of students all over the world are unfailingly shown?" No, the apologies of the Fasts and the Spitzers, even the inspired obscurantism of J. B. S. Haldane (who ought to know better), cannot make a scientist out of Lysenko, or make anything very useful out of his mystical "Michurinism."

If this is not science, what, then, is it, and why has it captured the imagination of the masters

* A story attributed to the Associated Press states that a refugee to the U. S Zone of Germany reports that Schmalhausen, Dubinin, and Orbeli have been liquidated. This has no confirmation.

† For an understanding of this situation, three sources are indispensable. The most complete review of the background and history of the genetics controversy in the Soviet Union was published in 1946 by P. S. Hudson and H. R. Richens, of the School of Agriculture at Cambridge, England. Dr. Eric Ashby, formerly chairman of the Australian National Research Council and at present professor of botany at the University of Manchester, spent 1945 in Moscow on a scientific mission as Counselor at the Australian Legation. His very sympathetic and analytical firsthand account of Soviet Science (*Scientist in Russia*, Pelican Books, 1947) deserves a wide reading in this country. For a more impressionistic analysis of the concept of "heresy" in present-day Soviet Russia, the reader is referred to Humphrey Slater's *The Heretics* (Harcourt, Brace, 1947), which traces a vivid and frightening parallel between the Albigensian Crusade of 1209–11 and the fate of "liberal" Communists in the International Brigade during the Spanish Civil War. What has happened to biological science in the Soviet Union remains a complete enigma until we sense this strange Muscovite obsession with authority and with heresy, which runs like a red thread through this controversy.

of the Kremlin? Lysenko has capitalized on a quirk of character, not confined to the Russians, which delights in dramatized struggle, especially if it is based on magic and buttressed by authority. Richard Goldschmidt has told of the weird propaganda film *Salamandra,* which he saw in Moscow in 1929 and which canonized Kammerer and the inheritance of acquired characters, picturing poor Kammerer as the victim of a fantastic capitalist-clerical plot. This was long before anyone had ever heard of Lysenko. Michurin, the central figure in the strange iconology of Lysenko's genetics, combines these three obsessions. He proposed to "wrest security from nature," and what is essential to his needs is "true." He ardently believed in a childishly naive inheritance of acquired characters, and he was a "practical peasant" with a profound contempt for "theorists." Burbank, capitalism's Michurin, had, as American biologists know, a "green thumb," an inflated ego, a flair for weird statements, and a contempt for plodding experiments. Zavadovsky made it clear what Lysenko's school had done to Darwin.

This ardent belief in authority, in the magical properties of "necessity," and of the magic power of struggle, long antedates Lysenko and gives him his power. The appeal of the strong-arm approach to science is implicit in Stalin's dictum: "There is no fortress the Bolsheviks cannot take by storm."

Lysenko utilizes, in his "dialectical materialist" approach to heredity, an organon that the Western world had struggled to outgrow since the Middle Ages. Experiment and the use of mathematics are interdicted in this "home-grown" Soviet science. In their stead we have the dialectical antithesis, authority and heresy. The iconology of Lysenko's biological cathedral is decked with the usual Communist apostles—Marx, Lenin, Hegel—and the chapel in which Lysenko carries on his devotions features a strange pentathlon indeed: Darwin, Timiryazev, Michurin, Burbank, and Lysenko himself; the "heretics" and "heresies" are led by Weismann, Mendel, Morgan, "mathematics," "idealism," "vulgar materialism." To label any "heresy" "Weismannism" or "idealist" precludes the need to disprove it experimentally.

Control of nature by understanding natural processes is interdicted. Michurin taught that we must "wrest secrets from nature," just as the Arctic is being "wrested" (with considerable futility) back into the Temperate Zone. Authority (which is absolute), heresy (which is always doomed in the end to fail before authority), the necessary rightness of what must be—these concepts take the necessary. This is not science, it is the ancient magic and ancient authoritarianism the human race has struggled so long to escape.

There is increasing evidence that the virus of medieval obscurantism is extending far beyond genetics—even beyond biology—and in several directions As long ago as August 1946, the Central Committee of the Communist Party accused social scientists, in particular, and the majority of scientific workers, in general, of being "backward." "The scientific worker is a public worker: he cannot be apolitical. He must guide himself toward the policy of the Party, which reveals itself to be the living basis of Soviet Society." It is also reported that the Party Central Committee has set up a new Academy of Sciences, independent of the existing Academy, whose "backwardness" has been censured by Stalin personally. This is the more strange because in the summer of 1945 the Soviet government celebrated, with great international fanfare, the 220th anniversary of that same Academy, founded by Peter the Great.

In recent months, these criticisms of scientific workers have run the entire gamut from atomic physics to sociology. Since the August reorganization, four atomic scientists have been harshly criticized by Soviet newspapers for the statement that science cannot predict the behavior of atomic particles. On January 26, 1949, A. A. Maximov, of the Institute of Philosophy, attacked, over the Moscow radio, foreign physicists who were "responsible for idealistic interpretations in relativity and the quantum theory." Einstein, Bohr, and Heisenberg were guilty of "Kantian acrobatics," Joliet-Curie, Blackett, and Haldane were praised for their sound doctrine.

The Varga incident of several months ago is typical of these widespread attacks. Varga, a leading economist, was so undialectic as to have made the disturbing suggestion in 1948 that the impending "collapse" of the United States might not come off according to the Marxian schedule. This position branded him as a capitalist-reactionary; he was violently attacked and removed from his job as Director of the Institute of World Economics of the All-Union Academy of Sciences. He continued the argument, however, and was allowed to state his views with considerable freedom. Now an Associated Press dispatch of March 15, 1949, tells us that Varga has recanted. The U. S. is going to "fold," Varga now agrees. In fact, it pretty much already has! Varga's recantation came one day after the announcement that N. A. Vognesensky had been relieved of his duties as head of the Soviet Social Planning Commission.

three leading scientists have

cently been fired from key posts, among them, C. F. Gause, Russia's best-known authority on malaria. At the Academy meeting of August 26, the Minister of Health criticized the reactionary attitude of Davidenkov, Gurvitch, and Rubenstein. The geographers also had a going-over in August 1948. "Pseudo-scientific conceptions, bourgeois in origin" were noted, and their elimination promised. Several dispatches have told of plans to rewrite the encyclopedia along Marxist-dialectical lines!

The die has been cast in the USSR. For better or for worse, the Soviet people are stuck with Lysenko's genetics. If the numerous "straws" of revolt against "capitalist-bourgeois" science mean what they seem to mean, then a home-grown science as broad as the encyclopedia is very busily in the making. The fortress which the Bolsheviks are going to storm this time is not just the Arctic, or the chromosome: it is the entire field of human knowledge. What *is,* painfully established by generations of experiment, must bow before what *must be,* to fit the preconceptions of the weird Procrusteans in the Kremlin!

The recent shifts in the Soviet high command have been analyzed by one authority as being dictated by a newly strengthened antiforeign "axis" centering around Lysenko's sponsor Malenkov, Andreyev and Popov, in the Politburo, and Alexandrov, top party propagandist. This analysis of the situation is fully congruent with developments in Soviet science since last August. This would appear to be a golden opportunity for fervid, bigoted, and convincing "mediaeval obscurantists" of the Lysenko type! The encyclopedia they come up with will make fascinating reading.

In the long run this bizarre attempt to give a dialectical bum's rush to reality is bound to fail, as such attempts have always failed in the past. Even in the short run it is most unlikely to produce any dramatic results. We might be tempted to relax and let "nature" take its course beyond the Iron Curtain, for this trend is surely in our favor. But there is a problem facing this country, and all the hopefully called "free world," that is far broader than the ultimate fate of Lysenko and his queer necromancy.

We would be well advised to give serious consideration to the Soviet predicament. Seven hundred years ago Friar Roger Bacon set forth the first crude code of scientific behavior, dictated by the urgent necessity "to keep from fooling ourselves," to keep the questions we put to nature relevant and in a form allowing unambiguous answers. Science advances by understanding the forces of life and of the universe, not by attempting to dominate them by magic. We are not so far advanced ourselves in practicing this exigent art that we can safely be smug about our own somewhat precarious position. Magic, authority, and wishful thinking are still with us—are still a danger.

All too often in our own country, and in the Western world generally, we are still tempted to ascribe to science and the scientist the role of the priest or the magician. It is not too difficult to understand the Russian obsession with "what must be" when we see it so often nearer home. Scientism very easily becomes the dead hand of preconceived authority. Whether it be in this country or in the Soviet Union, the scientist who pretends to speak with the voice of authority concerning subjects on which he lacks competent information becomes an ally of this, our greatest enemy. For the scientist, our constitutionally guaranteed freedom of speech has a special and a critical meaning. It is—and it must always be—a freedom *to speak the truth in so far as we see it.* It is also the freedom to speak against antitruth (better, "antireality") wherever we find it. For any scientist to speak nonscience, and to use the prestige of his scientific position to expound nonscientific views, is to be guilty of the ultimate treason in the long battle to free the human mind.

The fight against revealed authority, against enthroned opinion, and against the use of power to force acceptance of *ad hoc* assumption as "revealed truth" is by no means ended. It goes on here, as it must go on "underground" in the Soviet Union if the minds of that fine courageous people are ever to be free. In an uncertain world, we are hardly likely to find perfection anywhere, and it is stupid and chauvinistic to claim perfection here. In the vivid phrase of DeWitt Wing, things are "less worse" some places than they are others, and we are very fortunate in many respects. But all who are the intellectual descendants of Galileo, of Servetus, of Vavilov, must never forget that the fight is not yet over. This fight has always been to a finish. The enemy is here as well as in Moscow. Our Spitzers, our Fasts, our Haldanes, and Blacketts, our scientists who pontificate without adequate knowledge, our trustees and executives who engage in mass witch hunts, all these give aid and comfort to that ultimate enemy of science, and of what we call freedom.

But for the grace of all who have fought for the freedom of the human mind, each of us stood in Moscow on August 7 last, and heard the words of doom: "The Central Committee of the Party examined my report and approved it."

AEROSOLS*

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CLOUDS in the sky and fogs covering the face of the earth are made up of small, invisible drops of water suspended in the air, whereas smoke from the chimneys of our homes and factories and dusts of all sorts consist of tiny solid particles floating in the air. Clouds, smoke, and all air-borne suspensions of minute solid or liquid particles are classed as *aerosols*—perhaps the least-well-known type of colloidal system, although they cause the blue of a summer sky and the red hues of sunset.

My own introduction to aerosols came through work with the National Defense Research Committee during the second world war. In order to test gas-mask filters, new methods of measuring extremely small quantities of aerosols were needed, and these we helped to develop in our Laboratory. Later, in order to guard against the passage of single particles, we worked out automatic methods of counting such colloidal particles. These new techniques may be applied to the study of such problems as the production of dust-free air in factories manufacturing biologicals, the spread of air-borne infections, and dust and smoke contamination in cities. We are continuing the development of methods of determining the number, size, and shape of aerosol particles, in order to gain a better knowledge of these systems.

How are aerosols formed? What factors influence the length of time the particles remain suspended in the air? How may they be removed from the air? What interesting and useful physical properties do aerosols show? How can we determine the infinitesimally small amounts of suspended material they contain, and the minute size and astronomical number of these particles? The answer to these and other questions will give us a better insight into the nature of these fascinating colloidal systems and some of their many practical applications. In some ways aerosols resemble other colloids, and some of the methods of studying them are adapted from the field of liquid-dispersed colloidal systems; others have been borrowed from the biologist and the bacteriologist. This debt may be repaid by the new techniques in aerosol study, which will prove useful in other branches of colloidal chemistry, in the life sciences, and in industry.

Like all colloids, aerosols may be formed either by the subdivision of matter in bulk or by the condensation of molecular dispersions. Solids may be ground mechanically into air-borne dust, some of which is fine enough to remain suspended as an aerosol. Similarly, the toxic smokes used in the first world war were dispersed by an explosive charge which pulverized the irritant material. Wind storms and volcanic explosions in nature may form aerosols on a global scale. In August 1883 the whole top of the volcano Krakatao, on the island of Sumatra in Sunda Strait, was blown off in a series of terrific explosions. A tremendous pillar of dust was thrown into the air. The coarser particles fell upon ships in the Indian Ocean, 1,100 miles away. A cloud of the finer particles was thrown to a height of 50–100,000 feet, where the prevailing westerly winds carried it more than twice around the world. It spread out in all directions and blanketed the continent of Europe, far above the rain- and snowstorms which clear the dust from the lower atmosphere. For two and a half years this fine dust in the upper atmosphere caused brilliant sunrises and sunsets, in which at times the disk of the sun appeared blue or green in color.

On a somewhat smaller scale, fine desert dust from Africa has been swept aloft by the winds, carried 2,000 miles over the Atlantic Ocean, and deposited in England and on the continent of Europe. Dust from the central desert of Australia has been carried 1,500 miles over the Pacific to fall in New Zealand. In this country, during the droughts in the thirties, high winds swept the dust of the Western states aloft to heights of 5,000–10,000 feet, carried it 1,200–1,800 miles in twenty-

* The sections of this paper describing the work of the author and his collaborators are based in part on work done for the Office of Scientific Research and Development under Contract OEMsr-282 with Northwestern University, and in part on work done for Camp Detrick, Maryland, under Contracts WA-18-064-CWS-137 and -160 with Northwestern University, and Contracts W-18-108-CM-31 and W-18-064-CM-218 with Indiana University.

four hours, and covered whole states with a dull haze. Dust from north Texas was collected in Buffalo, New York, and that from the Dakotas fell on the Eastern seaboard. The collected dust ranged from 1 to 50 microns (thousandths of a millimeter) in diameter, with the most common size usually less than 10 microns.

Many wind-pollinated plants also form widespread aerosols, and the sufferer from hay fever may sneeze from the pollen of ragweed blooming hundreds of miles away. Frequently, plant diseases also are broadcast by spores dispersed as aerosols. E. C. Stakman records the rapid spread of an epidemic of wheat stem rust by spores carried a thousand miles in forty-eight hours; and there is evidence for a yearly cyclic migration of this disease, from Texas to points as far north as Manitoba. The rust in the North does not survive the winter, but the infection is reintroduced each summer from the South. There the rust is burned out each summer, and returns from the North in the fall. Thus the air-borne migration of the wheat rust is comparable in scope to that of birds. A similar cyclic migration of this disease occurs in India, between the hot plains and the cool mountain slopes.

In addition to mechanical subdivision of solids or liquids, aerosols also may be made by spraying a solution of a solid substance in a volatile solvent, which evaporates to leave small dispersed particles. For example, when the crest of an ocean wave is whipped off by the wind, the fine drops quickly evaporate to leave tiny salt crystals, which are carried by the wind almost all over the world. The same thing occurs when a solution of DDT, a solid, is sprayed from a "bug bomb" to form an aerosol lethal to insects.

Solids colloidally suspended in liquids may be converted to aerosols by spraying them into the air as fine droplets from which the liquid evaporates to leave the individual solid particles. This is the familiar method by which the victim of a cold, sneezing violently, sets up an aerosol consisting partly of air-borne viruses and bacteria which spread the disease. Figure 1 is a photograph of a sneeze, taken by Marshall W. Jennison, of MIT, using Edgerton's flash illumination of 33 microseconds' duration to "stop" the motion of the particles. More than 40,000 particles have been counted in such a photograph, and an average of over 20,000 microorganisms has been demonstrated in the air by W. F. Wells as the result of one good sneeze.

Fogs and other liquid aerosols are formed when the vapor is chilled below the dew point. Condensation occurs most readily around fine particles of dust, ions, or other nuclei in the atmosphere; hence, fine smoke particles in the air of cities help condense the water vapor to "smog." Similarly, the ions formed by radioactive rays act as nuclei for condensation in the Wilson cloud chamber used in studies of these rays.

Most of the physical properties of aerosols depend upon the size, mass, and number of the particles they contain. Thus, aerosols containing large, dense particles soon settle out and disappear, and those with a high concentration of particles tend to coagulate rapidly. Just as the study of hybrid plants requires uniform purebred lines, so the quantitative study of the properties of aerosols requires the production of homogeneous samples. Striking and beautiful optical effects shown by homogeneous aerosols may disappear completely if some of the particles are of a slightly different size. The efficiency of the screening smokes used for the protection of cities during the second world war depended largely upon their particle size. So also does the efficiency of insecticidal aerosols, and the ability of medicinal aerosols to penetrate, and remain in, the bronchial tubes.

Most naturally-occurring aerosols are heterogeneous, containing particles of many sizes. Although it is hard to produce homogeneous solid aerosols of any but the smallest size, homogeneous liquid aerosols can be produced over a considerable range by a method developed in 1941 by Victor K. LaMer and David Sinclair, working for the National Defense Research Committee. They saturated a stream of air with the vapor of a high-boiling liquid, diluted and superheated it somewhat, then passed it through a tube, where it cooled slowly and thus condensed to form a homogeneous aerosol. They found that if the diluting air was passed through an electric spark or over a heated salt it appeared to pick up condensation nuclei that improved the uniformity of the aerosol. The size of the particles was controlled by varying the amount of vapor in the stream. As soon as the homogeneous aerosol was formed, it was diluted with a large volume of air at room temperature in order to stabilize it and prevent it from changing further. LaMer and Sinclair used these aerosols to determine the influence of particle size upon the completeness of the removal of the aerosol particles in gas-mask filters, upon their light-scattering properties, and upon their efficiency in cutting down visibility when they were used as screening smokes.

It is no easy matter to determine the number, size, and shape of aerosol particles. If they are

large enough, it is possible to allow them to settle on microscope slides, where they can be counted or measured with suitable magnification. Simple solid particles may be examined satisfactorily thus, although more complex filaments may be distorted. The particles in liquid aerosols tend to coalesce if they touch each other, and to spread on the slide glass, unless the surface is treated with a special film that is not wet by the liquid. Under the most favorable conditions, microscopic examination with the highest practical magnification does not allow measurements below about 0.4 micron in diameter.

In sampling coarse aerosols, frequently it is convenient to divide them into a number of different fractions of approximately uniform size. This may be done by means of a cascade impactor developed by K. R. May and improved by L. S. Sonkin. The device consists of a series of jets of decreasing size, with a sampling slide mounted close behind each. The air stream passes through the jets at an increasing rate, up to approximately sonic velocity, and deposits the largest particles on the first slide, and successively smaller particles on the subsequent ones. This partial size fractionation speeds up the tedious process of determining the size of particles under the microscope, and allows an approximate estimation of size distribution from chemical or colorimetric analysis of the amount of material collected on each slide. This instrument functions most efficiently with particles from a few tenths to 50 microns in diameter.

The principle of the impinger has been applied ingeniously by Owens in his jet dust counter, used in industrial surveys. The sample of aerosol is collected in a cylindrical tube lined with a sheet of moist blotting paper. In the bottom of the tube is a slit 0.1 millimeter wide, directly above a microscope slide, which is enclosed in a small chamber connected to a suction pump. A quick stroke of the pump draws out the aerosol, and the nearly adiabatic cooling condenses moisture on the particles, increasing their mass and hence their tendency to strike the slide and to stick. The collected particles are studied under the microscope.

FIG. 1. Aerosol produced by a sneeze. (Courtesy of Marshall W. Jennison.)

Very fine smokes will not settle appreciably upon a slide, and must be sampled by some other means, such as thermal or electrostatic precipitators (which will be discussed later). In the study of such aerosols, the electron microscope is playing an increasingly important role. When a magnesium

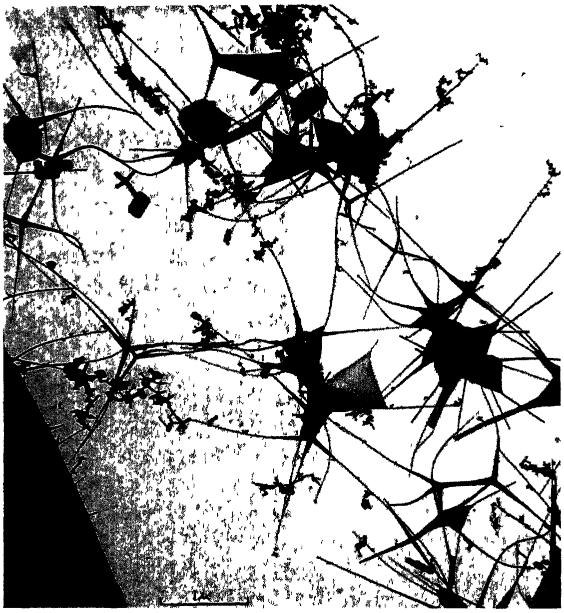


FIG. 2. Electron photomicrograph of zinc oxide smoke. (Courtesy of Farrand Optical Company.)

ribbon is burned in air, the resulting fine smoke of the oxide is produced by condensation of the molecules originally formed. Minute cubic crystals of magnesium oxide, collected from the smoke of a burning magnesium ribbon, are used to test the resolving power of an electron microscope. Another type of solid smoke is zinc oxide, formed when zinc is burned in air. This is illustrated in the electron photomicrograph obtained with a Farrand electrostatic electron microscope, and is reproduced in Figure 2. This shows how dust and soot particles in the home build up to "cobwebs" for which the housewife frequently blames the innocent spider.

The particles in liquid aerosols are much simpler than those in solid aerosols, since they are drawn into a spherical shape by surface tension and thus present the minimum surface for a given volume. Such liquid aerosols may persist far below the usual freezing point of the liquid. Clouds in the winter sky consist of supercooled liquid droplets, which freeze on contact with an airplane to form a layer of ice. Crystallization of the clouds to form snow may be induced by sudden local cooling, as Irving Langmuir, V. J. Schaefer, and B. Vonnegut, of the General Electric Research Laboratory, have shown. From an airplane they sprinkled pieces of solid carbon dioxide in a supercooled cloud to produce man-made snow. They achieved the same results with a supercooled water cloud in a cold chamber, which they "seeded" with very fine aerosols of silver iodide and other crystalline solids. They suggested that small amounts of these substances could be used to induce snowfalls over large or small areas, under the proper conditions.

Returning now to the methods of counting particles, R. Whytlaw-Gray and his collaborators in England have developed a special ultramicroscope cell in which they count the aerosol particles directly, without first precipitating them on a slide glass. The whole arrangement, very much like the standard slit ultramicroscope used in the examination of colloidal dispersions in liquids, is shown in Figure 3. The glass cell G is 0.1 millimeter deep and 2 millimeters long, with parallel sides which are made optically flat. The entire space between them is uniformly illuminated by an intense beam of light passing along the axis of the cell. The particles are observed through a microscope objective with a focal depth exceeding that of the cell. Thus the depth of the sample of aerosol is limited by the cell, and the area is limited by suitable stops. The particles appear as bright spots on a nearly black background. By smearing the parallel plates with a thin layer of glycerol, paraffin oil, or some other involatile liquid, particles striking the glass are made invisible, and those in the cell are brightened by condensation. The aerosol enters the cell through a short inlet tube I and leaves through an exit tube connected to an aspirator through a stopcock J, which is rotated by means of a motor Thus successive samples of the aerosol stream are drawn into the cell and stopped momentarily under the microscope. The size of the stop diaphragm is chosen to give an average of two or three particles

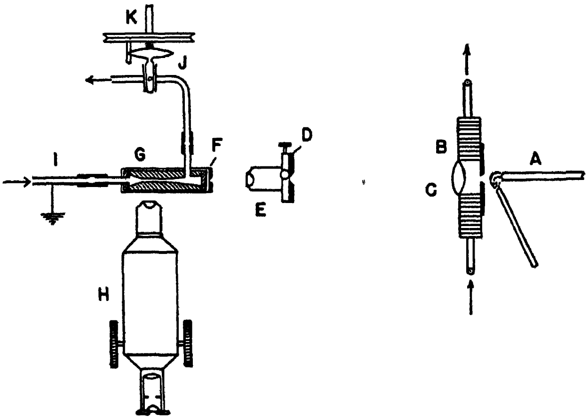


FIG. 3. Ultramicroscope counting apparatus of Whytlaw-Gray and his collaborators. (From *Smoke*, by R. Whytlaw-Gray and H. S. Patterson, Edward Arnold & Company, London, 1932.) *A*, arc; *B*, water-cooled slit; *C*, collecting lens; *D*, slit; *E*, illuminating objective; *F*, slit; *G*, ultramicroscope cell; *H*, viewing microscope; *I*, inlet smoke tube; *J*, rotating stopcock; *K*, wheel carrying arm to rotate stopcock.

in the field of view. In one minute, 60 fields can be counted, giving an adequate statistical average. Since the background light is appreciable with this cell, the method is limited to particles of about 0 1 micron in diameter.

For counting smaller particles, H. L. Green has developed an ingenious method of condensing water vapor upon them during an adiabatic expansion. This is a modification of the familiar cloud chamber developed by C. T. R. Wilson for photographing the tracks of radioactive rays. The expansion chamber is connected to an ultramicroscope cell, and photomicrographs are taken at right angles to the illuminating beam. The apparatus is operated to give 50 photographs in 100 seconds. The particles on the photographs are counted with a low-powered microscope and a calibrated squared graticule. Knowing the depth of the original light beam and the magnification in the process, the particulate concentration is found. Green's method is applicable to particles wet by water, and could be extended to others by using different liquids for condensation. In an inhomogeneous aerosol, the smallest particles may be missed because condensation is stopped by the latent heat evolved.

How long do aerosol particles remain in suspension, and how do they change as time goes on? We know that colloidal systems dispersed in liquids may remain stable indefinitely. In fact, suspensions of silica are found included in quartz rocks, where they have remained for geologic ages. Aerosols, however, are ephemeral, changing and disappearing more or less rapidly after they are formed. Whenever two particles collide, they apparently stick together to form a larger aggregate, and all the particles tend to settle out of suspension under the influence of gravity. Large particles settle much more rapidly than smaller ones of the same shape. In fact, the rate of settling of spherical particles is nearly proportional to the *square* of the diameter. Thus in one second a water drop 100 microns in diameter would fall 31 centimeters through still air at 20° C, a 10-micron drop would fall 3 millimeters, and a 1-micron drop, only 36 microns. A fog of 100-micron drops settles out nearly nine thousand times as fast as a fog of 1-micron drops. In an inhomogeneous aerosol, the largest particles settle first, leaving the smallest in suspension. The distance which the wind can carry aerosol particles such as dust or bacteria depends upon their size. Thus, if a spherical particle of the density of water and 10 microns in diameter falls from a height of 10 meters, it will be carried 9 miles by a wind of 30 miles per hour, before it touches the ground. Under the same conditions, a 1-micron particle would be carried 900 miles.

One method of measuring the size of aerosol particles 1 micron or more in diameter is to study their rate of settling, with an ultramicroscope. Great care is necessary to avoid thermal convection currents, and the density of the particle must be known, or determined by a separate experiment. H. S. Patterson and R. Whytlaw-Gray have used R. A. Millikan's oil-drop technique. They studied the rate of rise of individual charged particles in an electric field and their rate of fall under gravity. Knowing the charge of the electron, they have used Millikan's equations to calculate both the radius of the particle (assumed practically spherical) and its density. For a liquid (oil) smoke they found the mean density of all the particles was the same as that of the original oil.

The results for solid smokes are strikingly different, as typified by Table 1, which shows the data for magnesium oxide. The normal density of the solid is 3.6. One of the small particles has nearly this density Most of the others, however, have a much lower density, averaging 0.35, or about one tenth of the normal That is because the larger particles are loose aggregates of the denser primary particles first formed from the vapor phase. The study of individual particles is tedious and slow, and Whytlaw-Gray and Patterson have photographed an ultramicroscope cell to obtain traces of the paths of many particles, the lengths of which are proportional to the squares of the diameters.

Aerosol particles less than a few microns in diameter settle very slowly, and also show increasingly vigorous Brownian movement. Instead of falling in a straight line, they dance about under the buffeting of the gas molecules, with which they become more nearly comparable in mass. At any temperature, the mean displacement in one second, owing to the Brownian movement, increases inversely with the square root of the diameter of the particle. It equals the settling velocity for particles of unit density and about 0.5 micron in diameter.

A. Winkel and H. Witzmann have made a quantitative study of the Brownian movement to de-

TABLE 1

RADIUS (r) AND DENSITY (d) OF MAGNESIUM OXIDE PARTICLES

r(micron)	0.326	0.493	0.617	0 698	0.835	0.835	0.859	0.936	0.949
d(g/ml)	3.48	0.52	0.42	0.39	0.28	0.24	0.40	0.23	0.30

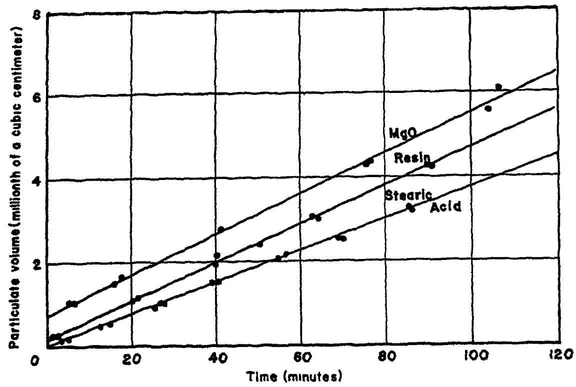


Fig. 4. Coagulation rates of typical smokes. (From *Smoke*, by R. Whytlaw-Gray and H. S. Patterson, Edward Arnold & Company, London, 1932)

termine the size of these small particles. They used an ultramicroscope with an intense light source interrupted by a rotating sector disk to illuminate the particles with 20 flashes of light per second. A microscope camera, with film moving at a uniform rate, thus took 100 ultramicrographs in 5 seconds, from which the movements of a number of particles were determined. The radii of the particles were calculated from the laws of the Brownian movement.

Another very ingenious method of measuring aerosol particle size was introduced by P. V. Wells and R. H. Gerke. They passed the aerosol slowly between the plates of an electrical condenser and took ultramicrographs of the tracks made by the charged particles, as the field was reversed by means of a rotating commutator at intervals of 0.25–0.5 second. They showed that the diameter of each particle could be calculated from the number of electronic charges upon it, the field strength, the frequency with which it is reversed, and the amplitude of the oscillation. All these factors can be measured conveniently. Wells and Gerke found that a charge was carried by about one third of the aerosol particles formed from oil dispersed in a cubic meter box (apparently by means of a blasting cap). They state that nearly all these particles possess a unit electrical charge, and calculate their diameters on this basis, in the range 0.05–0.7 micron. The method is valid if the particles are representative in size and singly charged. If, as is usually the case, the original aerosol contains few charged particles, some method of imparting a known uniform charge to most of them would be necessary before using this method.

In studying the aging of aerosols, the particles in representative samples may be counted from time to time to determine the number per cubic centimeter, or the *particulate concentration*, the reciprocal of which represents the average space per particle, or the *particulate volume*. As coagulation takes place, the particulate concentration decreases, and the particulate volume is found to increase linearly with the time. Thus the coagulation of a homogeneous aerosol follows the same equation as a second-order chemical reaction. Figure 4 shows plots of some results of Whytlaw-Gray and Patterson for the coagulation of smokes of magnesium oxide, resin, and stearic acid. All three show a linear increase of particulate volume with time, and nearly the same slopes, indicating that the rate of coagulation depends more upon the number than the nature of the particles.

An interesting application of the theory of collisions between gases and aerosol particles has been made recently by Theodore W. Puck to explain the mode of bactericidal action of aerosols of the glycols and other chemical agents. Many had argued that it was due to collision between aerosol particles and the air-borne bacteria. Puck showed, however, that such action would be very slow, and that the observed bactericidal rate must be due to the much more rapid diffusion of the molecules of the vapor to the bacteria. The difference is enormous. Under Puck's experimental conditions, aerosol particles of 2-micron diameter would have required twenty-three hours to collide with and kill 90 percent of the bacteria of 0.6-micron diameter, whereas diffusion of the molecular vapor requires but four seconds for lethal contact.

When an aerosol is fine enough to sediment slowly, its stability depends chiefly upon its particulate concentration. This stability may be measured by the *half life*, that is, the time required for the number of particles to decrease to half its original value. The half life is inversely proportional to the original concentration. If the concentration exceeds about 10 million particles per cubic centimeter, coagulation is rapid and the aerosol is not stable. If the aerosol contains somewhat less than a million particles per cubic centimeter, it changes more slowly and is stable enough for convenient study. Thus ammonium chloride aerosols containing 10 million particles per cubic centimeter have a half life of only two minutes, whereas those containing 100,000 particles per cubic centimeter have a half life of four hours. Aerosols used for laboratory study may contain originally 100–1,000 micrograms (one milligram) of dispersed material per liter. Thus the weight of the aerosol particles is usually less than that of the air in which they are dispersed.

Frequently, the removal of aerosols is a matter

of great practical importance, whether they are the toxic smokes of chemical warfare, dust or fumes from industrial processes, or bacteria. Various methods may be used, depending upon the quantity and type of suspended material and the amount of air which must be purified. Probably the simplest method is filtration.

When an aerosol is passed through a filter of absorbent cotton, paper, or any other material. the efficiency of its removal depends upon a number of factors. In any practical filter, the openings generally are far larger than the aerosol particles, so that the action is unlike that of a sieve. During the second world war, Irving Langmuir developed a theory of mechanical filtration, in which he considered the filter as a series of evenly spaced layers of fine fibers through which the aerosol passes. In respirator filters, the air moves slowly enough so that the flow is streamlined. The larger particles cannot follow the small radius of curvature of the flow lines; hence they collide with the fibers and are removed. The very small particles diffuse across the flow lines because of their Brownian movement and touch the fibers. Liquid aerosol particles of about 0.3-micron diameter are of an intermediate size, which is most penetrating. Extremely fine fibers, such as those of asbestos, supported in a porous paper which prevents them from matting, form a very efficient filter, found in many of the best gas-mask canisters during the second world war.

When a series of identical filter sheets is used, each should remove the same fraction of a homogeneous aerosol. Thus if one sheet allowed one tenth of the original aerosol to pass, a second sheet would remove all but one tenth of the remaining aerosol, or 1 percent of the original material. In general, a plot of the logarithm of the penetration against the number of sheets should give a straight line. This relationship is called the Filter law. Since a small pressure drop is a great advantage, particularly in a respirator filter, the efficiency of a filter is judged by the smallness of the product of the penetration times the pressure drop.

In removing dust from blast furnace, coal, or producer gas, filters may be used, although they become less economical the larger the volume of air and the mass of aerosol that must be handled. For large installations handling hot gases, the method of filtration is impractical, and the Cottrell electrical precipitator is used. Here a high d.-c. potential of 20,000–30,000 volts is applied to give a brush discharge between a series of sharp points and a flat plate. The discharge ionizes the air, and the ions charge the aerosol particles, which are attracted to the plate and deposited very rapidly. Some years ago, in a cement works at Riverside, California, a gas-fired rotary kiln, 7 feet in diameter and 100 feet long, spewed out hot gases at the rate of 50,000 cubic feet per minute. These gases carried along 4 or 5 tons of dust every twenty-four hours, and made the countryside desolate. A Cottrell unit, made of steel, was installed on the top of the 80-foot stacks. The electric current precipitated the aerosol from the hot gas, thus eliminating the smoke damage and at the same time collecting valuable potash. A Cottrell installation at the Washoe reduction works of the Anaconda Copper Mining Company uses more than 100 miles of chains for the points to give the electrical brush discharge. Tons of material, hazardous or noxious as smoke, can be precipitated and recovered in this way, frequently forming a valuable by-product.

A different method of precipitating aerosols is based upon a discovery by Aitken. He noticed that a heated wire, in a flask of smoke, is always surrounded by a clear space, from which the aerosol particles are driven by heat. Thus the establishment of a thermal gradient in an aerosol makes the particles move in the direction of the heat flow This causes the deposition of dust on the relatively cool wall behind a radiator and the gray streaks frequently visible on the ceiling of a room directly below the roof of an unheated attic. The strips of lath supporting the plaster make a difference in the heat leakage through the ceiling, and the fine dust is precipitated on the cooler strips. When a room is heated by true radiant heat from a fireplace, not a "radiator," the walls are warmer than the air next to them, so that the dust is not precipitated in this way.

The principle of thermal precipitation was ap-

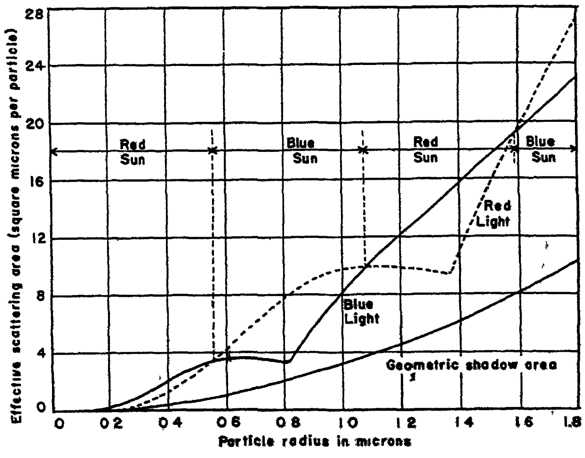


FIG. 5. Effective scattering area per particle of water fog, according to Mie's theory.

plied by Robert Lomax to sampling aerosols by passing them through a narrow space between two parallel plates, the upper heated to 110°C while the lower is kept cool. The aerosol is deposited quantitatively upon a glass slide covering the lower plate and may be examined microscopically. The air stream must be passed through the apparatus rather slowly, so that the principle has not been applied successfully on a commercial scale.

One of the most interesting and beautiful properties of aerosols is the way in which they scatter light. The beam from an automobile headlight is clearly outlined on a foggy night. Similarly, any other colloidal system, in which the refractive index of the dispersed particles differs from that of the medium, scatters some of the light which passes through it. This phenomenon, first noted by Michael Faraday in 1857, was studied later by John Tyndall, and is called the Tyndall effect. Later, Lord Rayleigh calculated the scattering due to very small particles, of a diameter less than one tenth the wave length of the light. He found that such particles should scatter light of short wave length much more than that of longer wave length. Thus he explained the red hue of the sun's disk near the horizon, since red light is little affected, whereas blue light is scattered about eight times as much by fine dust, clouds, and even molecules in the atmosphere. The blue color of the sky he attributed to multiple scattering of the blue part of the solar spectrum by these particles, and his explanation solved a long-standing riddle of nature.

The calculation of the scattering of light by larger spherical particles, comparable in size with the wave length of the light, is a much more difficult problem. In 1908, however, Gustav Mie developed the necessary equations from Clerk Maxwell's electromagnetic theory of light. He showed that the total scattering increases with the size of the particle. The increase, however, is not uniform, and depends upon the wave length of the light and the refractive index of the particle. In Figure 5 we have shown the effective scattering area, in square microns per water particle, for red and blue light. Up to a radius of about a half micron, these particles scatter blue light more than red, as predicted by Rayleigh. Between a half and slightly over 1 micron, the effect is reversed, and red light is scattered more than blue. Viewed through an aerosol of this particle size, the sun's disk would appear blue or green, which probably explains the weird sunsets seen after the explosion of Krakatao. Figure 5 shows another region of particle size over which the color of the sun's disk would appear red, and finally a second blue region.

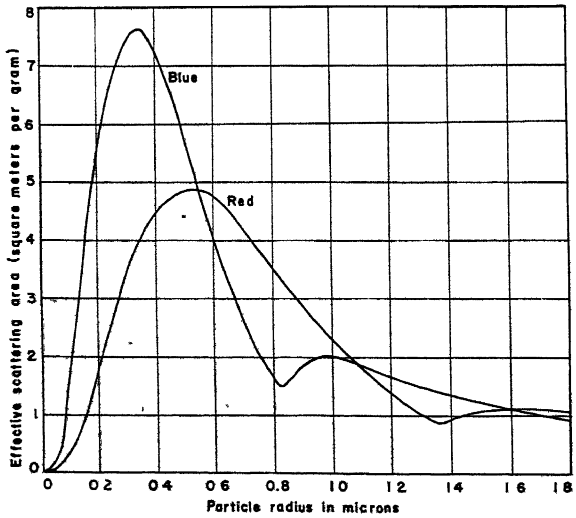


FIG. 6. Effective scattering area per gram of water fog, according to Mie's theory.

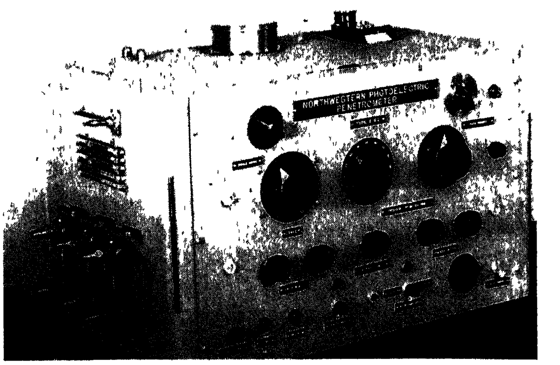


FIG 7. Photoelectric smoke penetrometer.

These curves show that visual observation of the sun's disk, or of any white light, through an aerosol, is a qualitative measure of the particle size, over a considerable range. V. K. LaMer and S. Hochberg, in their study of aerosols for the NDRC, developed these observations into a quantitative method. They passed red and green lights of definite wave lengths through a long cell containing the aerosol, and measured the reduction of intensity photoelectrically. They used the relative absorptions of the two wave lengths to calculate the particle size from the Mie theory, and found that this method is applicable even to aerosols that are not homogeneous.

An unexpected prediction of Mie's theory is the total scattering area for large particles. At first glance, this would seem to be simply the cross-sectional area, which would be true of completely absorbing particles. Either colorless or completely

reflecting spheres, however, show an effective scattering area *twice* that of the geometric shadow. The light which falls upon the particle and is scattered from its original direction *interferes* with the light which grazes the surface of the particle, and diverts it outward through a small angle Thus the diffraction pattern is analogous to that from a beam of light passing through a small pinhole.

A very interesting relationship is found when we consider the effective scattering area per gram of material, instead of the scattering area per particle. The large number of very fine particles per gram is nullified by their low scattering power. As the size increases, the decreased number is more than compensated by the increased scattering power, until a maximum scattering is reached which is very high and sharp for blue light, and less pronounced for red, as shown in Figure 6 Such considerations show how greatly the particle size influences the obscuring power of water fogs or the screening smokes used so widely in the second world war. They also show why a given aerosol might be a much less effective screen against red or infrared light than against blue light, and would be almost completely ineffective against the much longer radar waves.

Rayleigh predicted that the scattering of light by very small particles is symmetrical, with equal scattering in the forward and reverse directions. He also predicted that the light scattered at right angles to the beam would be completely polarized. and this is true of the sun's light scattered in the earth's atmosphere. As the size of the aerosol particles increases, however, more and more light is scattered in the forward direction, and a new phenomenon appears. A homogeneous aerosol breaks up white light into series of spectra, like those from a diffraction grating. These spectra may appear sharp and brilliant, particularly if they are viewed through a pair of Polaroid sunglasses. This effect was cited by B. Ray in 1921 to explain the "axial colors" he observed in homogeneous sulfur sols.

Observation of the spectra, and particularly of the number and position of the red bands, has been applied recently by LaMer and his collaborators to a problem in chemical kinetics—a study of the growth of sulfur sols. If a parallel beam of white light is passed through a homogeneous aerosol, the number of spectral orders is a measure of average particle size. The orders are determined most easily by viewing the aerosol through Polaroid glasses and counting the number of reds between 0 and 180 degrees of the light beam. For aerosols of all refractive indices, the average radius, in tenths of a micron, is equal to the number of orders, up to about five at least. Interpolations can be made between integral orders. The angular positions of the reds can be measured to one degree, and compared with the positions calculated from the Mie theory. These methods are applicable only to the homogeneous aerosols. In fact, the observation of brilliant spectra is a good criterion of a homogeneous aerosol.

A number of astrophysicists, including H. C. van de Hulst in Holland, have applied the Mie theory of scattering to interpret the changes in stellar spectra due to interstellar dust clouds, and have attempted to determine the chemical nature and size of these particles, which are thought to comprise half the matter in the universe.

The light-scattering properties of aerosols have been utilized recently in developing rapid and extremely sensitive methods of measuring the mass concentration of dilute aerosols (one microgram per liter or less). It is possible to measure either the decrease in the light transmitted through the aerosol, or the amount of light which is scattered.

In 1937, A. S. G Hill developed an apparatus for testing commercial smoke respirators by means of the difference in light transmission of the test smoke before and after filtration. A strong beam of light passed through a 50-centimeter smoke cell to fall on a photocell, the current from which was amplified electronically. Hill's carbon test smoke, containing 25 micrograms per liter, with an average particle diameter of 0.16 micron, reduced the light on the photocell by only 9 percent, so that the light intensity had to be regulated to two parts in one hundred thousand to achieve a sensi-

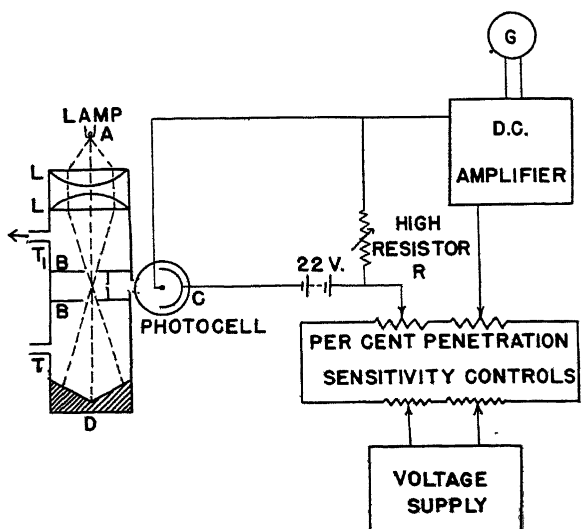


Fig. 8. Schematic diagram of photoelectric smoke penetrometer.

tivity of 5 one thousandths of a microgram per liter, or 2 one hundredths of a percent of his test smoke.

Only a small fraction of the light scattered from the primary beam by the smoke can be collected in an optical system. It can be measured directly, however, and not as a small difference between two large quantities; hence, the light intensity requires no elaborate regulation. A number of instruments have been built for visual comparison of light scattering from aerosols, but these cannot compare in convenience and sensitivity with a photoelectric instrument such as that developed by C. T. O'Konski, H. B. Pickard, and F. T. Gucker, Jr., in our Laboratory at Northwestern University to test gas-mask filters during the second world war. Figure 7 shows the instrument mounted in a 12″ × 20″ × 12″ cabinet. Figure 8 shows the fundamental principles of operation of the photoelectric smoke penetrometer—so called because it was designed to measure the filter penetration of test smokes (usually liquid aerosols). The brass smoke cell is covered on the inside with optically black paper, coated with soot to reduce reflection of light. Smoke enters through T and leaves through T_1. The light from a 50-candle power automobile headlight A is focused in the center of the cell by the two aspheric condensing lenses L, L, and finally absorbed by the V-shaped light trap D. The beam is outlined by the dashed lines. Baffles, B, B, cut off stray light. The light scattered at right angles passes through two vertical slits, to fall on a vacuum photocell C, in series with a 22-volt battery and a high resistor R. The scattered light reaching the photocell is outlined by the dotted lines.

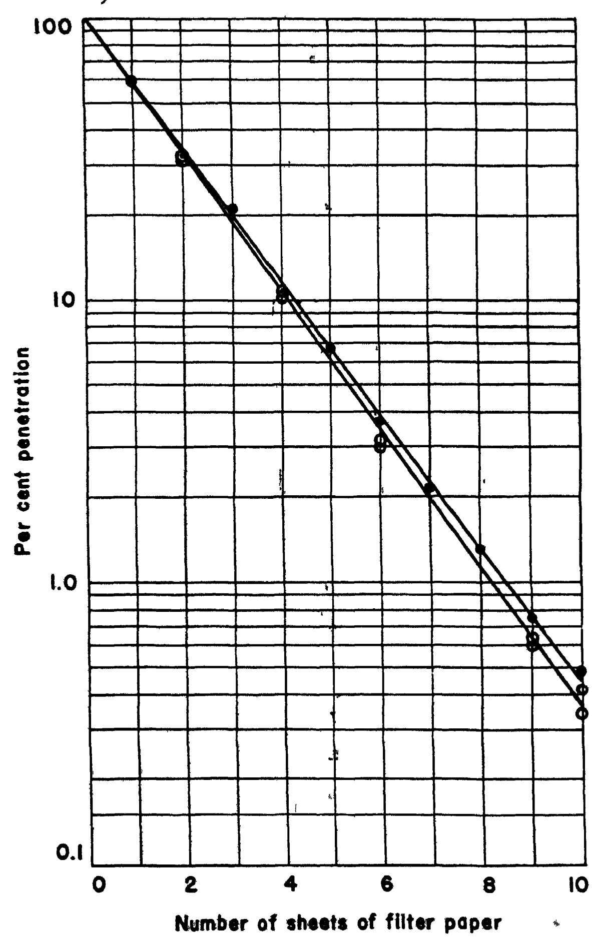


FIG. 9. Validity check of photoelectric penetrometer. (From the *Journal of the American Chemical Society*, 69, 429, 1947.)

The photocurrent, which is proportional to the light intensity, flows through R and causes a drop in potential, E_R, which is balanced by the potentiometer to obtain a null reading on the galvanometer G in the plate circuit of a single-stage d.-c. amplifier.

The smoke cell is designed to reduce background stray light to a low figure, and the stray-light current is compensated electrically when the cell is filled with carefully filtered air. Next the cell is filled with the raw test smoke, the percent-penetration potentiometer is set on 100, and the sensitivity controls are adjusted to balance the amplifier when the resistance R is 10 megohms. Finally, filtered smoke is passed through the cell and the potentiometer is balanced to read the penetration directly in percentage. For low smoke concentrations R can be increased in decimal steps to 10,000 megohms, so as to keep the photocurrent IR drop within the range of the potentiometer. The instrument is direct-reading at all times, is sensitive to one billionth of a gram per liter of a DOP (dioctyl phthalate) test smoke of 0.3-micron diameter, and can be used to test filters with a sensitivity of one thousandth of a percent.

This photoelectric penetrometer was found to agree with other methods of measuring smoke penetration, in the range where these methods were applicable. At lower concentrations, the self-consistency of a series of measurements with the penetrometer may be checked by means of the Filter law. Figure 9 shows two series of results obtained with one of our instruments. The linearity of the plot of percent penetration, on a logarithmic scale, against number of sheets of filter paper in a composite pad, shows the self-consistency of the measurements over a two hundred and fifty-fold change in concentration. This also indicates a homogeneous test smoke, since the large particles in an inhomogeneous aerosol would be removed chiefly in the first few sheets, and would give a curve concave upwards.

In the summer of 1944 we undertook to develop for the Army Chemical Warfare Service (now Chemical Corps) a supersensitive penetrometer to test the best service canister filters by an automatic count of the individual effluent smoke particles. By June 1945, O'Konski, Pickard, and Gucker had produced a photoelectronic apparatus able to count individual DOP particles of 0 6-micron diameter or larger, at rates from 1 to 1,000 per minute. This apparatus has many other uses in colloidal chemistry, bacteriology, and industry.

The photoelectronic counter is shown diagrammatically in Figure 10. Light from a 50-candle power automobile headlight bulb *A* is focused inside the black-walled cell by a pair of aspheric condensing lenses *L, L,* like those of the penetrometer. A black disk *B* mounted on the glass plate which forms the end of the cell cuts out the central part of the converging cone of light, outlined by dashed lines, so that the smoke particles at *D* are under intense dark-field illumination. The aerosol passes upward at one liter per minute through the central tube T_1, while filtered air flows through T_2 at the same linear rate, to form a protecting sheath which keeps the smoke from circulating before it leaves the cell through tube T_3. Each particle which passes through the focus scatters a pulse of light, chiefly in the forward direction. That portion which is intercepted by the lens *E* is focused upon the photosensitive cell *C*, as indicated by the dotted lines. The stray light, which contributes to the random background noise of the cell, is reduced by means of the blackened baffle *F*, the black disk *B* which prevents reflection from the glass, and the blackened baffle *G*. The electrical pulse from the tube *C* is increased about two hundred thousand times in a pulse amplifier, then fed to a thyratron "trigger" circuit, which actuates a mechanical recorder.

Figure 11 shows the smoke cell mounted on the amplifier chassis. Figure 12 shows the second unit, containing the trigger circuit, timer, and the regulated power supply.

In making a count, recorder 1 or 2 is selected, and the START button is depressed. This starts the count-recorder and timer simultaneously. When the counter has made a complete revolution, it closes a circuit which stops both timer and counter. Thus each experiment gives the time for 100 counts. Depressing the START button automatically resets the timer before it starts the next series of counts. A pair of experiments, using the two counters to check each other, is adequate for a filter test. Since DOP smokes containing as many as 10 billion particles per liter may be used, and

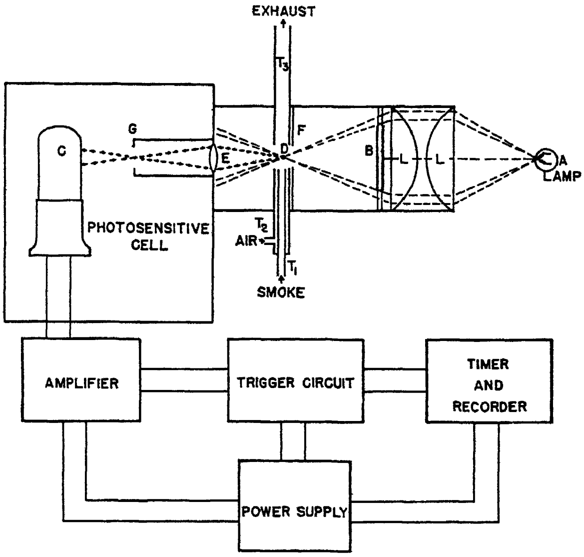


FIG. 10 Schematic diagram of photoelectronic particle counter. (From *Chemical Reviews,* **44**, 373, 1949, Williams & Wilkins Co., Baltimore, Md.)

as little as 1 particle per liter can be detected, the apparatus is sensitive to one hundred millionth percent penetration.

When longer counts are to be made, the switch is turned from AUTOMATIC to MANUAL, and the auxiliary counter in the upper right-hand corner registers the number of hundreds of counts. The apparatus may be stopped at any even hundred counts by turning back to AUTOMATIC. When the experiment takes less than 100 counts, the counter and timer may be read and the switch in the lower right-hand corner turned from COUNT to RESET. This connects a small relaxation oscillator to the thyratron circuit, and quickly resets the counter to zero.

Tests of the accuracy of the counter are even more difficult than the tests of the photoelectric penetrometer. The most reliable evaluation of the counter has been made by Ronald M. Ferry, Leo E. Farr, Jr., and Mary G. Hartman, who are investigating bacteriological uses of the counter, particularly in connection with the study of airborne infections. Recently they completed a series of experiments in which they compared the counts registered by our instrument with the numbers of aerosol particles determined by means of a carefully constructed two-stage impinger. As test material they used two bacteriological aerosols. The first was made by spraying aqueous suspensions of *Bacillus globigii* (BG), which are ellipsoidal, about 0.8 × 1 micron in size. The second was an aerosol of *Serratia marcescens* (SM). These are somewhat smaller, sometimes nearly spherical—

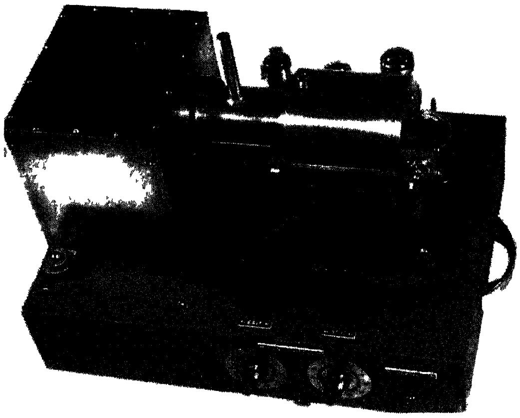


FIG. 11. Smoke cell of particle counter.

and frequently ellipsoidal—in form, and about 0.5 x 1 micron in size. These investigators found excellent agreement between the results obtained with the two methods. The advantages of the automatic counter are obvious compared to the slow microscope count of the slides in the impinger or with a bacteriological count of viable spores, collected on a cotton filter plug, washed onto an agar plate, incubated overnight, and determined by a count of the resulting colonies. A measurement which requires hours for incubation alone when carried out bacteriologically can be made in a few minutes with the photoelectronic counter.

The original photoelectronic counter was sensitive to individual particles weighing less than one billionth of a milligram. It may have considerable application in bacteriology and in the many industrial processes, like the preparation of penicillin and streptomycin, where large quantities of sterile air have to be used. Bacterial spores and the dust particles in a trace of unfiltered air from the room show up almost immediately in the counter, whereas the bacterial contamination which may be introduced in this way would not ordinarily appear until hours later in bacteriological plate counts. Although the light scattering goes down very rapidly with decreasing particle size, we are attempting to improve the optical system of the counter and thus adapt it to still smaller particles. We are also attempting to adapt the counter to the rapid determination of particle size in aerosols by measuring the size of the electrical pulse given by each particle, which can be correlated with the amount of light scattered by the particle, and hence with its size.

In addition to the optical properties we have just considered, aerosols also show a number of interesting and important electrostatic properties. Frictional electricity has been studied for a great many years. If a rod of hard rubber, glass, or some other insulating material is rubbed with fur or flannel, it will become electrified. The two substances rubbed together will acquire opposite charges, since electrons are transferred from one to the other Frictional electricity also may be generated when aerosol particles pass through a small nozzle or are blown against a surface. A number of people have observed that wires exposed to a violent snowstorm will be charged sufficiently to emit a continuous stream of sparks or a distinct corona discharge with a current of a number of milliamperes. Very high voltages may be built up in this way during dust storms, and the lightning flashes observed during the eruption of volcanic ashes probably are due to the same cause. Airplanes flying through dust clouds, rain, or snow often become so highly electrified as to give off a blue corona discharge—St. Elmo's fire—which interferes with radio reception. Over a hundred years ago, W. G. Armstrong constructed an apparatus consisting of a boiler from which steam was forced out through a jet, thus forming an electrostatic generator. More recently a number of people have studied the charging of finely powdered minerals and other dielectric materials which were blown through metal tubes. R. E. Vollrath has built an electrostatic generator that can produce a current of 80 microamperes at a potential of over a quarter of a million volts. A jet of air carrying finely divided diatomaceous earth is blown rapidly through copper tubes mounted in an insulated metal sphere. The fine dust is then returned to the original container, which is grounded and provided with a canvas top to allow the air to escape. W. C. Hall has made a study of the possibility of discharging the static electricity from airplanes by means of diatomaceous earth blown out from the plane through brass tubes filled with metal turnings.

Whatever the exact process by which the aerosol

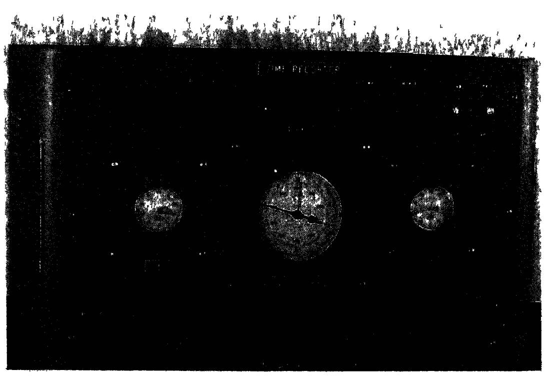


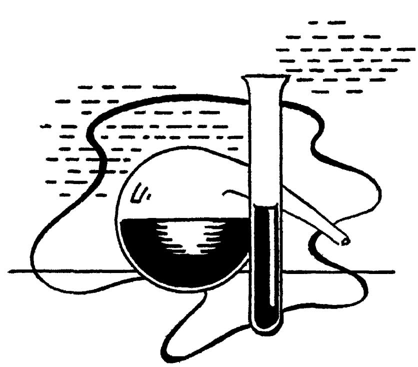
FIG. 12. Time recorder and power supply of particle counter.

particles acquire their static charge, there is evidently a theoretical upper limit to its magnitude. This is reached when the breakdown potential of air is exceeded and the excess charge leaks off It may be shown that the maximum possible charge on the particle is proportional to the square of its radius—i.e., to its surface area

An electrostatic particle counter based upon these principles was developed by Arthur C. Guyton at Camp Detrick between June and October 1945. In his apparatus, a stream of air is sucked through a hole 0.8 millimeter in diameter at the end of a 45-degree jet at a very high velocity. The aerosol particles are directed against an insulated metal collector close to the orifice. The solid dielectric particle imparts to the collector an electrical impulse of about 50 microseconds' duration, the voltage of which Guyton concluded was proportional to the square of the particle radius in accordance with the upper limit of charge mentioned above. This pulse is fed through a four-stage amplifier, where it is increased by a factor of one hundred thousand. The output pulse may be viewed on an oscilloscpoe or put through a thyration circuit to operate a mechanical counter. Guyton found that conducting solid particles like iron powder and aqueous drops produced very weak pulses, but that these may be amplified by charging the collector, and that the amplification was in direct proportion to the applied voltage.

In Guyton's apparatus an electrical discriminator could be set to limit the counting to pulses exceeding any predetermined voltage. He determined the size distribution of the particles in an aerosol with his instrument, assuming that the pulses are proportional of the square of the particle diameter. This application requires calibration of the apparatus with particles of known size, and assumes that the pulse voltage depends only upon the size of the particle. Guyton's counter was sensitive only to particles of about 2.5 microns in diameter or larger. Part of this limitation was due to the amplifier circuit which he employed, but it is doubtful if this method could be applied to particles as small as 1 micron in diameter. The initial voltage pulse caused by these small particles is no larger than the voltage fluctuations caused by the random motion of the electrons in the input circuit of the amplifier—the so-called thermal noise of the amplifier. However, the simplicity of the pulse pickup in the electrostatic counter is a decided advantage; it may prove useful in a study of the larger-size aerosol particles and may possibly allow a discrimination between particles of different chemical composition.

The next few years should see an expanded knowledge of the nature and behavior of aerosols, as the new optical and electronic methods are applied to their study. One of the fascinations of these shortest-lived of all colloidal systems is the ingenuity in theory and the experimental skill which their study requires.



THE "NATURAL SCIENCE IDEAL" IN THE SOCIAL SCIENCES

LEWIS WHITE BECK

Dr. Beck (Ph.D., Duke, 1937) who has taught philosophy at Emory University, the University of Delaware, and at George Washington University, is now professor of philosophy at Lehigh. His article is based on a lecture he gave last year before the the Delaware Psychological Association.

THE ORIGIN OF THE "NATURAL SCIENCE IDEAL"

IMAGINE a man who builds a house like the Joneses, at considerable inconvenience to himself because actually he needs something quite different. As soon as he gets the foundations laid, the Joneses begin tearing down parts of their house, adding new wings, and overhauling its foundations. Our poor social climber has committed himself; he has to continue to build according to his plans whether he likes them or not. To comfort himself, he says he has the kind of house the Joneses have and ignores the fact that they are doing a big job of renovating.

This little fable of keeping up with the Joneses fits the relations existing until a short time ago between the social and the natural sciences. The climbers, the social scientists, have tried to imitate the Joneses, the natural scientists. Now the social sciences have an immense house much of which is not very useful; it lacks many of the modern conveniences; but it seems to be scientific, just the same, and that often seems to be enough. But the social scientist might be far happier in his house, or he might be more successful in renovating it to meet modern needs, if he gave up pursuing the past glory of the great edifice of nineteenth-century physics.

When splitting off from philosophy in order to become scientific, the social studies took a bad moment to imitate the natural sciences. They did so just before the natural sciences themselves began to undergo major changes. The result is that many social scientists pride themselves on being natural scientists or regret that they cannot be, whereas the science they emulate or would like to emulate became obsolescent fifty years ago.

In imitating the natural sciences, the social sciences attempted to follow both the methods and the metaphysics of the former. The social studies tried to attend only to observable and measurable entities and to connect these by simple causal or functional laws. If the social scientists thought that they were like the natural scientists in studying "reality," they became mechanists or materialists. If they feared equating their verified hypotheses with "reality," as many natural scientists did, they became positivists. In either case they took over ready-made philosophies of the nature of scientific objects. But there was no unanimity on the philosophical foundations current among the natural scientists, and the "unity of the natural sciences," by virtue of which they might have served as an unequivocal model, was an illusion even before the death of Comte.

The social sciences, therefore, neither emerged from, nor could they later merge into, a homogeneous body of natural science doctrine. The natural science ideal, which many social scientists wished to pursue but which was vehemently rejected by others, was much more ambiguous than it appeared to be in the work of Comte and Spencer. By the time of Dilthey, with his emphasis on the function of sympathetic imagination in social studies, the opposition of the natural and the social sciences was predicated upon an almost complete misunderstanding of the methodological foundations and metaphysical implications of the natural sciences. It would have been much more to the point to have compared the status of the new social sciences with that reached by physics in the time of Galileo than to compare these nascent sciences with a physics already showing signs of passing through the change of life. The contrasts between explanation and description, between nomothetic and ideographic procedures, and between the ideals of a *beschreibende* and a *verstehende* psychology were not so much contrasts between the natural science ideal and the ideals perhaps more germane to the social studies, as they were signs of problems which every science, whether it be natural or social, must face in the early stages of its development.

It is consequently beside the point to contrast the natural and the social sciences in the language used in the early part of this century. Neither the

natural nor the social sciences were homogeneous bodies of doctrine in simple conflict with each other. No clear-cut decision could have been intelligently made between the alternative of following or rejecting the natural science ideal. There were analogous conflicts within both bodies of knowledge between opposing strategies. In each case these conflicts have been resolved in analogous ways during the present century. There is now a continuity of method and philosophy in the two branches of science that could not have been dreamed of even by the most naturalistic of social scientists of the time of Spencer, because this continuity is a consequence of a *rapprochement* in which both sciences have actively participated. We shall see this in detail throughout this essay; at the moment let it suffice to mention the vocabularies of the two. It would not be possible, upon looking into the index of a scientific book, to tell whether it was a book on natural or social science if it contained only the following entries: constitution, dimension, experiment, field, migration, population, prediction, probability, space, statistics, vector. And the list of common terms is growing year by year.

It would be going too far to say that there is no difference between the two groups of sciences, but we should not overemphasize their differences, as was frequently done early in the century, or underestimate them, as has been fashionable since then. It is sound scientific procedure to substitute differences of degree for differences in kind whenever apparent differences in kind can be interpreted as consequences of variation of some common factor. The common variable that I believe will account for both the unmistakable differences and the current *rapprochements* between the natural and the social sciences is "complexity of subject matter." It is my belief that the major differences between them are due to the greater complexity of the subject matter of the social sciences, and that differences of method and interpretation of results are due primarily to awareness of this difference.

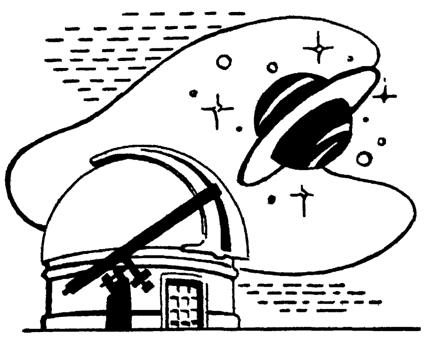
If this is correct, we should be able to test it empirically, by seeing whether the social sciences, when they deal with simple subject matters, are able to approach the natural science methods, and whether the natural sciences, when they deal with complex subject matters, appropriate social science methods. Let us, then, turn to an examination of their respective subject matters in order to answer the question: Will differences in complexity account for differences in their observational, experimental, and conceptual techniques?

SUBJECT MATTER OF NATURAL AND SOCIAL SCIENCES

When we think of the social sciences as only the "poor relations" of the natural sciences, we forget that an insight into the order of society was prior to that into nature. Every primitive people sees nature by an analogy with its social organization. Science began when laws, like those given by governments and tribunals, were projected into nature.

The great Greek philosophers approached nature with the anticipation that it would conform to simple principles, some aspects of their society providing them a model for the interpretation of nature. Anaximander (*ca.* 550 B. C.), in an epoch-making analogy, held that changes in nature are regulated by *justice,* anticipating the function later ascribed to *laws*. Henceforth nature was to be seen as a cosmos.

But in searching for regularity and simplicity and lawfulness, the philosophers and early scientists found that they had to work with abstractions from observations and not with complex observations themselves. From the time of Galileo, at the latest, we feel that the "right abstractions" were made, because he chose to report those aspects of his observations which could be related to each other by simple mathematical laws.



Since Galileo, the subject matter of the natural sciences has been relatively simple and repetitive series of simple events. Such series are repetitive because they can be isolated from many other events and understood without reference to them. The natural sciences deal with isolated systems, because the variables they choose to observe are controllable by means of varying other chosen variables. Solely for this reason are simple experiments possible. Only a small number of variables have to be known for us to give functional laws relating one to another.

Certainly every event in nature is related to an untold number of others, perhaps even to every-

thing else in nature. But by abstraction and material isolation, we are able to reduce the effects of most of the others to negligible quantities, and to attend only to the functional relations of certain chosen events. In the natural sciences, lack of repetitiveness in a series of events, as this occurs when an experiment "turns out wrong," is always taken as evidence that the systems were not isolated, and we thereupon carry through a process of successive approximations toward complete isolation.

Nature not only has serial orders which can be studied in relative isolation; the things of nature also come in "vertical" arrangements, or wholes with contemporaneous parts. Field concepts, rather than merely serial concepts, apply to this aspect of nature. Bue because we can isolate systems, we can determine the boundaries of these fields, and eliminate the factors which would make our study of a given whole unmanageably complex. By moving the "isolation partitions," we can determine experimentally the effects of parts on wholes and wholes on parts, even though we never deal with an entity which is not a part of some whole.

The subject matter of the social sciences, on the other hand, consists of highly complex constellations of complex events in systems that are only poorly isolated. Instead of indistinguishable atoms, as the chemist considers his subject matter, the social scientist must deal with societies of individuals of almost infinite internal complexity and variability. No one has yet made the fortunate discovery comparable to that of Galileo in physics: though we know that science cannot deal with an unlimited number of variables, no social scientist has yet shown us precisely which ones to choose to interrelate and which ones may be safely neglected.

When we try to isolate systems in the social sciences, we therefore do not know what to include in them and what to try to eliminate. We cannot move our partition boundaries at will, because the contexts within which we find human beings are not variable to such an extent that we can try out many different wholes for a single part. We cannot isolate a child from all social environments to see where the partition between eliminable and noneliminable environmental factors should be drawn. Until we do so, however, we have no generally acceptable rule by which we can decide what factors to include in our descriptions of the relevant environment or social field. We have parts always within wholes; and, though the social sciences have advanced on the basis of this recognition, which has often in the past not been given sufficient weight, it is hard to specify the relevant part-whole relation because it *always* obtains.

Let us not overlook the fact that these differences are differences of degree, and that as the social sciences approach the stage where they may be able to decide which few variables may be most profitably observed, the natural sciences are undergoing developments of techniques for taking more and more variables into account. It is now recognized that the high regularities of the physical sciences are only statistically simple; as the physical scientist gets closer to the individual object, as it were, the complexities that had been neglected before reappear. Instead of attending only to serial collocations of simple events, the physicist is now finding it necessary, in spite of all his efforts, to deal with field concepts and probabilities as ineluctable parts of his conceptual system. We should not forget that "statistics" is originally a concept and technique of social science, and its use in physical science signifies an often overlooked appropriation of social science methodology.

OBSERVATIONAL TECHNIQUE IN NATURAL AND SOCIAL SCIENCES

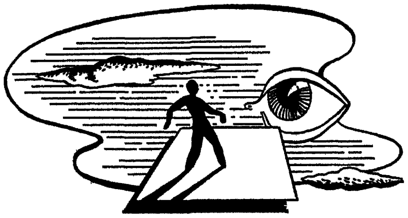
Observation in the natural sciences differs widely from that of everyday life. Most observations in natural science are instrumental results, usually observations of pointer readings. The major part of natural science work is not the taking of observations, but deciding what to observe and constructing instruments to make the observation. The observations of the natural scientist, therefore, are never the raw data or brute facts of common sense; they come to him already conceptually transformed and instrumentally abstracted from irrelevancies. They are what Loewenberg has aptly called "postanalytical data." In getting these postanalytical data, the scientific instrument reduces the subjective contribution of the observer almost to zero and "narrows the field of vision" to a specific observable event uniquely correlated with some unobservable we are interested in measuring.

Until about a century ago, observations in the social sciences hardly differed at all from those of everyday life. The student of social phenomena observed the phenomena of society as a physician would observe a patient if he had no thermometer or laboratory reports. The data of the social studies were "preanalytical." Where the contribution from the object ended and that from the observer began, no one could tell. Because there was no standardized instrument to narrow the field of vision to specific and relevant phenomena, the facts of so-

cial science might vary from common-sense observations to the narrow observations of a man with an *idée fixe*. The facts of the social studies were about as objective as journalistic observation, and no science could be based on such unstable and disputable facts.

As the social studies became scientific, they did so in part by the use of instruments. Usually these were not "brass instruments," but conceptual devices that served comparable purposes—reducing the subjective contribution to observations, and abstracting the desired observable from irrelevant data normally given along with it. But these conceptual devices served the same purpose as physical instruments: they gave an indisputable postanalytical datum which seemed to be uniquely correlated with some vague preanalytic observation or with some wholly unobservable entity in which the scientist was interested.

Consider an intelligence test, perhaps the most nearly perfect of all social science instruments. For a quality not directly observable but the ob-



ject of many common-sense judgments, the test substitutes a postanalytical datum, a ratio between two observed quantities, namely, age and a set of marks on a paper. The set of marks and this ratio are obtained by standardized and conventional procedures. "Intelligence" is not only measured by this instrument; it is operationally defined by the methods used to measure it. Until the test is devised, "intelligence" is not a part of scientific vocabulary at all.

Even with this instrument, the results still differ widely in scientific standing from those obtained with, say, a galvanometer. The galvanometer substitutes a postanalytical datum, a number, for a preanalytical datum, the shock we all feel when we hold a wire under some conditions. The galvanometer standardizes the conditions, eliminates subjective differences between observers, disregards irrelevancies such as the "appearance" of the circuit, and gives us a "hard" and indisputable datum. With a galvanometer, we can forget all about the original shock we felt. But with an intelligence test, we still think that it is measuring something that we already knew about, and if its results conflict too widely with those of our common sense, we decide the instrument must be changed. The social scientist simply does not trust his instruments as much as the natural scientist trusts his. The social scientist rightly reserves some insight against the reduction that his instrument would effect. However much the instruments of social science localize and control the subjective contribution to observations, the design, choice, and evaluation of instruments still depend upon the same kind of insight that social philosophers have always possessed or claimed; otherwise the results of instrumental observation may be very neat and elegant, but they have no noticeable relevance to the prescientific problems which led to the development of these, rather than other, instruments.

Hence the social scientist, equipped with the finest batteries of tests, is still in the position of the legendary people who wished to weigh a pig very accurately. They planed the board to which the pig was to be tied until it was of identical thickness, measured in "milli-micro-mulahs," throughout its length; they used as counterweights stones whose sphericity had been established within limits of one "milli-micro-mulah;" they carefully balanced the pig and board against the stones—then they asked the first stranger who came along to estimate the weight of the stones.

Because the operational definitions of the objects of natural science are applied to terms of no great emotional significance, and are definitions of which there are no counterparts in everyday language, we tend to forget that the way in which the natural scientist has obtained them is logically not unlike that of the social scientist or the legendary pig-weighers. The natural scientist's objects themselves do not determine what aspects shall be observed. The instruments he uses are extensions or projections of the questions he asks. With other questions, there would be other instruments and other data. The choice of his instruments is not ultimately determined by the object, but by the kind of answers he wants. In this respect he is exactly like the social scientist.

But here, again, the natural scientist knows better what he is looking for. As he is interested in correlating his data in simple functional laws, he is interested only in an instrument whose reading will be a variable in an equation by which he can predict what the reading on another instrument will be. He uses only those types of instruments which will give him such results; even further, he uses only those *specific* instruments which will give him those results, and sends the others to the shop. The social scientist, however, is lucky

if he possesses even a single instrument for getting data. Outside a few fields, such as that of factor analysis in psychometrics, he must correlate his instrumental results with his vague common-sense preanalytical observations; he therefore has little or no check on the accuracy of his instruments. In consequence, although the introduction of instruments into the armory of social sciences has given intersubjectively valid data which the social scientist did not formerly have, it has not permitted him to state categorically what is the conceptual significance of his results. He must still "estimate the weight of the stones."

Hence observation in both the natural and the social sciences necessarily involves a subjective element of choice of observable variables. But, whereas in the natural sciences this choice is constantly modifiable by reference to other chosen observations, in the social sciences the choice is usually corrigible only by reference to the "enlightened common sense" of the observer, which tells one social scientist (but unfortunately often him alone) what weight is to be attached to the results, which observations are worth getting, and which ones are not. We can see the reason for this in the differences in complexity of the sciences: the instrumental "sieving" of the facts of nature is very precise and fine-grained, whereas the facts of society are large-grained and recalcitrant to narrow abstractive procedures, whether instrumental or conceptual.

EXPERIMENTAL TECHNIQUES IN THE NATURAL AND SOCIAL SCIENCES

Often the contrast between the natural and the social sciences is as succinctly drawn as that between experimental and observational sciences. But there are nonexperimental natural sciences, and there are experiments in the social sciences. This contrast, therefore, is not perfect; but it throws light on another consequence of the different complexity of the two kinds of science.

The natural sciences, as we have said, can establish physically isolated systems in which only a small number of variables play a significant role; therefore, an experimental determination of their correlation is possible. The social sciences cannot physically or temporally isolate their subjects. Though experiments may be performed under conditions of imperfect isolation, neither in physics nor in sociology would we know how much of the object we were experimenting with. The physicist can meet this objection by moving his partition boundaries; the sociologist cannot. An experiment on children puts the boundary, let us say, at 9:00 A.M in a classroom; but the previous history of the child, the home conditions, the hereditary conditions, and so on are uncontrolled variables from which the subjects of the experiment are by no means isolated. The social scientist, therefore, has to perform the same experiment over and over again with the idea that the uncontrolled variables will be randomly distributed in the series and thus cancel each other out. Hence in the experiments of social science there is a large inductive element lacking in the interpretation of good experiments in the natural sciences.

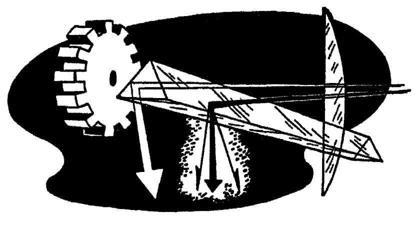
In recent years the social scientists, especially Lewin, have developed techniques for deriving results from only one or a very small number of experimental situations. This is possible when there are a large number of variables within the "field," so that some interconnection between them can be found and little or no recourse has to be made to relatively unknown variables outside the field. Work of this kind, in which the conceptual apparatus is adequate to the complexity of the subject matter, is one of the most encouraging signs of a further affiliation between the natural and the social science techniques. In contrast, when the external trappings of a natural science experiment are imitated, so that only a few highly abstract data are obtained, the lack of isolation of the variables being measured really prevents the experiment from being comparable to those of the natural sciences.

There is another difference between the experiments of the two branches of science which is dependent upon differences in their complexity. Isolation from the operator is difficult to achieve in the social sciences; the adventitious circumstances of the experimental setup, the isolation partitions themselves, function as significant causal variables. From the experimental results we can extrapolate to "normal situations" only with a wide margin of error, since these variables may be very important in the experiment and wholly absent in the situation we wish to make predictions about.

In experiments in natural sciences, the experimental situation is comparable to the normal, or at least the effects of the experimental situation can usually be estimated and conceptually eliminated. Certainly putting a new meter into a functioning circuit affects the circuit as a whole, but this effect can be measured in other experiments on the meter itself and we can eliminate the interference.

As the physical sciences come to deal more and more with the "individual physical object"—e.g.,

a single particle—it is found that the experimental conditions may play a more disturbing role which cannot be eliminated. The Heisenberg principle of uncertainty is illustrative of this comparatively unusual situation in the natural sciences, but one very common in the social The study of individual members of a population of electrons may suffer from many of the same disabilities as the



study of human individuals in society. As physics turns its attention to the complexities of the individual case, and sociology finds itself able to deal with large numbers of cases, their operational conditions and results become more nearly comparable.

Each science begins with "middle-sized" facts, those which are within range of convenient observation. The middle-sized facts of physics have a specious simplicity because individual differences have been statistically canceled out; if physics, like the human sciences, had begun with the individual case, it is likely that it would have made no more rapid progress than sociology. The middle-sized fact of sociology is the small community, and this is more complex and variable than the individual particle in physics. As sociology approaches statistically evened-out states of affairs, it may approach the simplicity of classical physics, which dealt only with its evened-out, middle-sized facts.

THEORETICAL STRUCTURE OF THE NATURAL AND SOCIAL SCIENCES

The differences between the theoretical structures of the natural and the social sciences are even more obviously contingent upon differences of complexity in their subject matter. We shall see this in two respects: the parsimony of the two systems, and the modes of explanation in the two systems.

First, a word about the theoretical structure of any science. In scientific research there are three types of hypotheses functioning. First, there is the *substantive* hypothesis, the hypothesis being tested. Second, there is an *operational* hypothesis, stating that if such and such things are done, such and such observable results should be attained, provided the substantive hypothesis is true. The operational hypothesis is always formulated as a basis for experiment or observation, and it is chosen in the light of the substantive hypothesis we wish to test. Finally, there are *collateral* hypotheses, which are not being tested at the moment, but which provide the route by which the mind moves from the substantive to the choice of the operational hypothesis.

To illustrate these hypotheses, let us take an exceedingly simple example. We have the substantive hypothesis "Salt is soluble in water." We test it by performing an experiment based on an operational hypothesis: "If I put the crystals from this bottle into water, they will disappear." How do we move from the former to the latter hypothesis, by which it is to be tested? We do so by means of certain collateral hypotheses, viz., "These crystals are salt," "This liquid is water."

A given hypothesis is not inherently substantive while another is always collateral. We may subject any one of them to test. In our previous example, for instance, we could test the hypothesis "These crystals are salt," using the other hypothesis, "Salt is soluble in water," as collateral.

When an observational result differs from the prediction from a set of hypotheses, it is always possible to choose whether we shall consider (*a*) the operational hypothesis to have failed (experimental error); (*b*) the substantive hypothesis, the one we intended to test, to be wrong; or (*c*) some collateral hypothesis, by virtue of which we choose this experiment, to be in error (systematic error)

If we decide on the first alternative, we are in effect "testing a fact by a theory." This is sometimes necessary in even the best-organized sciences in order to avoid renegade instances and to give credit to the obvious fact that not all observations are equally trustworthy. But science becomes dogmatic if this procedure is always followed, because then there can be no occasion to modify a theory once adopted. We have already seen the difficulty of eliminating experimental error in the social sciences, and consequently in them frequent recourse is had to this expedient; if the result is not as predicted, we can always say that there were disturbing and uncontrolled factors, or the observer was inaccurate, or the like.

Assuming that the experiment has been done well, we then have a choice as to which of the other hypotheses is to be modified or rejected. In the natural sciences this choice can be made by performing still other experiments involving different collateral hypotheses (in our example, we could use crystals from another bottle), or by undertaking other experiments in which the collateral hypothesis is tested without reference to the hy-

pothesis in which we were originally interested. (In our example, we could undertake a chemical analysis of the crystals in the bottle to see if they are sodium chloride.) The result of this multiplicity of approach is that in the natural sciences there need be no untestable hypotheses, and every well-performed experiment is crucial for *some* hypothesis in the body of the science.

Because of the complexity of each hypothesis in the social sciences, testing seriatim is rarely possible. For instance, we wish to determine the existence or nonexistence of racial differences in intelligence. We give a test to a group of children of different races. Their marks differ significantly. Does that prove the hypothesis that there are significant differences? Not unless we assume the collateral hypothesis, namely, that the test is independent of cultural differences. Can we test that experimentally? Only by devising a test in which the different cultural groups make approximately the same marks. But usually we cannot independently control the racial and cultural components; therefore, we do not know which hypothesis—a hypothesis about our particular intelligence test, or a hypothesis about the intelligence of different races—must be rejected.

Because some assumptions are untested in our experiment, there will be disagreement about them. The result is that we have "schools" of psychology and sociology (e.g., "racial theories" and "cultural theories") that are distinguished by disagreement about collateral hypotheses which function as "presuppositions." Crucial experiments which might resolve controversies between schools are thus almost unknown in the social sciences. The route by which we move from a substantive hypothesis to an observation or experiment is so circuitous, and involves so many assumptions, that experiments can usually be cited equally well by both sides in a controversy.

The hypotheses of the natural sciences are so simple that they can be tested seriatim; those of the social sciences are so complex and interpenetrating that we have to take them in families. Nevertheless, here again the difference is one of degree, and recent science is narrowing the distance between the two theoretical structures. The natural scientist now realizes that no hypothesis can be tested without assuming others, and ultimately a circle in testing is completed. There now exist in physics several alternative families of hypotheses in which the circle has been completed. All the observations of one are translatable into results of the other, though the two sets are not logically equivalent, and future observations *may* lead to decision between them. At present, the choice must be made in terms of their relative parsimony. Yet the estimation of the degree of parsimony involves aesthetic, procedural, and subjective considerations of elegance, ease of inference, and the like. The philosophy of science during this century has largely emphasized subjective elements in even the most objective sciences, and we find a prominent physicist speaking of science as "nature refracted through human nature." If the subject matter of physics were as complex as that of the social sciences, this human refractivity and selectivity would be more obvious than it is. If the subject matter of physics permitted the same variety of abstractions to be parsimoniously organized, it is likely that the conceptual structure of the natural sciences might appear as arbitrary as that of sociology or political science.

Finally, we come to the general strategy of explanation in the two branches of science. I do not refer to the age-old problem of mechanical *vs.* teleological explanation, for this metaphysical controversy appears in both types of science. I refer rather to the logic of explanation. In the natural sciences, the chief mode of explanation is description of the more pervasive and abstract features of the situation, whereby prima-facie different states of affairs are described in the same terms For instance, a freely falling body and the moon are special cases falling under Newton's laws. Explanation in the natural sciences is therefore analytic or reductive, through discovery of common and simple conditions of diverse effects whose prima-facie description would involve a very large vocabulary. Hence a phenomenon in chemistry is explained when it is described in the simpler terms of physics; the motions of the planets and of bodies rolling down an inclined plane are explained when a common set of variables is discovered in the description of each phenomenon.

Certainly this relation between explanation and description is met with also in the social sciences. We would, for instance, describe war and migration in quite diverse terms; but we might explain them in terms of a condition not obvious in either but underlying both, e.g., "population pressure." We shall, in the following section, deal with the limits of this type of explanation as one of the unsolved problems in the logic of the social sciences. Still, it must be admitted that at least at present the common mode of explanation in the social sciences is not reductive and analytic, but synthetic. That is, we predict some event in terms of psychology alone; but for more complex events we have to add to the psychological causes suffi-

cient factors to get to the effect we actually find. Thus we say that we must attend not only to the psychological conditions, but draw in also the sociological, the economic, and the like. What would be called explanation in the natural sciences is all too often seen as "oversimplification" in the social sciences.

The extent to which reductive techniques should be universally employed, especially in psychology in its relation to physiology, is one of the crucial problems in the philosophy of social science. Just as physiological description is translated into physiological explanation, it is often held that the logically simpler is everywhere the explanation of the more complex, and psychology must be "reduced" to physiology. If this is the case, then of course there is no autonomous social science; it is simply a division of labor to be tolerated only until the natural sciences are able to effect a reduction. Such reducibility, if it exists, strengthens the thesis that the difference between the two branches of science lies in their differing complexity. The argument of the reductionist is that in the future the natural sciences will become better able to deal with states of affairs of high complexity, and the social sciences will have succeeded in conceptually diminishing the complexity of societal facts, so that the transition can be made. At present it cannot be done, perhaps because of the great disparity in degrees of complexity. Whether it can and should be done is one of the unsolved problems to which we now turn.

SOME UNRESOLVED QUESTIONS OF SOCIAL SCIENCE STRATEGY

There are two problems we have lightly touched on, but which deserve more than passing notice even in a brief discussion. The first is strictly metaphysical: Are there any indigenous and irreducible categories of societal nature (e.g., culture, personality) that will successfully resist all attempts at reduction? Is the *only* difference between societal and natural reality a difference in complexity?

I have called this question metaphysical rather than scientific, for, whatever answer we give to it, the effect on scientific procedure will be the same. Different answers to this question will affect only the philosophical evaluation of the findings and procedures of the social sciences. Admitting irreducible categories would not in the least exempt the social scientist from reducing all that he can in order to increase the likelihood that the remaining ones *are* irreducible and not simply nonreduced. He would still do everything in his power to diminish the scope and importance of the not-yet-reduced concepts. Following the principle of parsimony, he would and should try to account for as much as possible by means of reductive explanation.

Analogous questions are met with in the natural sciences. The world of nature is not prima facie homogeneous, but has manifold discontinuities, levels of organization, and emergent properties. Acceptance of these with "natural piety" would have arrested the development of science. Yet the natural scientist does not have to explain them away in order to be scientific; he has only to attempt to explain them by showing the conditions under which they occur. Inasmuch as the necessary, but not the sufficient, conditions of life have been found in chemical studies, there still remains a task for the biologist—the description of his own phenomena, and the interrelation of them under

unreduced biological categories. There can thus be purely biological explanation if he is successful in elaborating a general system of biological categories.

Similarly, in the social sciences it may be argued that, though it is important to know the natural conditions of societal phenomena, the social sciences have an indigenous subject matter, their own categories for its elaboration (e.g., "meaning" or "value"), and their own techniques for dealing with them (e.g., "understanding"). Much can be said for this point of view so long as it is not allowed to arrest the reductive procedures by which societal phenomena can be related to those of nonhuman nature. We have to deal here with two basic principles of method.

In the logic of science there is a principle as important as that of parsimony: it is that of sufficient reason. The former directs us to look for simplest causes; the latter cautions us not to simplify so far that the explanation is inadequate to the facts to be explained. Opposition to the hegemony of reductionism, insisting on the autonomy of social science categories, emphasizes the importance of the maxim that the adduced reasons shall be sufficient, rather than that they shall be parsimonious. Parsimony is not itself a simple criterion of a good methodology; we cannot simply count the factors of explanation and say that the theory containing

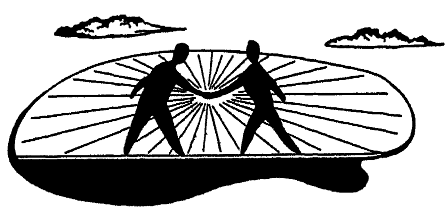
the smallest number is the best. The ideal of parsimony cannot be expressed without the proviso that the conditions for which it is a norm shall themselves be adequate. But if simplicity is difficult to define adequately, how far from simple it is to define adequacy!

Whether an explanatory system is adequate depends in the final analysis on what we want of an explanation. No one holds a brief for an "autonomous chemistry" and for the "indigenous and irreducible facts of chemistry" and thus fights physicalistic reduction. But for practical, even if not for metaphysical, reasons, a comparable reduction of social science concepts to those of natural science may be quite legitimately resisted. Even if we overlook the possibility of metaphysical discontinuities between nature and man, the social sciences, if they are to be of use either practically or for the sake of an insight into social problems, are inextricably tied to enlightened common sense with its terminology. Operational simplification of sociological terms may be oversimplification in the sense that the problems as solved in the reduced vocabulary of natural science are not practically or intuitively equivalent or germane to the problems that originally led to the undertaking of the study. It may be that we ask for the bread of social insight and are given stones of natural science. It may be that an explanation of societal events in natural terms will have to be translated back into original language before anyone will admit that the explanation is adequate to the problem at hand.

Translations of this kind seem trivial and inadequate to the understanding of the initial problem. At the present stage of social sciences, then, it is quite defensible to hold that the explanatory concepts shall be germane to the motivating problem and not simply statements of correlations between societal and nonsocietal phenomena. This being the case, the tasks of the social sciences are the determination of adequate germane categories, such as culture, meaning, function, and value; their rigorous definition within the context of social science phenomena; their theoretical elaboration into parsimonious explanatory systems; and the establishment of rigorous procedural rules for their empirical application. For these tasks, the history of the more highly developed sciences may provide useful cues, but no more.

The second problem, though closely related to the former one, is logically independent of any answer we give to that question. Assuming that reduction will be practiced as far as possible, we still have to decide the proper procedures with respect to the concepts and hypotheses which have not yet been reduced. It is a question of the strategy of theorizing in the social sciences themselves. To be more specific: The social scientists now debate the question as to whether the chief desideratum is a general overarching theory or a series of particularistic hypotheses of relatively low degrees of generality.

The history of the natural sciences provides a valuable guide to the answer to this strategic prob-



lem. The physicists kept their hypotheses as close to observations as possible; their theories were integrations of hypotheses, not highly abstract summaries of concrete facts all on the same level. Galileo and Kepler had to do their theoretical as well as observational work before there could be Newton's. But, we may be told, the social scientists now have almost as many hypotheses as facts; more unkindly, they may be said to be long on hypotheses and short on facts.

The mind of man, however, is not so prodigal of imaginative hypotheses that it can generate an infinite variety of them. Hypotheses show an inner kinship of common parentage in a given milieu and in the inventiveness of the social scientists—the demonstration of this being one of the great accomplishments of the sociology of knowledge. Hypotheses are increasing in number, but the variety of their types may be diminishing. General theory is not to be built by addition of hypotheses, except indirectly; it arises from their analysis and reduction.

Hence it may be expected that when a plethora of facts is elaborated in hypotheses of low generality, the broad outlines of an overarching theory may be subtly adumbrated. But in view of the complexity of subject matter, the looseness of theoretical structure, and the uncontrolled character of many of the observations of society, it is too soon to expect—indeed, it is too soon to be impatient for—a Newton of the social sciences.

EXPLORING THE OZONOSPHERE

CHARLES JAMES BRASEFIELD

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RESEARCH in upper-atmospheric physics has been stimulated in recent years both by a growing appreciation of the importance of physical phenomena at high altitudes and by the increasing availability of equipment suitable for investigating these phenomena. The most fertile part of the unexplored upper atmosphere is the region between 100,000 feet and 150,000 feet in altitude. Since this is the region of maximum ozone concentration, it may be called the ozonosphere. During the past ten years, considerable information on the temperature structure and, to a lesser extent, on the humidity and wind structure of the first 100,000 feet of the atmosphere has been collected by means of the radiosonde. This instrument is essentially a radio transmitter, whose signals vary depending on the pressure, temperature, and humidity of the air through which the radiosonde is rising.These signals are received and recorded by a ground station. By evaluating the ground station record, it is possible to determine the altitude of the radiosonde at successive points in its flight and the temperature and humidity of the atmosphere at these altitudes. Furthermore, if the transmitter is tracked by a directional receiving antenna, it is possible to compute the magnitude and direction of the winds encountered by the radiosonde throughout its flight. The balloon used to carry the radiosonde aloft is called a sounding balloon. The largest sounding balloon heretofore developed by the Signal Corps Engineering Laboratories and supplied to the U. S. Air Force for field use weighs about five pounds and has a diameter of about five feet when barely inflated. Balloons of this type generally burst around 100,000 feet, although occasionally one will reach an appreciably higher altitude.* A search of the technical literature indicates that the previous record for high-altitude balloon flights may have been made in Russia, where a balloon was reported to have reached 131,000 feet.[1]

The ozonosphere is a relatively unexplored region of the atmosphere. Our knowledge of the physical properties of this region has been obtained principally from indirect experiments rather than from soundings made in the region. It is known that ozone strongly absorbs radiation in the far ultraviolet (3200A–2200A). Measurements of the intensity of the solar spectrum in the far ultraviolet prove that ozone in the atmosphere is responsible for the effective termination of solar radiation at 2900A, and indicate that the concentration of ozone is a maximum (7 parts ozone to one million of air by volume) at about 130,000 feet. Absorption of ultraviolet radiation by ozone causes the temperature of the ozonosphere to increase with increasing altitude. This absorption is so strong that most of the ultraviolet radiation is absorbed by traces of ozone at approximately 200,000 feet, and consequently the temperature of the atmosphere should reach a maximum at about this altitude. This has been confirmed by experiments on anomalous sound propagation. Haurwitz[2] and others have suggested that the missing link between solar activity and surface weather may be found in the ozonosphere. They suggested that the larger quantities of ultraviolet radiation presumably emitted by a solar flare may cause disturbances in the ozonosphere that would be reflected in pressure changes at the earth's surface. If this reasoning is correct, then it is obvious that measurements of temperature, ozone concentration, and possibly water-vapor concentration, obtained by balloon-borne soundings in the ozonosphere, should be invaluable for weather forecasting.†

* On September 15, 1948, the Associated Press reported that a sounding balloon released at the USAF weather station at White Sands, New Mexico, reached a height of 120,000 feet.

† It is possible that there are localized regions in the ozonosphere which are especially sensitive to solar flares, and that these regions could be detected if the temperature structure of the ozonosphere were known. The projection of these regions on the earth's surface would then identify the areas in which disturbances in the general circulation may occur at the time of a solar flare.

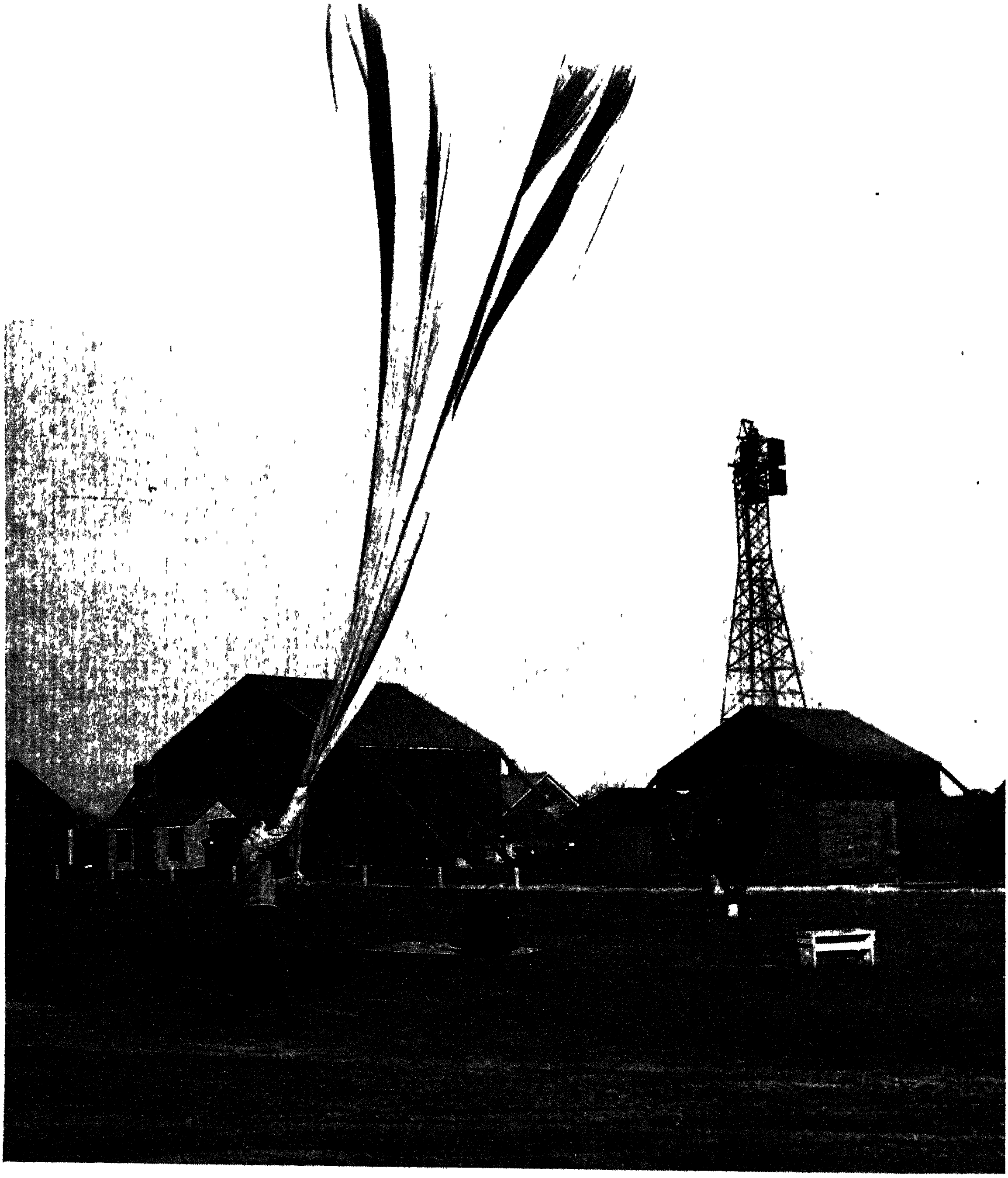


FIG. 1. An ozonosphere balloon just prior to release.

Information not only on the temperature structure of the ozonosphere but also on the wind structure may prove to be an important aid to weather forecasting. For a knowledge of the winds at these altitudes should contribute significantly to our understanding of the general circulation of the atmosphere.[3] Furthermore, data on winds and temperatures in the ozonosphere may be useful in designing and launching rockets and in evaluating the performance of these rockets.

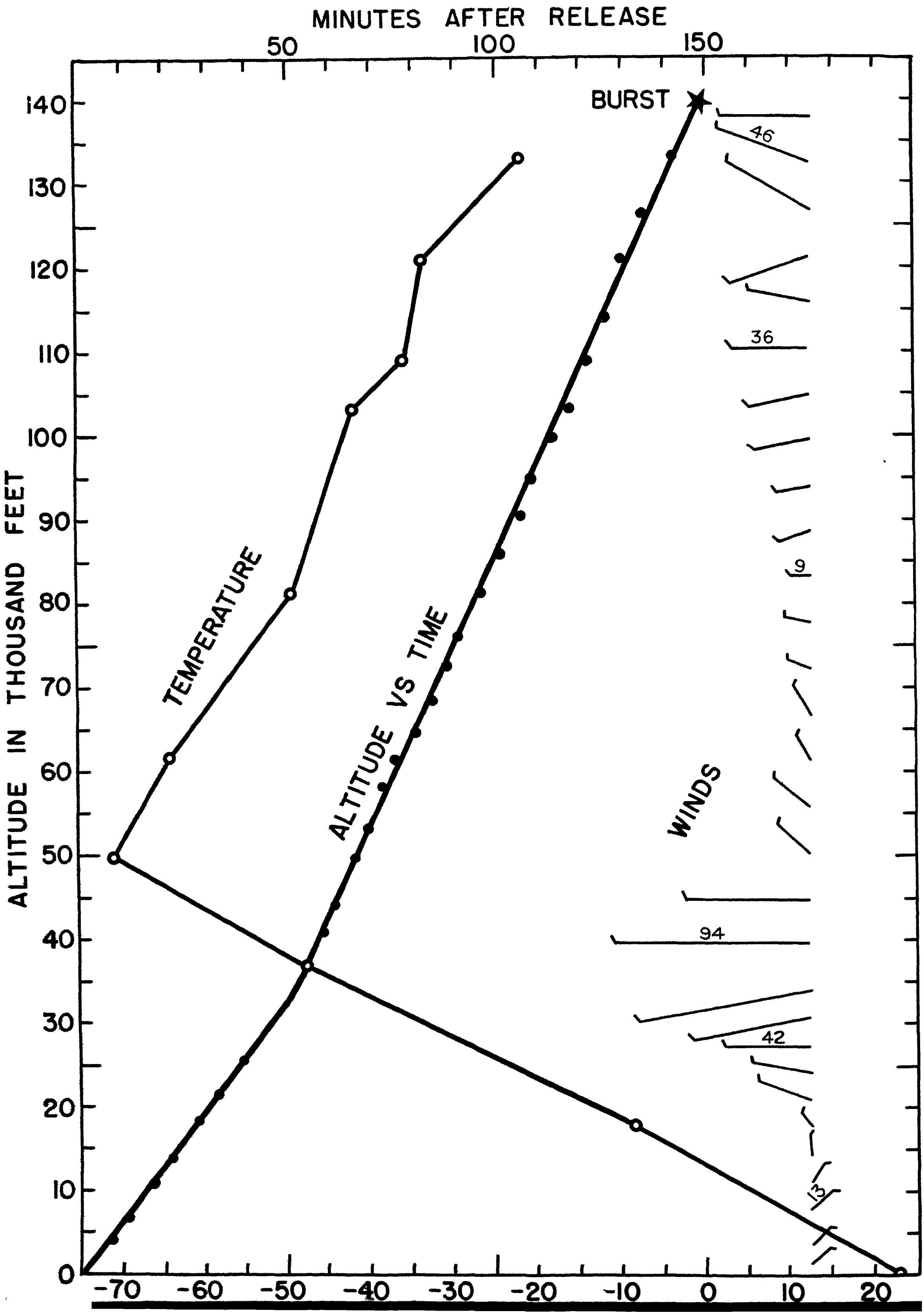
There are several problems involved in extending the limit of radiosonde flights from 100,000 to 150,000 feet. Perhaps the greatest of these problems is that of obtaining a suitable balloon. As has already been mentioned, sounding balloons heretofore available are capable of carrying a five-pound radiosonde to approximately 100,000 feet. Even if these balloons carried no pay load whatever, they would burst at approximately 115,000 feet; consequently, there would be little gain in maximum altitude if a cluster of two or more balloons were used to carry the radiosonde. What is required is a much larger balloon; how much larger can be easily estimated. The pressure at 100,000 feet is approximately 10 millibars (at the earth's surface, normal pressure is 1,013 millibars); at 150,000 feet the pressure has decreased to about 1 millibar. Thus, a balloon that will support a radiosonde at 150,000 feet must have ten times the volume required at 100,000 feet if no increase in weight of balloon is considered. Assuming that the weight of the balloon must be increased by a factor of five, the required volume at 150,000 feet must be about thirty times the bursting volume of the five-pound sounding balloon. This means that the surface area of the required balloon must be ten times greater than the area of the five-pound sounding balloon. In the initial design of the ozonosphere balloon, it was planned to gain a tenfold increase in surface area over that of the five-pound sounding balloon by increasing the weight by a factor of five and decreasing the film thickness by a factor of two. This objective was not quite attained in the first balloons to be developed,‡ which weighed about 17 pounds and whose barely inflated diameter was about 16 feet. When used to carry a radiosonde aloft, this balloon is not completely inflated when it leaves the earth's surface (Fig. 1). It first becomes spherical at about 35,000 feet, and when it reaches 140,000 feet its diameter is about 75 feet.

‡ This development was undertaken by Molded Latex Products, Inc., of Paterson, New Jersey, under contract with the Signal Corps Engineering Laboratories. The manufacture of the ozonosphere balloon requires equipment of a size unheard-of in the balloon industry. A spherical shell of gelled neoprene latex 57.5 inches in diameter is inflated to a diameter of 25 feet. After drying and curing, the balloon shrinks to a diameter of about 16 feet. Recently the equipment has been enlarged, and now spherical gels 81 inches in diameter are being inflated to a diameter of 30 feet. The first samples of the larger balloon, delivered in December 1948, weigh about 25 pounds and have a barely inflated diameter of about 19 feet.

The accuracy with which the altitude attained by the balloon may be computed depends primarily on the accuracy of pressure measurements throughout the flight. The probable error of pressure measurements made with the usual radiosonde is in the neighborhood of 2 millibars. This error is inconsequential at 50,000 feet, where the pressure is about 100 millibars, but becomes serious at 100,000 feet (10 mb), and is intolerable at 150,000 feet (1 mb). It was therefore necessary to develop a more accurate pressure-measuring element for the radiosonde.§ The objective of this development was a pressure element which, at low pressures, would be accurate to 10 percent of the measured pressure. This would permit altitudes to be computed with an accuracy of about 2,500 feet. It should be noted that this objective requires that in the vicinity of 140,000 feet pressure be measured with an accuracy of 0.2 mb, and in the vicinity of 150,000 feet with an accuracy of 0.1 mb. These are very exacting requirements for flight equipment. At the present time, insufficient test data are available to determine the probable error of the new pressure element, but it is estimated to be in the neighborhood of 0.2 mb in the range 1–2 mb. (As an additional check on the accuracy of the new pressure element, future high-altitude radiosonde flights will carry as accessory equipment a small hypsometer, which should measure small pressures with an accuracy of 0.1 mb.)

During the summer and fall of 1948, about twenty radiosonde flights were made from the Evans Signal Laboratory at Belmar, New Jersey, using development samples of the ozonosphere balloon. The performance of these balloons varied, owing to changes in manufacturing techniques, but on the average the balloons reached an altitude of about 120,000 feet. Four balloons went above 130,00 feet, and at least one balloon reached 140,000 feet, which is a new record for sounding balloons. The results obtained from this flight are shown in Figure 2. It can be seen that the balloon rose about 650 feet per minute until it reached 35,000 feet, after which the rate of ascent increased

§ This development was undertaken by Washington Institute of Technology, of College Park, Maryland, in accordance with design and performance specifications prepared by the Meteorological Branch of the Signal Corps Engineering Laboratories.


MINUTES AFTER RELEASE
50
100
150
BURST
ALTITUDE IN THOUSAND FEET
140
130
120
110
100
90
80
70
60
50
40
30
20
10
0
TEMPERATURE
ALTITUDE VS TIME
WINDS
46
36
9
94
42
13
-70
-60
-50
-40
-30
-20
-10
0
10
20

to 1,080 feet per minute. The temperature of the air, which was 23° C on the ground, fell to a minimum of – 71° C at 50,000 feet, then rose to – 22° C at 133,000 feet. The winds aloft were generally westerly, with a maximum speed of 94 miles per hour at 40,000 feet. On previous flights during the summer, the winds were also westerly below 50,000 feet, but were predominantly easterly above 60,000 feet, so that the distance of the balloon from the launching site was decreasing during the latter part of the flight. On several occasions under these conditions, the balloon was easily visible to the naked eye at an altitude of 120,000 feet.

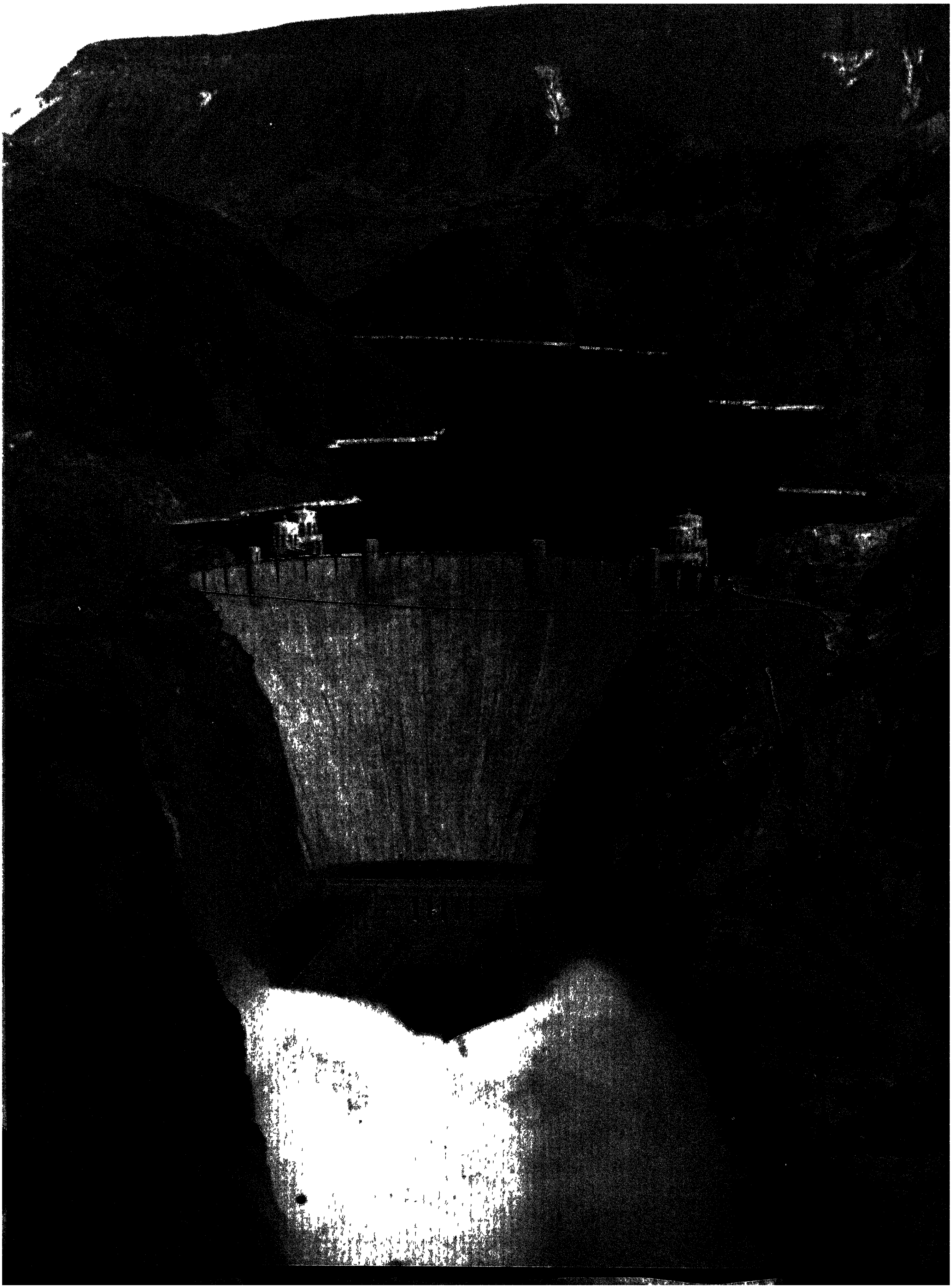
Plans for the future include daily flights to 150,000 feet, in an attempt to correlate the temperature of the ozonosphere with surface weather. Measurements will also be made of the diurnal variations in the temperature of the ozonosphere. In addition, simultaneous flights will be made from widely scattered points in order to obtain a better knowledge both of the temperature structure of the ozonosphere and of the atmospheric circulation in the ozonosphere. These data will be correlated with similar data obtained in another phase of the upper-atmosphere research program of the Signal Corps Engineering Laboratories, namely, the measurement of temperature, winds, and gas composition at high altitudes by use of rockets.[4]

FIG. 2. Results obtained from balloon flight at Evans Signal Laboratory, Belmar, New Jersey, on September 28, 1948, 1:20 P M. The length of the wind vector is proportional to the wind speed (mi/hr). The tail of the wind vector indicates the direction from which the wind was blowing. Thus, near the ground, the wind was northeasterly; at 15,000 feet, northerly; at 40,000 feet, westerly; and at 50,000 feet, northwesterly.

These experiments have required the cooperative effort of several sections of the Meteorological Branch of Evans Signal Laboratory. Thanks are due especially to Lt. Col. A. F. Cassevant, Director of the Laboratory, for his generous support of this program; to Dr. Michael Ference, Jr., Chief Scientist, for his enthusiastic sponsorship of upper-atmospheric research; to Mr. A. Arnold for valuable advice in the design of the balloon and for supervising the launching of balloons; to Mr. W. C. Conover for assistance in calibrating radiosondes and in evaluating flight records; and to the Rawin Section for supplying high-frequency transmitters and for operating the direction-finding equipment.

REFERENCES

1. DOBSON, G. M. B. *Sci. Progress*, July 1935, **30**, (117).
2. HAURWITZ, B. *Trans. Amer. Geophys. Union*, 1946, **27**, 161.
3. ROSSBY, C. G., and WILLETT, H. C. *Science*, 1948, **108**, 643.
4. LOTZ, W. E., JR., and FERENCE, MICHAEL, JR. *Weatherwise*, 1948, **1**, 52.



WATER, WATER, EVERYWHERE, BUT . . .

BERNARD FRANK and ANTHONY NETBOY

Mr. Frank (M. F., Cornell, 1929), assistant to the chief, Division of Forest Influences, U. S. Forest Service, has been engaged in watershed management research for the past eleven years. In collaboration with Mr. Netboy (M.A., Columbia, 1928), former editor of the U. S. Forest Service, he is writing a book—of which this article will be the first chapter—to be published next year by Alfred A. Knopf, Inc., under the title "Water, Land, and People"

WATER—especially in the humid regions of the United States—is commonly regarded as ubiquitous and plentiful. Produced in a diversity of ways, it is an indispensable component of all forms of life, from the elephant to the microbe, from the giant sequoias to the tough, crusty lichens clinging to the sides of a granite cliff.

Like other natural resources, such as forests and minerals, water is unequally distributed over the earth's surface. Even in regions blessed with abundant rainfall, the supply is definitely limited. Not all the rain or snow that falls on the earth remains on the surface. Some evaporates into the air, sinks into deep crevices or fissures in the underlying rock—where it may be out of reach of wells and pumps—or returns through subterranean channels to the oceans. The balance is potentially useful water, and its volume and quality are determined, to a significant extent, by factors generally within man's control. Regions sorely deficient in water remain barren or uninhabited, or at most capable of supporting only primitive forms of human society.

Useful water is not "free" or excessively abundant anywhere in the United States. It is largely a commodity whose production and distribution require the outlay of labor and materials and often the application of unusual skill to control its flow and deliver the right amounts and desired quality to the right places. This is true of drinking water, irrigation water, and water for hydroelectric power, inland navigation, manufacturing, and even for fishing and recreational purposes. Safe water "in the raw" is today available on a relatively small portion of the nation's 3 million square miles, mostly in the high plateaus and mountain ranges not yet penetrated by highways, railroads, settlements, towns, or industries. Only in such relatively inaccessible regions, where the water has been filtered and cleansed on its passage through the layers of virgin soil, or washed down from a melting snowbank or glacier perched on a mountain peak, can a person feel safe in drinking from a spring, pool, lake, or stream.

Water has certain characteristics and behavior patterns that distinguish it from other natural products. By far the greatest source of water is the moisture-bearing winds. Unlike trees or grass, whose production can be diminished, maintained, or even increased by man's intervention, no way has been found to increase the amount or change the character of the precipitation that falls upon the earth, except experimentally and on a very much restricted scale. (Climatological investigations are raising a suspicion, however, that the exposure of large areas of forests and grassland by clearing for agriculture, towns, airports, and highways may be partly responsible for the less favorable rainfall conditions of recent times.)

Like soil, water can be productive and support healthful and prosperous communities provided its energy or flow is properly controlled and wisely used. But also, like soil, the flow and behavior of water when uncontrolled or unwisely used, may cause great distress, and impair or destroy, sometimes rapidly, the livelihood of communities. In fact, our treatment of the soil and its plant cover determines to a great extent whether water becomes friend or foe.

Because water is such an intimate part of our daily lives, most of us give little thought to it. Even in the drier Western states, people have been concerned almost entirely with water rights and water developments and have ignored the distant forest and range uplands whose condition determines the amount and quality of usable water that will become available to them. Few city folk have any idea of—and much less care about—the source of their water supply. Only when a crisis looms, as during extended drought or disastrous flood, does the stunned or annoyed populace become conscious of its water problems. And then it usually prefers to apply only temporary or stopgap measures aimed

Hoover Dam, downstream face. The usefulness of the 115-mile-long reservoir above it is steadily being diminished by silt washing down from the eroding Colorado River watershed. (Bureau of Reclamation photo.)

Columbia River flood scene, June 1948. Floods are perhaps the most aggravating type of water problem that has plagued the United States in recent decades. (U. S. Forest Service photo.)

at eradicating the immediate, superficial causes of distress.

The United States has, in fact, never faced up squarely to its water problems. Our policies, whether activated by Federal, state, or municipal agencies, have been to a large extent a potpourri of mutually conflicting measures, one community often striving to obtain benefits that prove harmful to others or even to an entire region.

Our water problems, like the land problems with which they are intimately related, are the product of civilized man's constant efforts to adapt his physical environment to his economic and social needs. In other words, they are the end result of man's lack of foresight, combined with greed and indifference to the welfare of his fellows. They are also, to a large extent, the result of ignorance of the laws of nature, as well as a refusal to adjust human institutions to conform more closely to these laws.

TOO MUCH OR TOO LITTLE WATER

Floods are perhaps the most aggravating type of water problem that has plagued the United States in recent decades. Soil-depleting agricultural practices, the burning and misuse of forest and grazing lands, and the general laxity in preventing erosion have contributed to devastating inundations and heavy debris and sediment movements over large parts of the country. A key factor has been the settlement and overdevelopment of vulnerable bottom lands, especially in the densely populated Eastern and Far Western states. The flood plains and channels of the Ohio, Mississippi, Allegheny, Connecticut, Delaware, Potomac, lower Missouri, Susquehanna, Willamette, Columbia, Sacramento, and other rivers, as well as their tributaries, have been increasingly encroached upon by real-estate developments, manufacturing, industrial and business establishments, and intricate transportation and communication networks. At the same time, the uplands have been increasingly abused by improper clearing, plowing, grazing, or lumbering, causing rain and melting snow to cascade into and overload the stream channels and tear up their once stable banks and bottoms. Over large parts of the United States nature's balance has been upset all the way from hill and mountaintop to valley floor, and despite large-scale costly measures the toll of flood and sediment damage has mounted steadily, and in recent years has averaged some $300,000,000 annually in property, business, and crop losses alone.

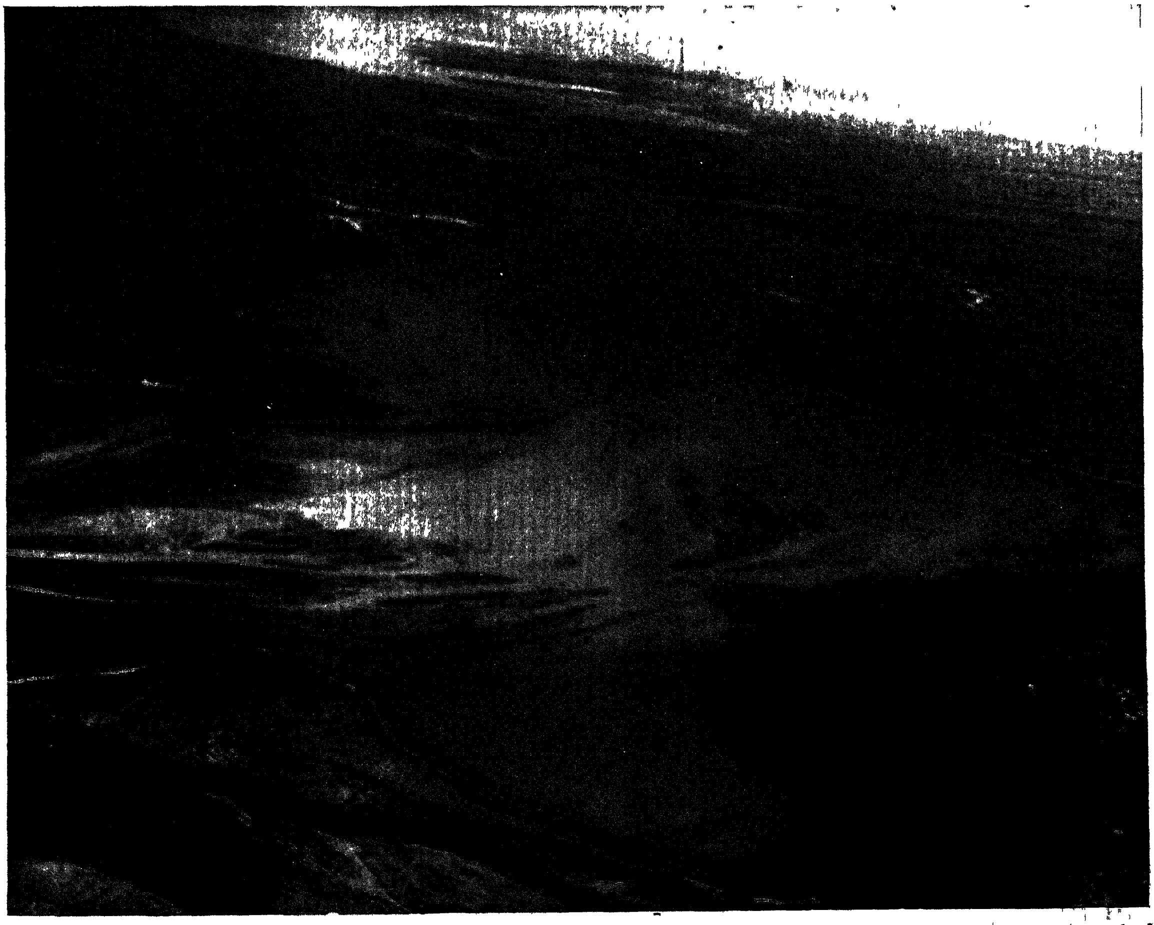
The flood problem is intimately related to the

growing inadequacy of many streams for navigation. Increasingly dams must be built on our major river systems to help smooth out the erratic seasonal flows; unstable river bottoms must be dredged to prevent channels from filling up and forming sand or gravel bars; and riprap and other devices must be constructed to protect riverbanks against erosion.

Nearly all these, however costly and imposing, are by themselves usually stopgap measures. In a great many localities, dams and other channel restrictions intensify rather than solve streamflow and sediment problems, as for example, along the Rio Grande following the construction of the massive Elephant Butte Dam in New Mexico, along the Colorado River after the building of gigantic Hoover Dam, and within the Grand Coulee Reservoir on the Columbia River. In fact, no permanent correctives for unstable channels or floods can be developed until the watersheds of our more troublesome river systems are dealt with as integral units. Local water problems are usually part of a vast pattern, and we cannot successfully cope with any facet—floods, sedimentation, water shortages, etc.—unless the basic evil—abuse of watershed lands and their waterways—is corrected.

Just as some parts of the United States are suffering from a surplus of water, others are struggling with perennial shortages. Scarcity of drinkable or otherwise usable water is fast becoming the limiting factor in the expansion of agriculture and industry and the growth of many communities in every part of the nation.

Only fifty years ago in the humid sections of the United States the natural flow of almost any large stream could be tapped to provide, with little treatment, ample quantities of "sweet" water, even in dry seasons. Today such favorable conditions are practically nonexistent. Underground water levels have receded in many developed or settled sections, forcing us to sink wells to great depths, with often less water for our pains than in the days of shallow wells. The deep wells, moreover, require expensive pumping equipment, which entails heavy operating costs for fuel oil, coal, or electric energy. And the water obtained in many instances



The life of an increasing number of water-supply reservoirs—on which farms, cities, and towns depend—is threatened by excessive siltation. (U. S Soil Conservation Service photo.)

is of unsuitable chemical quality if "pure," or it contains salts or pollution that have seeped in.

There are few communities of any size that can depend any longer upon local, natural stream flow for their normal water supply. Municipalities and towns must constantly be on the alert for new sources and must incur larger outlays for storage reservoirs, aqueducts, and treating plants. Such problems are being faced, sometimes acutely, by Los Angeles, Baltimore, Tucson, Chicago, Louisville, New York, San Francisco, San Diego, Santa Barbara, Philadelphia, and scores of other municipalities. And many a city or town that only a few years ago had adequate reservoir capacity now finds that its resources have been depleted by siltation from eroding watersheds or by insufficient inflow during the dry season from disturbed mountain streams. Thus it is conservatively estimated that because of siltation 21 percent of the nation's 2,700 water-supply reservoirs have a useful life of under fifty years. In the Southeastern states, where the rate of siltation is high, 33 percent of the reservoirs face complete loss of usefulness within a half century.

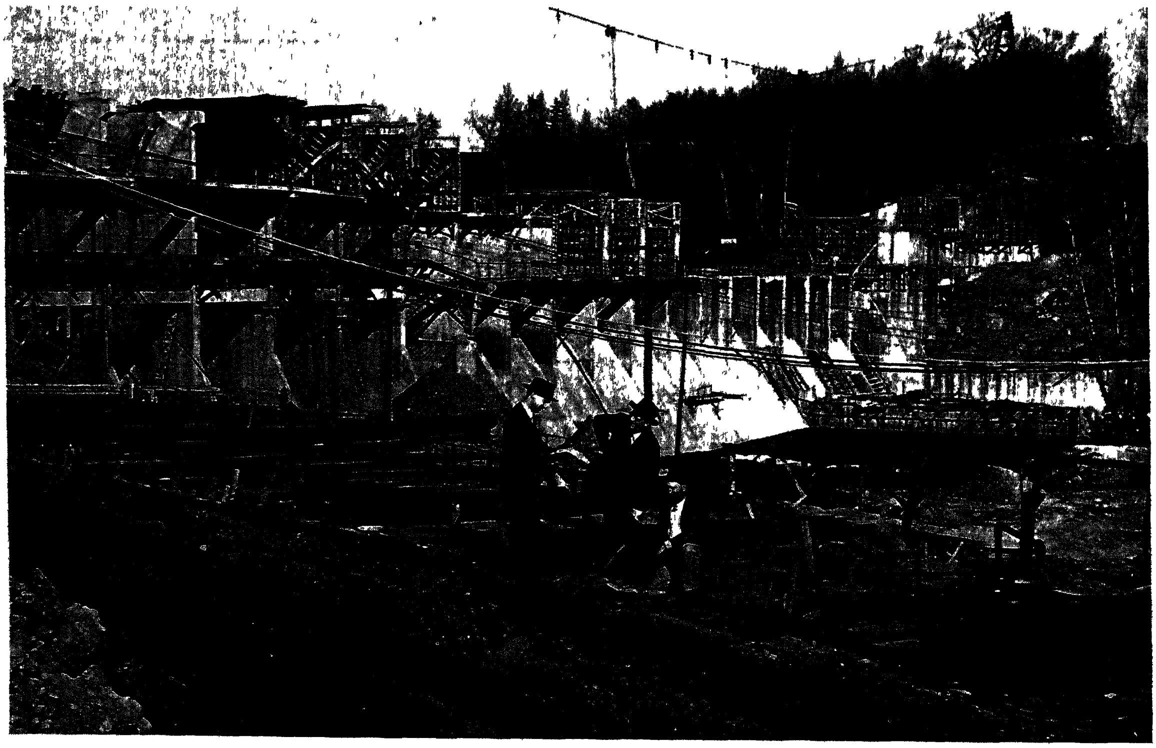
As a matter of fact, the nation's preventable water problems generally are destined to increase, perhaps in geometric ratio, if remedies are not applied on a unified watershed basis from the uppermost headwaters to the mouths of the major rivers. So far not a single watershed of any size in the United States has been so treated.

DEMAND FOR WATER OUTRUNNING SUPPLY

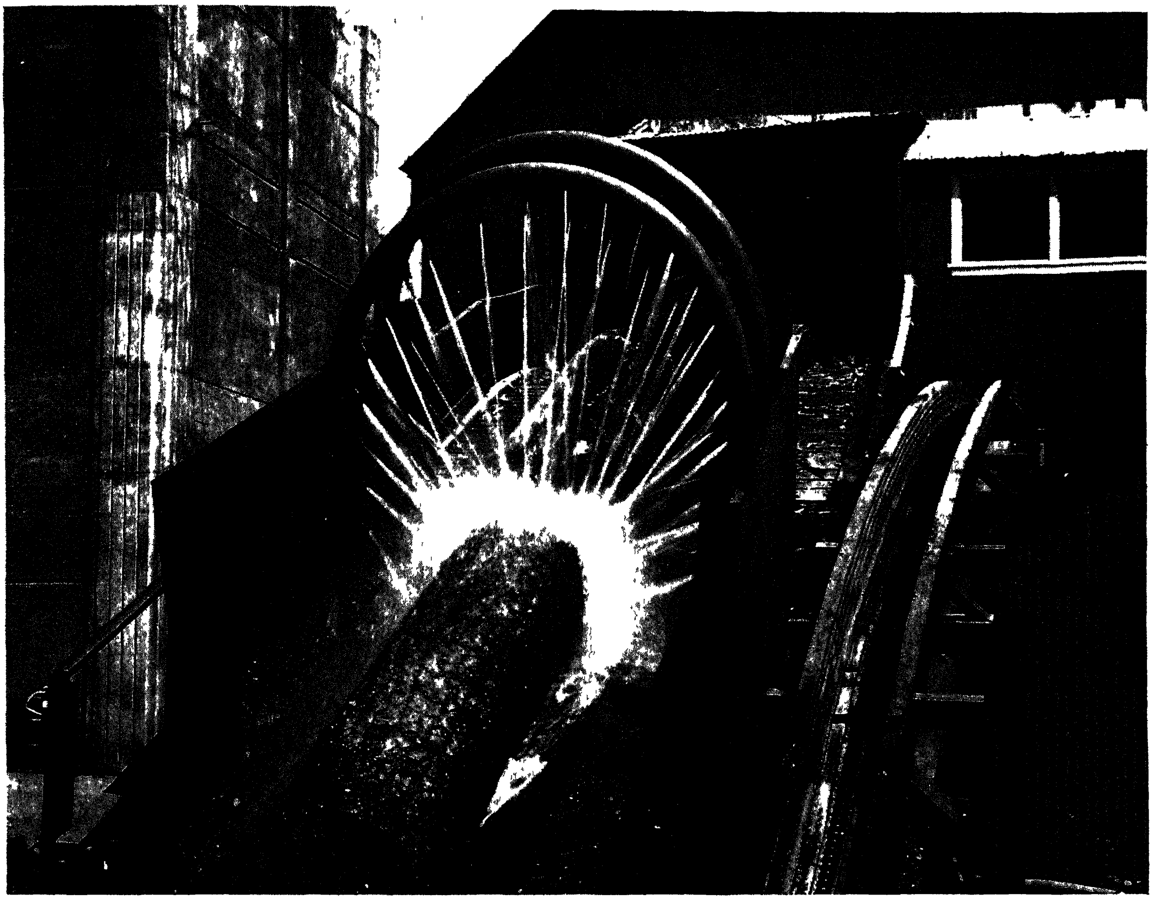
Although both the quantity and quality of our water supplies are declining, demand is steadily rising in response to population growth, industrial progress, expansion of agriculture, and technological improvements. Such developments as air conditioning and the spread of rural electrification create greatly augmented needs for water. Likewise, as the irrigated acreage increases and farming becomes more intensive and diversified, much more water is required. Also, rising health and living standards inevitably mean higher per capita water consumption.

A hint of the phenomenal growth in the demand for municipal water may be obtained by the increase in water consumption in a large city like Chicago. In 1880 the rising municipality of Chicago, with a population of 500,000, consumed around 140 gallons per person per day, or some 26 billion gallons per year. By 1940 the 3,400,000 inhabitants of Chicago were using about 290 gallons per person per day, or some 360 billion gallons every year.

The rapid growth of cities has indeed put a



Brighton Dam, Washington Suburban Sanitary Commission. Water for drinking and other domestic purposes is obtainable only at considerable outlay of labor and materials. (U. S. Soil Conservation Service photo.)



Water enters intimately into all industrial processes in a great variety of ways. The growing shortages of water threaten the existence of many industries. (U. S. Forest Service photo.)

tremendous strain on our water supplies, but expansion of irrigation agriculture has added perhaps even greater burdens. Public and private irrigation projects now under way or projected call for the development of nearly all the water supplies still "unused" west of the Mississippi River. The area in irrigated crops has grown from less than 8 million acres in 1900 to nearly 20 million acres in 1940 and 22.5 million acres in 1948; under current authorized plans, more than 7 million acres of new land in the 17 Western states will be supplied with water by the Bureau of Reclamation, and supplemental water will be brought to more than 3.5 million acres now inadequately supplied.

The expansion of irrigation agriculture creates a large *indirect* as well as direct drain on our limited water supplies. New rural and urban communities spring up in the reclaimed areas; and business, transportation, and industrial enterprises arise to process and ship the agricultural produce and to provide farm supplies, equipment, and community services. All these require water for their very existence.

As a result of rapidly increasing demands, competition for "water holes" is rife in some parts of the United States. Industrial or municipal demands are infringing upon the needs of agriculture in Utah, Arizona, California, and even Virginia. Some states have taken up arms against other states to protect their water rights—and cities are locked in combat with other cities. Communities in southern California and central Arizona are disputing bitterly for rights to Colorado River water despite a compact, reached after much bickering, allocating the supplies impounded by Hoover Dam and its 115-mile reservoir, Lake Mead. Los Angeles is barring the towns on its periphery from tapping its water mains, so short is the supply for the mushrooming city. Demand for water here has already reached a point which engineers before World War II did not expect before the year 2000. Municipalities like San Diego, with war-swollen populations, find that demand is constantly outrunning resources, and the search for clean, usable water becomes ever more intense. Philadelphia's avowed intention to

Industrial and silt pollution on far too many Eastern and Western streams is destroying fisheries and otherwise rendering the water unfit for use downstream. (U. S. Forest Service photo.)

increase its draft on the Delaware River is causing alarm in New York as well as in New Jersey cities. New York City's expressed desire to tap the Connecticut River is arousing the ire of Boston, and its claim to rights on the Delaware watershed has already resulted in litigation with the city of Philadelphia.

The expansion of our economy will inevitably intensify the quest for dependable water supplies. For example, a major battle has developed in the far-flung Missouri River Basin between economic interests of the lower valley, who demand adequate stream flow for navigation, and those on the upper reaches of the river and its tributaries, who seek water for irrigating new farm land. The flow of the vast Colorado River is being diverted in several directions—to the Missouri River in the east, and to the farms and cities of the Intermountain Basin, Imperial Valley, and southern California on the west, thus vitally affecting the often conflicting economic interests of Utah, Colorado, Arizona, California, and even northern Mexico.

In some parts of the United States there are already insufficient water supplies to support the present population, industries, and irrigation agriculture. For example, the building of the huge Geneva steel plant at Provo, Utah, provided a shot in the arm for the economy of the region, but it also created a tremendous water problem. The Weber River, whose waters are diverted to this area, cannot indefinitely supply the towns, the steel mill, and the farm lands dependent upon it. Likewise, the Los Angeles area will soon have to decide between airplane factories and orange groves, since the available sources of water cannot indefinitely supply both at reasonable cost.

Other Western regions are in a similar plight. In the San Joaquin Basin of California the usable water supply fails to meet the needs of the 2 million valuable acres in irrigated farms which produce, among other crops, luscious fruits for the discerning palates of Eastern as well as Western people. In years of abnormally low rainfall, low waters in the streams bring crop failures or force farmers to leave the land fallow. The rivers cannot be navigated, and saline waters from San Francisco

Bay seep into the wells of farms on the coastal plain. During such periods ground waters are overdrawn and water tables are lowered, causing pumping costs to zoom.

In the Salt River area in Arizona, water users are threatened with the loss of the vast storage capacity of Roosevelt Reservoir, on which one tenth of the state of Arizona, especially the highly productive Phoenix region, is dependent. Here sediment is washing down from the overgrazed highly erosive granitic soils of the mountains and foothills into the channels and reservoirs.

The Gila River watershed in Arizona and New Mexico presents another challenge to man's ingenuity in restoring our water resource. In this region of 17,000 square miles, floods have become more frequent and destructive in recent years, ground waters are ebbing materially, and the large San Carlos irrigation reservoir is silting up. Large portions of the watershed have been stripped of their grass and brush cover, with the result that flash floods are more common in winter and heavy movement of debris in summer.

In these areas, as elsewhere, uncontrolled or uncoordinated use of the land and its water resources by diverse interests cannot continue without harming the entire economy. Even in the Columbia River watershed, despite vast sources of relatively untapped water supplies, conflicts have already developed between flood-control and irrigation interests on the one hand, and commercial fishermen on the other. Also, California is now considering the possibility of tapping the distant Columbia's flow to augment the supply from the Sacramento River. Industrial and silt pollution, however, on tributaries of the Columbia River, as on the Willamette, for example, is rendering the water unfit for use further downstream without costly treatment. On the uplands, unsatisfactory agricultural practices, overgrazing, the effects of past fires, and accelerated logging and road building are resulting in high rates of snow melt, surface runoff, and sedimentation and debris movement, as was so forcibly demonstrated by the flood of May–June 1948.

Snow survey investigations, Crater Lake, Oregon. Too few people give any thought to the source of their water supply in distant forest uplands. (U. S. Soil Conservation Service photo.)

In short, so long as present land-use trends continue, the nation's hill and mountain areas face a steady decline in their ability to furnish well-regulated supplies of usable water to farms, cities, and towns dependent upon them, or to restrain runoff that annually floods tilled and populated bottom lands, with catastrophic results.

A FOREST REAPPRAISAL

LYLE F. WATTS

Mr. Watts (M.F., 1928, Iowa State) is Chief of the United States Forest Service. He has been with the Forest Service since 1913 except for a year as professor of forestry at Utah State College.

SEVENTY-SIX years ago the American Association for the Advancement of Science, at its meeting in Portland, Maine, took note of the alarming rate of exploitation and depletion of the forest resources of the United States. It appointed a committee to memorialize Congress and the state legislatures, urging action to stop the wanton destruction of American forests and to make provision for the proper use and perpetuation of the forest wealth remaining.

Three years later, in 1876, Congress gave a modest bit of recognition to this request. To a section of the appropriation bill granting funds for the distribution of seed, Congress added a provision that

> . . . two thousand dollars of the above amount shall be expended by the Commissioner of Agriculture as compensation to some man of proved attainments, who is particularly well acquainted with methods of statistical inquiry, and who has evinced an intimate acquaintance with questions relating to the national wants in regard to timber, to prosecute investigations and inquiries, with the view of ascertaining the annual amount of consumption, importation, and exportation of timber and other forest products, the probable supply for future wants, the means best adapted to their preservation and renewal, the influence of forests upon climate, and the measures that have been successfully applied in foreign countries, or that may be deemed applicable in this country, for the preservation and restoration or planting of forests; and to report upon the same to the Commissioner of Agriculture to be by him in a separate report transmitted to Congress.

That was a large order, even for a man of "proved attainments." The man selected for this comprehensive assignment was Dr. Franklin B. Hough, who had headed the AAAS committee that prepared the memorial to Congress. Dr. Hough set diligently to work. He traveled over the country visiting lumber districts, tanneries, and tree plantations. He interviewed governors and other officials of many of the states, and circularized land offices and manufacturers using wood. A year after he took office he was ready with his report. In spite of its 650 pages, Congress ordered a printing of 25,000 copies.

His appointment being continued, Dr. Hough completed a second report in 1878, and a third, after a trip to Europe, in 1882. His three reports were monumental pieces of work. They gained wide attention both in this country and abroad; they were awarded a diploma of honor at an international geographical congress in Vienna. They brought together more information on American forest resources than had ever before been assembled. They helped greatly to stimulate the development of a forest conservation movement in the United States. From the small acorn planted by the AAAS, an oak began to grow.

It was of course impossible for any one man to obtain complete and accurate information on the extent and condition of forests and forest lands covering nearly half a continent. We still lack complete, detailed, up-to-the minute information even today. Since Dr. Hough's reports, a number of other reports have been compiled, each adding to our understanding of the forest situation in America. In 1909, the Bureau of Corporations made a study of lumber supplies and uses. The "Capper Report" of 1920 was prepared by the U. S. Forest Service in response to a resolution introduced by Senator Capper of Kansas calling for a report on timber depletion, lumber prices, lumber exports, and timber ownership in the United States. In 1933, the Forest Service prepared and sent to the Senate *A National Plan for American Forestry.* Popularly called the "Copeland Report," since it was published under authorization of a Senate resolution introduced by Senator Copeland of New York, this 1,677-page report brought together the largest amount of information on the forests of the United States that had been assembled up to that time. It also presented recommendations for nation-wide action to insure the economic and social benefits that could and should be derived from well-managed forest lands. In 1936, the Forest Service issued a report on the Western Range. Another comprehensive statement was prepared by the Forest Service in 1940 for a Joint Congressional Committee on Forestry that had

been established in response to a special message to Congress from the President. Following a three-year study, the Joint Committee issued a summary report on *Forest Lands in the United States.*

The McSweeney-McNary Act of 1928—the act which provided a legislative charter for a broad national program of forest research—among other things authorized a

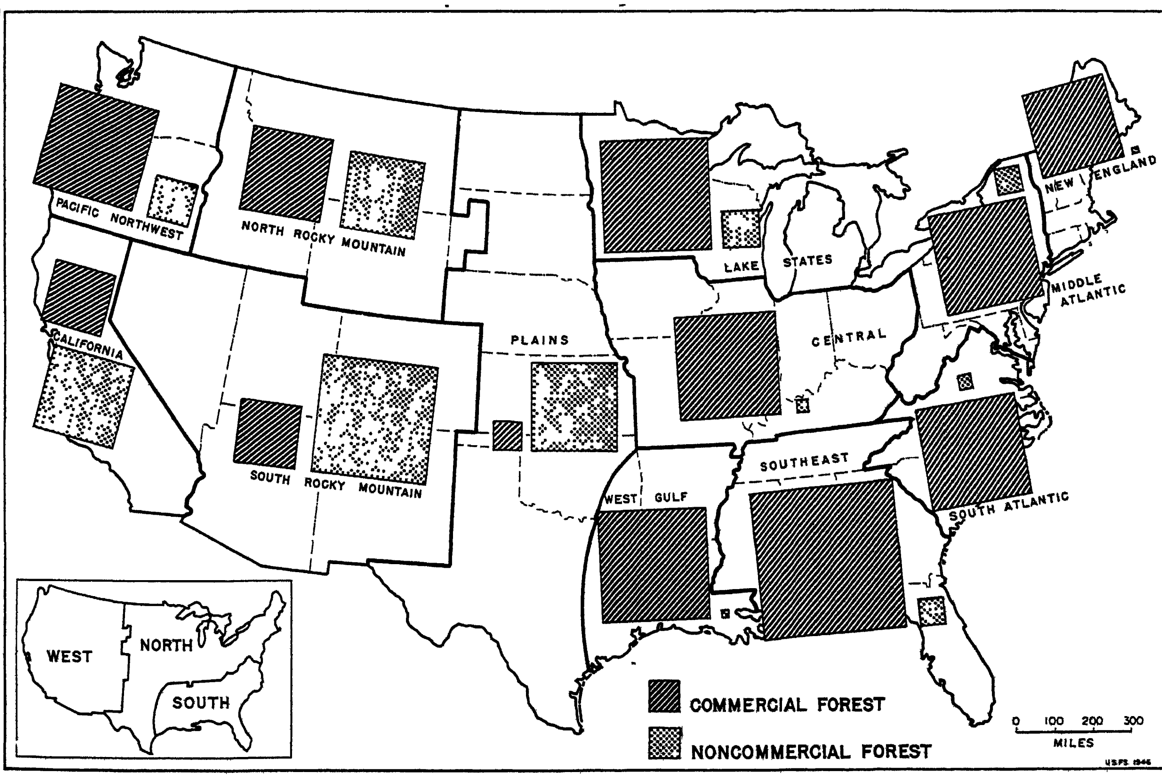
comprehensive survey of the present and prospective requirements for timber and other forest products in the United States, and of timber supplies, including a determination of the present and potential productivity of forest land therein, and of such other facts as may be necessary in the determination of ways and means to balance the timber budget of the United States .

Under this authorization, the Forest Service began in 1930 the first complete survey ever undertaken of forest resources and conditions on the nation's 631,000,000 acres of forest land. To date, initial field inventory work has been completed on nearly three fifths of the country's forest area. Since the war, resurveys have been made in several states where the initial surveys were made a decade or more ago or where there have been marked changes due to growth and cutting. Other phases of the Forest Survey have produced reports on present and prospective requirements for lumber for urban and rural housing, for shipping containers, and for several other classes of forest products. Completed and kept current, the nationwide Forest Survey eventually will provide comprehensive and reliable data on forest resources in every state and region. Such authentic information is essential both for the formulation of sound public forestry policies and for business decisions of forest industries and landowners.

Meanwhile, another big step toward providing what Congress called for back in 1876 has been made in a postwar reappraisal of the forest situation in the United States, recently completed by the Forest Service. With the close of World War II, the Forest Service undertook to check up on current trends, evaluate progress in forestry, and provide an up-to-date factual basis for national conservation objectives and policies.

I

The forest reappraisal was a huge fact-finding job. It involved checking and supplementing large amounts of information available from the Forest Survey and from other sources. Much new resource information also was obtained to assure an adequate summary of the quantity, quality, distribution, growth, and drain of the timber re-



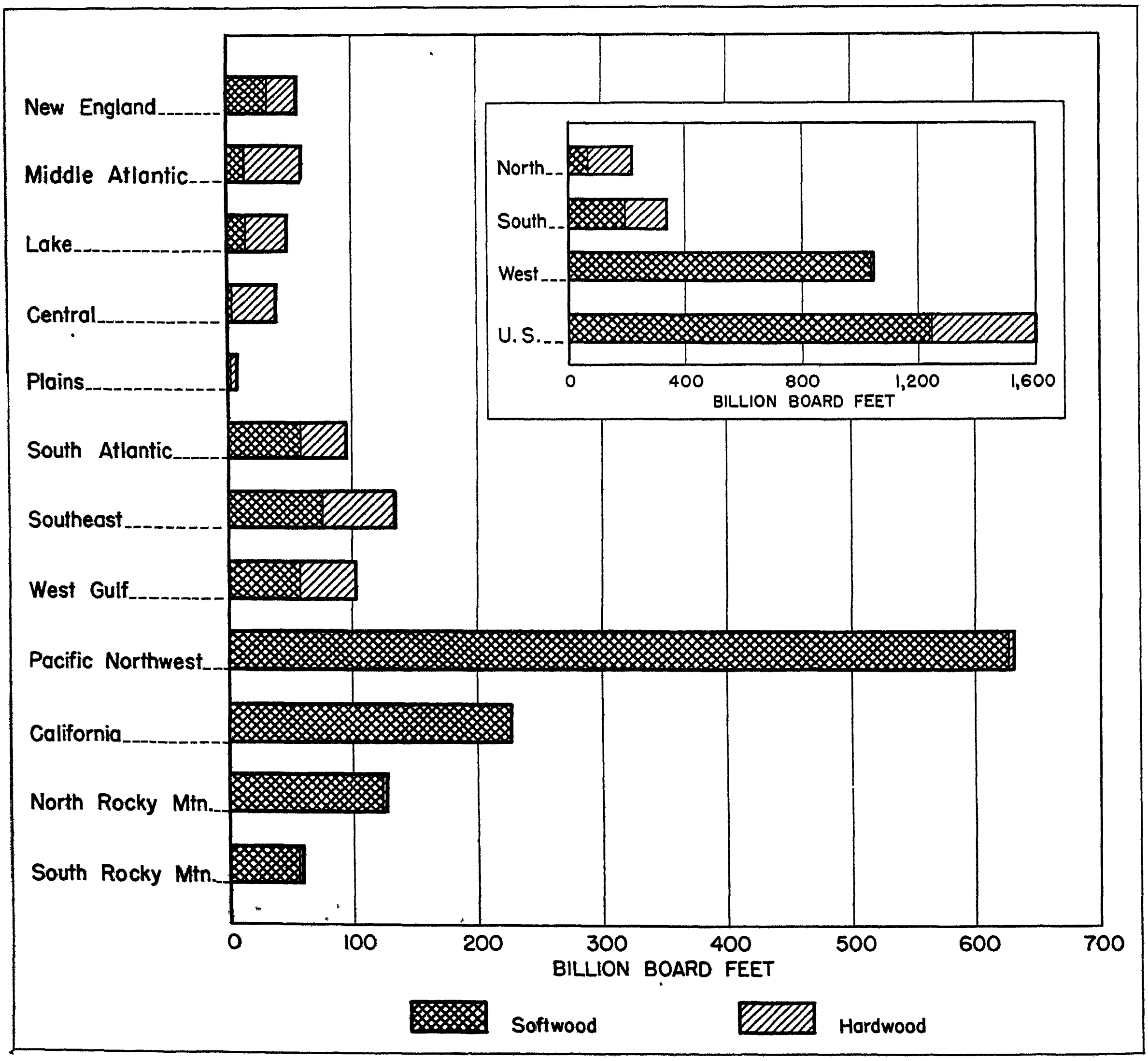
Distribution of the forest lands of the United States by regions.

sources. Estimates were made of potential requirements for forest products. Especially important new information on the character of forest practices and the intensity of forest management was obtained by a field survey. The volume and character of wood waste and the possibilities of using more of it were explored. Problems of the timber industries in relation to raw-material supplies were reviewed. The status and needs of forest protection were re-examined.

The reappraisal was conducted under the general direction of Raymond E. Marsh, Assistant Chief of the Forest Service. It was a Service-wide undertaking, with a large number of administrative and research personnel in the several regions and in Washington participating in one phase or another. The Division of Forest Economics and the regional forest and range experiment stations carried the main load. The Forest Service had the cooperation of other Federal agencies, state foresters and other state officials, and the American Forestry Association. Many private organizations and individuals contributed to the project.

An over-all report analyzing and interpreting the reappraisal findings has just been published under the title *Forests and National Prosperity*. Earlier, six reappraisal reports were issued, on the following subjects:

1. Gaging the Timber Resource of the United States.
2. Potential Requirements for Timber Products.



Saw-timber stand in the United States, by region, 1945.

3 The Management Status of Forest Lands
4. Wood Waste.
5. Protection Against Forest Insects and Diseases.
6. Forest Cooperatives.

The forests are invaluable in the protection of watersheds. They have important values for recreation, for wildlife habitat, and for production of livestock. The reappraisal, however, dealt mainly with the timber resource.

The reappraisal findings give us the best picture we have yet had of the timber situation in the United States. The situation is such as to give cause for concern. The nation's saw-timber supply is declining, and its quality is deteriorating. Yet indications are that our annual timber growth is below what we really need now and is far short of what should be available in the future for a strong, expanding economy.

There is enough forest land in the United States, if well managed, *ultimately* to grow all the timber products we are likely to need, plus a margin for unavoidable losses, new uses, export, and national security. But our forests are not now in condition to do this.

Of the 624 million acres of forest land, about 460 million are classed as "commercial" because they are capable now or prospectively of producing merchantable timber and are available for that use. But about 16 percent (75 million acres) of this commercial forest land is now so denuded or so poorly stocked that it must be classed as idle land. Another 180 million acres supports only pole timber or seedlings and saplings, fairly well stocked. Saw-timber stands cover 205 million acres, of which 160 million are second growth of varying quality, and 45 million are virgin stands. Of this remnant of virgin timber, only one fourth is of high quality; more than a third is of doubtful commercial value.

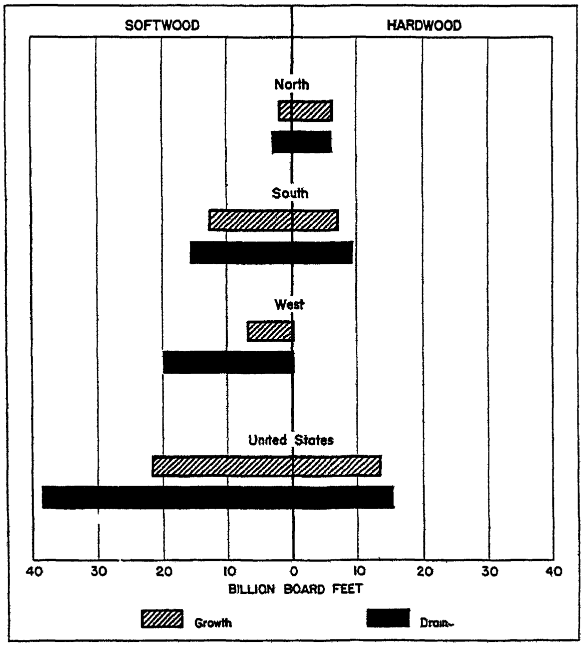
We now have less than 1,600 billion board feet of saw timber, and this is not well distributed. About one third is concentrated on the 6 percent of the commercial forest lands in western Washington and Oregon. The East, with three fourths of the commercial forest land area, does not have enough growing stock to sustain for long its present output.

The volume of standing saw timber in 1945 was 43 percent less than that estimated in 1909, and 9 percent less than in 1938. Just as important, the quality and size of the timber are deteriorating.

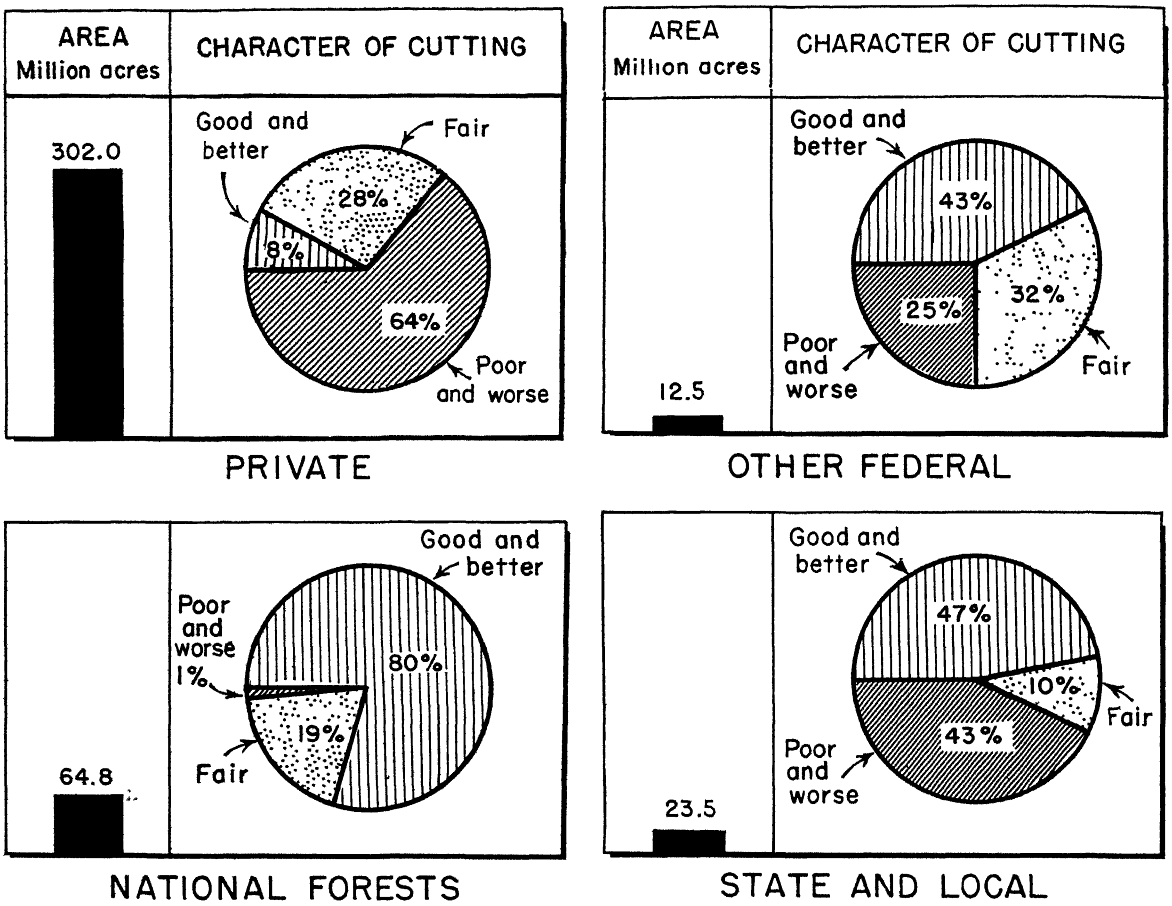
Saw-timber drain in 1944 was 53.9 billion board feet. It exceeded the estimated annual growth of 35.3 billion board feet by 50 percent. (Drain includes timber cut, plus natural losses; about 90 percent of the total drain is from cutting.) For all timber, including trees below saw-timber size and quality, the total drain was 13.7 billion cubic feet, compared with an annual growth of 13.4 billion cubic feet. But 80 percent of this drain is in saw timber, particularly softwoods, whereas much of the growth is in small low-grade trees and inferior hardwood.

Production of quality lumber and other quality saw-timber products has been affected by a growing scarcity of suitable, accessible timber. Shortage of high-quality timber has contributed to high lumber prices, which rose much faster than those of other building materials, and which undoubtedly are a deterrent to our using as much lumber as we should like to. There is much evidence that the intrinsic needs of the country for timber products are considerably greater than the present cut.

The forest situation thus poses a dilemma. To increase current output means an acceleration of timber depletion, especially in the East, that would hasten the day when drastic reduction in the use of timber products would be inescapable. To curtail Eastern output so as to facilitate building up growing stock and annual growth would leave urgent needs, such as that for housing, unfilled and would restrict the base for maintaining a high-level national economy. This country cannot rely



Growth and drain of saw timber, United States, 1944



Operating area and character of cutting by ownership class, 1945.

to any great extent on imports from other countries because there is a world shortage of timber, especially of softwoods for construction. There does not seem to be any wholly satisfactory solution.

To help maintain national output, the cut of virgin timber in the West can be increased for a number of years. This will require rapid construction of access roads into undeveloped country, particularly in the national forests. Any increase in cut in the Western states should not be at the expense of good forestry practice, if future growth is to be maintained.

From the national forests of Alaska, whose resources are as yet largely untapped, we can eventually get about 7 percent of the nation's potential pulp and paper requirements.

Utilization of some of the wood now wasted in harvesting and processing can help bridge the gap, though it cannot decisively relieve the pressure on our growing stock. At present less than half the weight of the wood we cut or destroy in logging shows up in finished products.

About one third of the nation's total volume of standing saw timber is now in the national forests, although the national forests comprise only 16 percent of the country's commercial forest area. Thus these public forests assume large importance in cushioning the decline in output of forest products which we face in the years before the needed productivity of the forests as a whole can be built up. Output of the national forests should be developed as rapidly as possible, but it becomes doubly important to safeguard their future productivity by keeping the cut within their sustained-yield capacity.

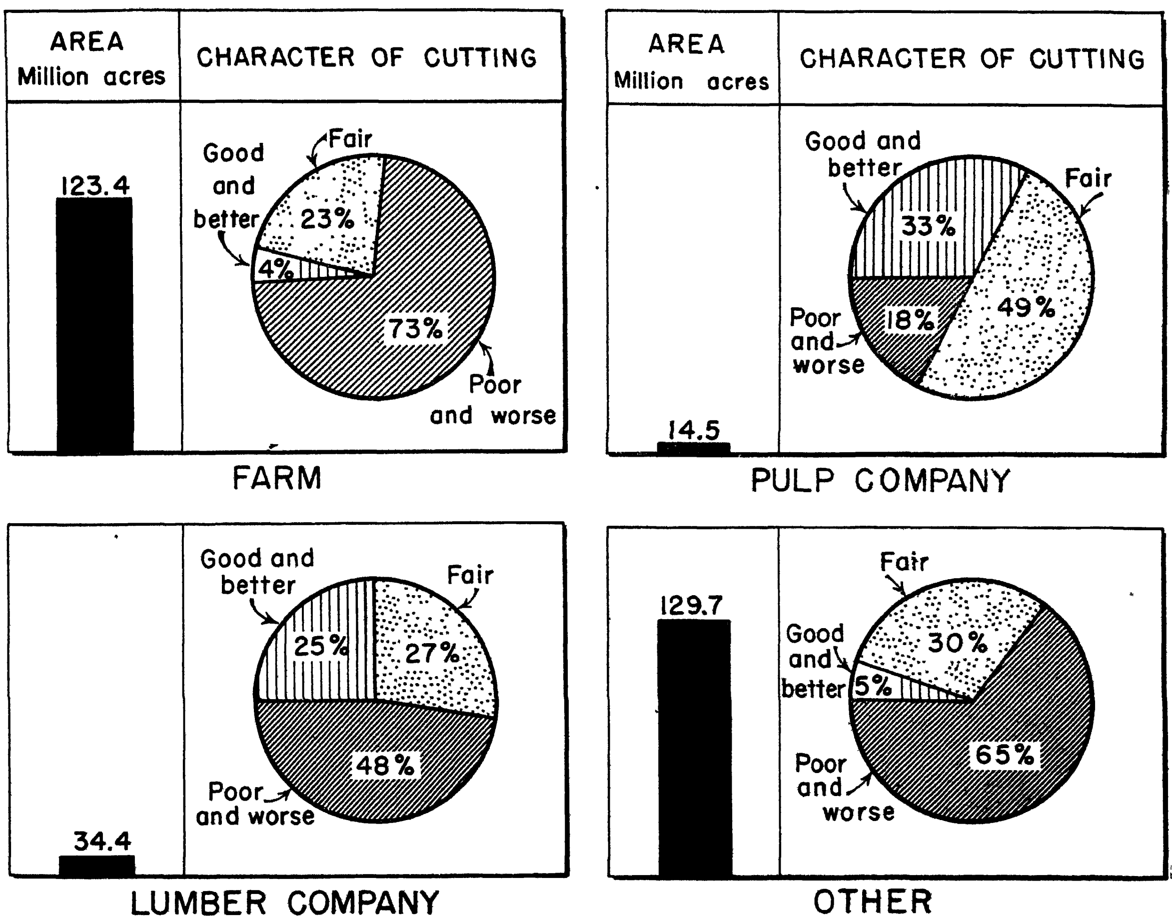
With only 16 percent of the country's commercial timberland, however, the national forests certainly cannot supply all our requirements for wood. We must depend mainly on the private land.

Timber-cutting practices on these lands, with some notable exceptions, are far from satisfactory

Encouraging improvement has been made in recent years, especially by some of the larger owners, but about two thirds of the cutting is still poor or destructive; only 8 percent is up to really good forestry standards. The larger properties, chiefly lumber- and pulp-company holdings, receive the best treatment. But these comprise only 15 percent of the private commercial forest land. Three fourths of it, about 261 million acres, is in more than 4 million small properties averaging 62 acres each. About half of these small forest properties are farm woodlands. On the small forest holdings, farm and nonfarm, about 71 percent of the cutting is poor or destructive. Improvement of forest practice on these millions of small private holdings is an especially difficult problem.

Careful study—looking beyond current limitations to long-range needs—indicates that 65–72 billion board feet would be a reasonable goal of annual saw-timber growth to meet prospective future requirements. That will mean doubling the current rate of saw-timber growth, which is a big order. Even if there could be prompt and widespread adoption of good cutting practices and other forestry measures—which only the most optimistic among us would expect—it would probably take at least until the end of this century to build up the needed growing stock. But to aim for less would not be sound public policy nor consistent with the responsibilities and needs of the nation.

In presenting the findings of the Forest Reappraisal, the Forest Service recommended certain measures that it believes essential to achievement of national timber growth goals. Although those measures are concerned primarily with timber production, they will go far toward meeting related needs—toward safeguarding watershed, range, scenic, recreation, and other values that, in some regions, transcend that of timber supply. Some of the measures call for new authority; others simply



Private operating area and character of cutting by type of owner, 1945.

require more adequate implementation of current activities. In brief, they are:

First, an extension of public aids to private forest landowners, including:

a) Technical advice and assistance to private owners in establishing and tending forests and in harvesting and marketing forest products, and corresponding advice and assistance to operators of small wood-processing plants.

b) A federally sponsored forest credit system to make long-term loans on terms and conditions suitable for forestry purposes.

c) Encouragement of forest cooperative associations as a means of achieving good forest management, particularly on small holdings.

d) Acceleration of forest planting on private land.

e) Extension and intensification of cooperative fire protection.

f) More prompt and adequate detection and suppression of incipient epidemics of forest insects and diseases.

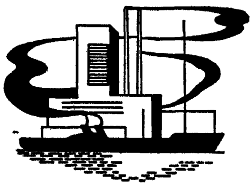
g) Advisory service to aid states in the improvement of forest tax laws and their administration.

Second, public control of cutting and other forest practices on private lands sufficient to stop destructive practices and keep these lands reasonably productive. It is just as important to protect the forests against destructive cutting as it is against fire. The Forest Service believes that adequate regulation could be accomplished by a Federal-state plan that would assure nation-wide application of the same standards but would give opportunity for state action and for Federal financial assistance.

Third, intensified development and management of the national forests. This includes among other measures, forest planting, timber-stand improvement, and access roads to open up hitherto inaccessible working circles and expand the contribution of national-forest timber to the nation's needs. Beyond this, substantial areas partly within existing national-forest boundaries either should be placed under public ownership and administration because they are submarginal for private ownership, or they should be acquired for other reasons of public interest, such as watershed protection. State and community forests also have an important part to play in public ownership and management.

An important corollary is that, commensurate with this country's growing responsibility in world affairs, we should work actively to encourage international cooperation in forestry. And to back up the needed program of action, we must of course have continued and intensified research in forestry. More research will be needed to make sure we have the best answers to the many problems of forest management, protection, and utilization, and to find the answers to problems yet unsolved. Through research we can find even better and faster ways of growing timber. We can find ways to reduce waste and make the timber cut go farther.

Our forest land area is capable of producing timber in sustained abundance. The challenge is to make it produce.



SCIENCE ON THE MARCH

FRACTURE OF LIQUIDS: NUCLEATION THEORY APPLIED TO BUBBLE FORMATION

California redwood and sequoia trees (Fig. 1) grow to a height of 300 feet, and the water that evaporates daily from their leaves must be raised this great distance from the soil. Water-carrying passages in the trunks of the big trees are relatively large; the capillary rise in them is a matter of inches. How, then, is the water lifted a hundred yards into the air?

One might suppose that water is forced to the tops of trees by pressure at the roots, as sap is forced from holes bored in the trunks of sugar-maple trees early in the spring. If root pressure were responsible, a pressure of about 10 atmospheres would have to be present in the sap at the base of a sequoia tree in order to support and lift a column of fluid 300 feet high. Were a hole bored in the tree, a strong flow of sap would be expected. Yet when such a hole actually is made at the base of a sequoia tree (or of any other tree in full leaf), nothing comes out of the opening. On the contrary, water poured into the hole moves into the tree and later appears in the leaves. The sap pressure at the base of the trunk is therefore *less than atmospheric,* and a mechanism other than root pressure must operate to supply the leaves with moisture.

Water that arrives at the tops of trees is in reality pulled up by the leaves. As moisture evaporates from leaf cell surfaces, cell walls resist the inward pull of their shrinking contents; the pressure of the water in the leaves drops. It becomes less than atmospheric, then less than zero, and finally reaches a large negative pressure. When the negative pressure in the leaves of a 300-foot tree exceeds 10 atmospheres, the pressure in the roots becomes negative also, and water from the soil diffuses into the roots. The upward motion of water in a tree therefore depends upon the presence of a sufficiently large negative pressure at the treetop to maintain a lower diffusion pressure of water in the roots than in the surrounding soil.

Only in early spring, when the soil is saturated and the diffusion pressure of soil water is high, and when—more important—there are no leaves from which evaporation can occur, does the sap pressure become positive. The sap flows from sugar-maple trees only after the spring thaw and before the first leaves appear.

Negative pressures are present in most plants that grow in air. For example, 10 atmospheres negative pressure in corn plants is frequently attained on dry sunny days. A hundred atmospheres of negative pressure have been measured in seeds. Were it not for the fact that water can withstand a large hydrostatic tensile force in the absence of free surfaces, most land plant life could not exist in its present form.

Liquids with free exposed surfaces are known to boil when their pressures are reduced below the vapor pressure, yet tree sap does not boil even at pressures far less than a vacuum. The difference lies in the difficulty of creating a bubble of vapor inside a liquid.

Figure 2 shows a small vapor bubble in a liquid under negative pressure. The liquid is contained in a rigid tank, and the negative pressure is maintained by a weight suspended from a frictionless piston. A definite potential energy change is associated with the formation of the bubble:

1. A surface is created between the bubble of vapor and the surrounding liquid. Work is required to form a surface against the force of surface tension which tends to shrink it out of existence, and the potential energy increases by the amount of this work.
2. The weight falls until the volume of the container behind the piston has increased by the volume of the bubble (less the usually negligible volume that the vapor in the bubble would occupy if condensed). The potential energy of the system decreases as the weight falls.

The total potential energy change associated with the formation of a bubble of radius r is plotted versus bubble radius in Figure 3. It can be seen that the formation of little bubbles having relatively large surfaces requires an increase in potential energy. On the other hand, a potential energy decrease accompanies the formation of larger bubbles which have relatively small surfaces. Bubbles of critical radius r^* require the greatest potential energy increase.

Does a bubble tend to grow or to shrink? The answer depends upon its size. Bubbles with radii less than r^* require energy for further growth, whereas those with radii larger than r^* grow with deceasing energy. Since bubbles grow larger or smaller one atom at a time as the result of sta-

tistical thermal fluctuations, it is evident that small bubbles with radii less than r^* usually will disappear without reaching the critical size. Only rarely will a long chain of favorable energy fluctuations produce a bubble exceeding the critical size in a liquid initially free of bubbles. When this unusual event does happen, however, the supercritical bubble grows with increasing rapidity; the liquid boils.

A cup of liquid contains about a million million million million molecules, each of which is moving around so that it hits its neighbors about 10 million million times a second. Tiny bubbles appear unavoidably from time to time here and there in the liquid, as the random dance of molecules continues. In time, a supercritical bubble will appear spontaneously in a liquid subjected to any given negative pressure.

FIG. 1. Giant redwoods soar 300 or more feet in the air.

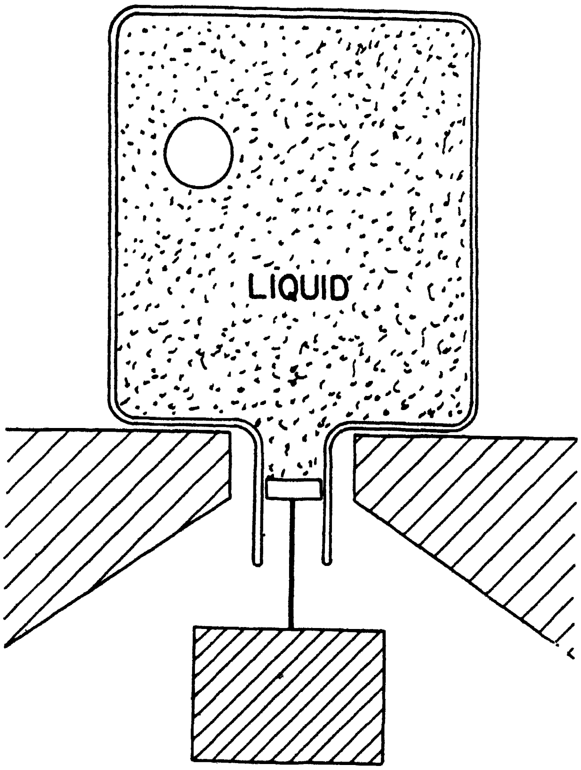


FIG. 2. Small vapor bubble in a liquid under negative pressure.

The theory of nucleation, which deals with spontaneous processes of this kind, gives an expression for the rate of formation of bubbles of vapor in a liquid subjected to negative pressure (assuming, of course, that somehow the pressure is maintained even after the first bubbles have begun to grow).

From this expression it is fairly easy to estimate the negative pressure at which a liquid will fracture (boil). Since the first bubble that forms fractures the liquid, the fracture pressure of the liquid will be that which gives one bubble in a reasonable time—say, one bubble a second. For water at ordinary temperatures the fracture pressure is 1,300 negative atmospheres. If the fracture pressure is computed for one bubble a year rather than one a second, the fracture pressure for water is reduced only to 1,180 negative atmospheres—still enough to lift water to the top of a tree 40,000 feet high.

Measured values of the fracture pressure of water range from the positive vapor pressure to a negative pressure of about 350 atmospheres. The highest experimental negative pressure that water has withstood is therefore only about 30 percent of the theoretical value. However, the theoretical fracture pressure was derived assuming that the vapor bubble responsible for fracture was formed in the interior of the liquid. The low experimental values suggest that the initial vapor bubble may form instead at the interface between the water and the container.

A vapor bubble at the interface between a liquid and a plane solid surface assumes a shape bounded by a plane and a portion of a spherical surface, as shown in Figure 4. In the figure, σ_{lv}, σ_{sv}, and σ_{sl} represent the liquid-vapor, solid-vapor, and solid-liquid surface tensions.

For certain values of the three surface tensions σ_{lv}, σ_{sv}, and σ_{sl}, it is much easier to nucleate vapor bubbles at a solid-liquid interface than in the body of the liquid. Although new liquid-vapor and new solid-vapor interfaces are created bounding the bubble, the solid-liquid interface is destroyed over that portion of the solid surface touched by the bubble. If the solid-liquid surface tension is high enough, sufficient energy is made available by destroying the solid-liquid interface to supply much of the energy needed for both the liquid-vapor and the solid-vapor interfaces of the bubble. Then little or no net energy is required for the formation of the bubble surfaces, and the liquid will fracture more easily. A tiny crack or other irregularity in the solid at the solid-liquid interface tends to re-

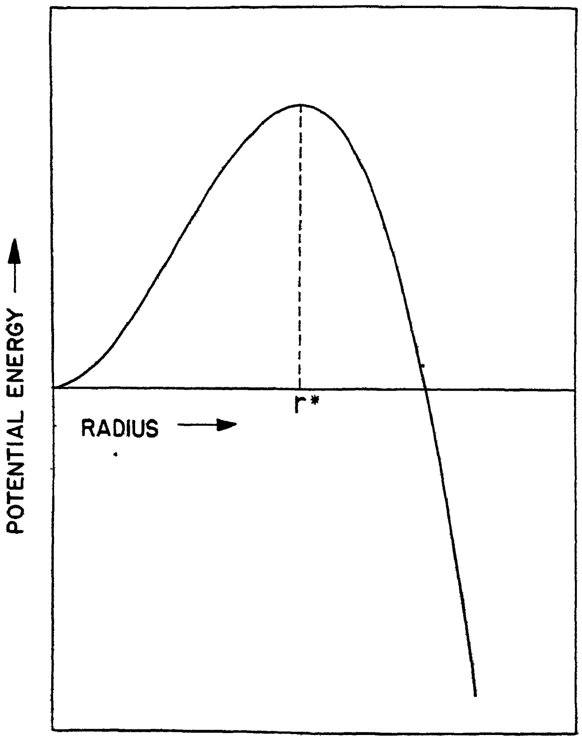


FIG. 3. Energy of forming a bubble of radius r versus bubble radius.

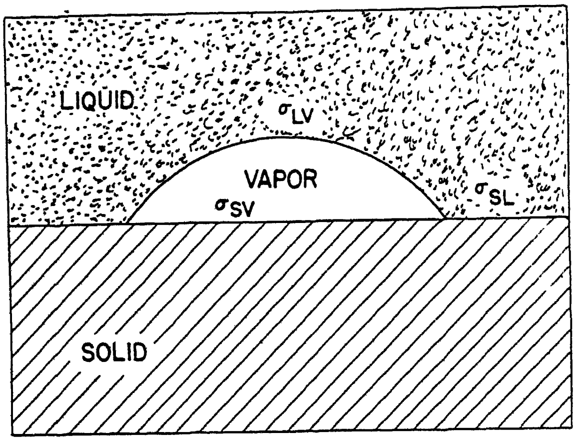


FIG. 4. Vapor bubble at interface between a liquid and a plane solid surface.

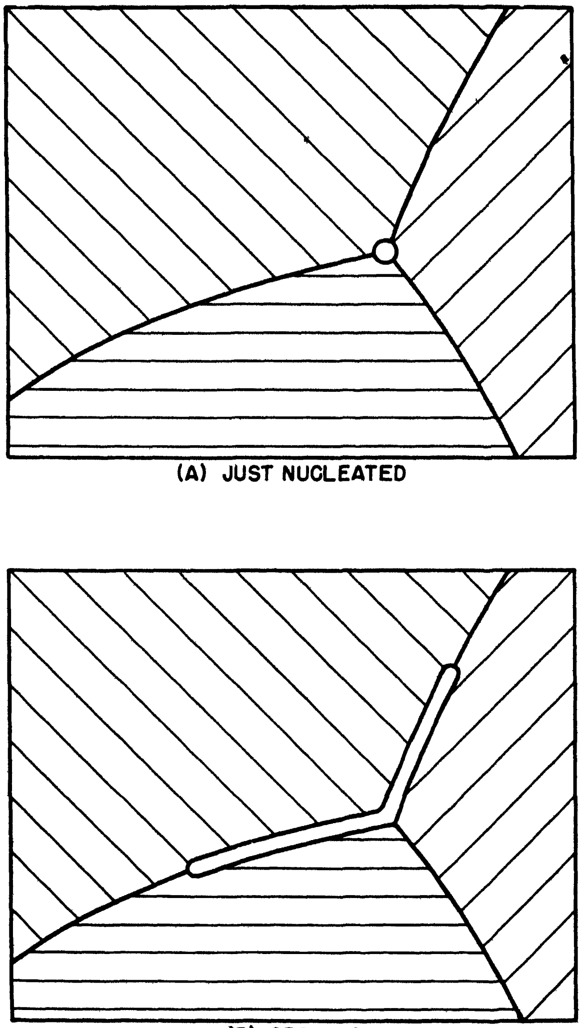
duce the magnitude of the fracture pressure still further.

Since the solid surfaces of the containers in which the fracture strength of water has been measured probably had widely differing values of σ_{sl} (a small spot of impurity may lead to a very large local value of σ_{sl}), one ought to find experimental fracture pressure values for water lying anywhere between the vapor pressure and the theoretical value for internal bubbles. The wide scatter of observed fracture pressures can be interpreted satisfactorily in this manner. The full 1,300 negative atmospheres can be realized only when the solid-liquid interfacial tension is sufficiently small.

Undercooled liquids (amorphous solids), such as glass, also will fracture when subjected to high negative pressures. Cracks appear in glass instead of bubbles as for liquids; otherwise the fracture mechanism is quite similar. The theoretical value of the fracture pressure for glass at room temperature is about 130,000 negative atmospheres, a value that checks quite well with the highest observed fracture strength of about 100,000 negative atmospheres for tensile tests of fine glass fibers. Most ordinary glass fails at very much smaller stresses by the growth of pre-existing surface cracks resulting from accidental damage.

When the fracture of liquids or amorphous solids is thought of as resulting from the nucleation and growth of a bubble or crack, a theoretical fracture strength value can be derived from the theory of nucleation. The theoretical strengths so found are, as they should be, larger than the highest measured values; yet they are not unreasonably larger. On the other hand, the theoretical strength calculated from the force necessary to separate planes of atoms in the absence of bubble or crack nucleation is an order of magnitude greater than that derived from nucleation theory; hence, there can be little doubt that such separation of planes of atoms is not the actual fracture mechanism for liquids or solids—the nucleation and growth of bubbles or cracks is a much more promising mechanism.

The fracture of metals and other crystalline solids is more difficult to analyze than the fracture of glass. Metals usually bend or yield before they break, and the regular patterns of the thousands of individual crystals or grains of which an average piece of metal is composed are distorted as deformation proceeds. Fracture usually originates as a crack in a region where a distorted crystal is subjected locally to a negative pressure far in excess of the average applied pressure. It is difficult



(A) JUST NUCLEATED

(B) GROWING

FIG. 5. Progress of a bubble along a grain boundary (schematic).

to estimate the magnitude of the stress concentrations that occur in severely distorted crystals, and the theory of fracture is incomplete for crystalline solids that deform before they break.

At very high temperatures, however, metals pull apart at the boundaries between grains. High temperatures give mobility approaching that of liquids to the atoms at grain boundaries, and a metal subjected to negative pressure fails by nucleation and growth of bubbles at grain boundaries, just as a liquid fails by nucleation and growth of bubbles at the container wall. The progress of a bubble along a grain boundary is illustrated schematically in Figure 5.

A marked decrease in the strength of metals is observed at the temperature where intergranular failure begins. According to nucleation theory, this decrease in strength is caused by a difference in fracture mechanism at high and low temperatures similar to that which requires the fracture strength of water to be one hundredth that of glass.

Engineers call the fracture of liquids under reduced pressure "cavitation." Cavitation seriously limits the design of ship propellers; in the presence of propeller surfaces and the surfaces of thousands of tiny marine plants and animals, the fracture pressure of sea water frequently is little less than the vapor pressure. Underwater sound generators cause cavitation where their vibrating surfaces are in contact with the liquid. Intergranular fracture of metals also could be called cavitation.

Prevention of the cavitation associated with engineering structures is difficult. However, there is a simple means for the prevention of cavitation

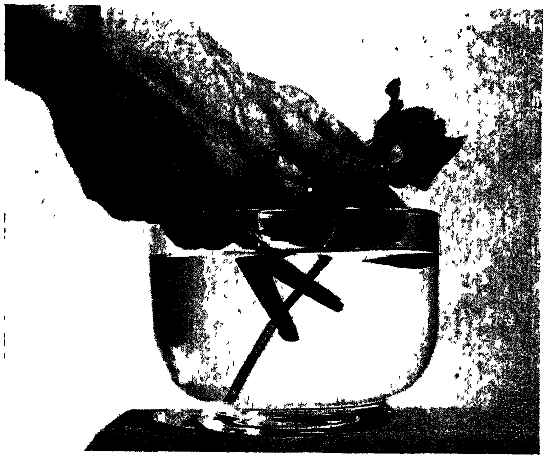


FIG 6. Prevent cavitation in flowers by cutting stems under water.

damage in an interesting nonmechanical system. Cavitation occurs in flowers that are cut when the water in their stems is under tension. Water columns initially under tension snap back into the stalk when cut, pulling in air behind them. Trapped air will block water passages and will cause wilting and death of flowers even when they have been placed with their stems in water. It is possible, of course, to prevent this difficulty by cutting the stems a second time under water, removing the length of stem containing air blocks (Fig. 6). Few cavitation problems can be as easily resolved.

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MINERAL SPIRITS AS SELECTIVE HERBICIDES IN CONIFER NURSERIES

A COMPARATIVELY new development in weed killers is the use of mineral spirits in forest nurseries as a selective herbicide. When applied as a fine, mistlike spray on seed beds of certain species of conifers they cause heavy weed mortality, with little or no damage to the tree seedlings.

These mineral spirits are of napthenic origin and usually have an aromatic hydrocarbon content of 10–20 percent by weight. The trade names of the petroleum products successfully used to date in conifer nurseries include Stoddard Solvent, Sovasol No. 5, Varsol, Stanisol, and Sohio weed killer.

This method has been used successfully in recent years to control weeds in carrots, parsnips, and in cranberry marshes. Heavier petroleum derivatives have given successful weed control in fields of guayule—a rubber-bearing shrub—and in citrus groves in California, but they have not given such favorable results in conifer seed beds.

When used in conifer nurseries, the mineral spirits are usually applied as a fine, mistlike spray using 25–100 gallons per acre (depending on species and age class) and pressures of 100 or more pounds. The application is generally made at full strength, although a few nurserymen do report using emulsions.

Most conifers tested to date appear to tolerate treatments with 50–80 gallons per acre without appreciable "oil burning" or mortality. Certain tree species, for example, white spruce just emerging from the soil in newly seeded beds, are subject to damage or mortality at higher levels of treatment; hence, conservative applications for four- to six-week-old white spruce appear to be in

the range of 25–35 gallons per acre. After this stage, white spruce will tolerate considerably heavier treatments. Larches are reported to be subject to heavy mortality, and spraying them with mineral spirits is not recommended.

The reaction of the mineral spirits on weeds and grasses is very rapid and striking. Within a few hours after application of the herbicide, the weeds begin to droop slightly. Most of them are dead and the foliage turns gray to tan in color after twenty-four hours. The percentage of weeds killed usually is in the range of 60–95 percent from dosages of 50–75 gallons per acre. Small weeds are easier to kill out than larger ones of the same species. Certain plants, mostly perennials, are difficult to kill out with the usual dosages; quack grass, clover, and Canada thistle are among these. Hence, a "mop-up" operation by hand weeding is often necessary after use of the mineral spirit sprays.

Even including the cost of supplemental hand weeding, the use of mineral spirits in forest nurseries has reduced weeding costs by 50–60 percent or more. Their value is greatest in first-year beds, when the small size of the trees precludes the use of mechanical cultivation for about six weeks after germination and when practically all weeding must be done by hand. The spray material usually costs from 20 to 25 cents per gallon, and costs of applying it are generally in the range of \$2.20–\$5.00 per acre if applied with power spray equipment. Costs per acre for spraying will usually amount to about \$20.00 per acre, with an additional outlay of \$4.00–\$12.00 per acre for supplemental hand weeding, bringing total costs to an average of about \$28.00 per acre. Weeding done exclusively by hand involves costs usually ranging from \$60.00 to \$100.00 per acre for a single weeding.

Experiments were conducted at the Hugo Sauer Nursery, Rhinelander, Wisconsin, in 1948 to work out some refinments in application technique with one- and two-year seedlings and three- to four-year-old transplants, indicating that the possibility of damage to buds and needles of trees may be reduced to a minimum by watering the beds by means of overhead irrigation immediately before application of the mineral spirit sprays. Spraying at night or in cooler weather, or growing the trees under shade, also reduced oil damage to the trees to an absolute minimum.

There is strong experimental evidence that the mineral spirits, especially in dosages of 75 or more gallons per acre applied in late spring or early summer, kill weed seeds which have soft, permeable seed coats and which are lying very near the soil surface. This clue offers the prospect of spraying beds several days before sowing the seed of the evergreens in late spring, thus keeping them fairly free of weeds in the critical one-month period after seeding. In the 1948 experiments at Rhinelander, the weed stand in beds of jack pine treated with 75 gallons per acre of mineral spirits had only 10 percent as many weeds as untreated beds thirty-two days after application of the sprays. All visible weeds had been pulled by hand just before the spray application.

The killing action of the mineral spirits is apparently quite different from that of the much-publicized 2,4-D products. Not much information of a fundamental nature appears to be available on the action of the former, and this phase of the work would seem to be an interesting field of investigation for botanists and plant physiologists

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NEW OPTICAL GLASSES

Before 1880 the only types of optical glass available to lens designers were the flint-crown series, in which the addition of progressively increasing amounts of lead oxide led to a progressive increase in both refractive index and dispersive power, so that in effect there was a fixed relation between the dispersive power and refractive index of all available glasses (Fig. 1).

In order to achromatize any lens, the positive elements must be of lower dispersive power than the negative elements, and in those days this meant that the refractive index of the positive elements had to be low, and that of the negative elements, high. This had two adverse effects: On the one hand, it tended to make the Petzval sum large, giving a strongly inward-curving field; and, on the other hand, it made the surfaces of the positive elements strong and those of the negative elements relatively weak. As the lens on the whole is positive, this resulted in considerable amounts of zonal spherical aberration, and, also, indirectly in large residuals of all the other aberrations.

For two reasons, then, a crown glass was needed with low dispersion and high refractive index and also, if possible, a flint glass of high dispersion and low index. The invention of barium crown glass

in the 1880s by Abbe and Schott helped enormously to meet the first requirement, but the problem of the low-index flint was not solved at that time. (Incidentally, plastics and liquids mostly tend to have higher dispersion for their index than glasses, but these materials are generally undesirable for other reasons.)

With the introduction of barium crown glass, many new types of photographic lenses became possible, the first to be developed being cemented triplets of the Dagor type, in which the three glasses are common crown, light flint, and dense barium crown, in that order. Barium crown glass also greatly improved all lenses of the airspaced type (the Cooke Triplet and the Celor, for example) by weakening the surfaces of the crown elements and enabling the designer to shorten the lens without adversely affecting the Petzval sum. To be sure, some successful lenses of the meniscus type—e.g., the Omnar—were designed without using barium crown, but these were the exceptions, and they did not become very popular.

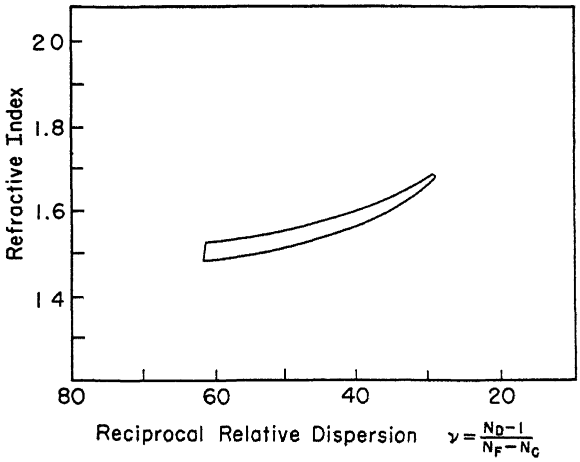

Fig. 1 Optical glasses (prior to 1880).

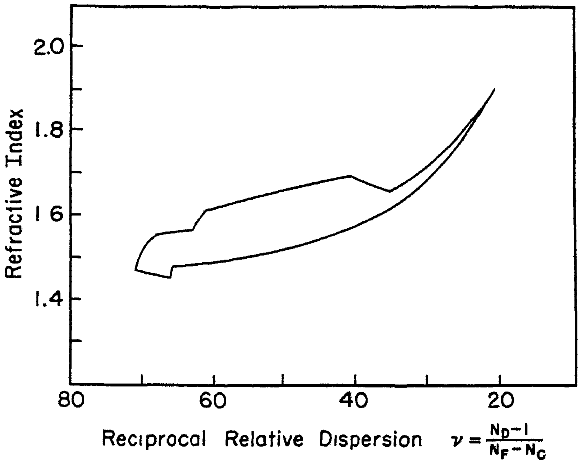

Fig. 2. Optical glasses (prior to 1934).

The refractive index of barium crown glasses was raised slowly and steadily by the Schott Company until the early 1930s (Fig. 2). At that time the company introduced SK-16 and SK-18, which marked the upper limit of the ordinary barium-soda-lime-silica types. These glasses are chemically unstable and present difficulties in lens manufacture because of their susceptibility to acid staining and their brittleness.

Following the first world war, C. W. Frederick, chief of the lens design department of Eastman Kodak Company, discussed the advances that were needed in optical glasses with G. W. Morey, who had done work of great value in the production of optical glass for military purposes. Frederick suggested that what was wanted was a very high refractive index with low dispersive power and that even a small production on a laboratory scale would be useful. Morey thereupon undertook to study the optical properties of glasses in relation to chemical composition, especially with a view to the production of high-index crown glasses. To this end all high-atomic-number cations were chosen for systematic study in silicate, borate, and phosphate glasses. Small melts of 20–40 grams were made to indicate the field of glass formation. The more promising ones were repeated in larger melts of 50–100 grams.

By 1933, the work had progressed to the point where silicon and phosphorus were discarded as glass-forming elements. Boric oxide had by now proved to be by far the best fluxing agent. Oxides of elements such as lanthanum and thorium, found in the rare earths, and columbium, tantalum, tungsten, titanium, zirconium, and strontium were used in major portions up to 80 percent by weight, with or without the usual barium, zinc, magnesium, and aluminum.

In 1934, samples of unusual glasses in the region with an n_D of about 1.85 and a ν of 43.0 were in existence and their properties well measured.[1] About this time the work was expanded to a larger scale by the Kodak Research Laboratories. Under the direction of S. E. Sheppard, a systematic study of the solubility of the rare elements in boric acid and of the limits of glass formation was made by L. W. Eberlin and P. F. DePaolis.[2]

The solubility of lanthanum in boric acid is remarkable, and its contribution to higher refractivity without increase of dispersion is a revelation. The oxides of tantalum, thorium, and tungsten are soluble in the lanthanum borate base glass

in amounts up to 35 percent. These new borate glasses are very stable and fairly hard. They are harder than flints, suitably stable to the atmosphere, and amenable to optical shop practices of molding, grinding, and polishing.

Early in the development work it was found that the new rare-element borate glasses were extremely corrosive to all known pot refractories. A decision was therefore made to use platinum for the actual production of these glasses. This was justified on the basis that no platinum would be lost by contamination and that the glass once homogenized could be poured in its entirety, free from striae, into a single slab or into cast shapes without striae, seed, bubbles, or other defects usually attending a glass made in a refractory pot.

The first of the new glasses to be made had a refractive index of 1.7445 and a ν-value (reciprocal dispersive power) of 45 8. The glass was slightly yellow, but further work established the origin of the yellow color, and finally glasses were produced as colorless and homogeneous as any other ordinary optical glass.

Pilot-plant operation began in September 1937, and the first commercial glass was delivered in June 1939. Production increased rapidly, and more than 125,000 pounds of rare-element glass were produced during the second world war (1942–45). Much of the success of the enterprise was due not only to the platinum equipment but to the Kodak method of using all-electric heating and a small-pot, "multi-pot-multi-stage" process.

Under sustained production conditions during the war, the yield of finished usable glass in a cast form was 95 percent of the theoretical glass available in the batch.

Electric heating was retained in a plant that was erected during the war and operated from December 1942 to September 1945 on a continuous basis of approximately 5,000 pounds of finished glass a month. The process consisted of feeding thirty-two 10-pound platinum-lined pots every twenty-four hours, starting one every three quarters of an hour and progressively moving these pots through the various stages of the glassmaking process.

Since 1940 the types of these glasses in production have been extended, and at the present time seven types are being made (Table 1).

All these glasses contain thorium, and in the case of folding cameras, in which the lens may rest for a long time in close proximity to film, the radioactivity of the thorium may be a disadvantage. A thorium-free equivalent of EK-320 is available.

The new glasses were first used in lens design in 1934, actual production of lenses began a few years later, and today many of the Eastman Kodak "Ektar" lenses contain the high-index glass. The versatility of these glasses is evidenced by the large number of lens patents that specify such glasses.

After the transfer of the production of the new glasses to the factory, the Kodak Research Laboratories continued their study of optical glasses. Theoretical considerations by M. L. Huggins and K. H. Sun[3] indicated the possibilities of further new glasses, and various new fields were studied. Of the glass systems of this type which were worked out by Sun, the most useful were flint glasses containing titanium oxide and using fluorine in addition to silica.

These unusual glasses lie well below the ordinary flint line (Fig. 3). For a given index, the dispersion is appreciably greater than that of ordinary flints. In this sense the glasses may be termed superflints. The best glasses lie in the region 45 percent SiO_2, 28 percent TiO_2, 27 percent NaF, ranging in optical properties from $n_D - 1.65/\nu - 29$

TABLE 1

	n_D	ν
EK-110	1.69680	56 2
-210	1.73400	51.2
-310	1.74500	46.4
-320	1.74450	45 8
-330	1.75510	47 2
-450	1 80370	41.8
-448	1 88040	41 1

to $n_D - 1.58/\nu - 36.6$. The glasses are moldable, very resistant to tarnish, and easily fabricated by usual optical methods.

The fluosilicate flints are almost as useful to the optical designer as the high-index glasses, since they extend the possible difference between crown and flint index and between crown and flint dispersion. With these glasses three-element lenses have been designed to give even better performance than the usual four-element types. Later, K. H. Sun succeeded in producing novel mixtures in some twenty quite different glass fields, including fluoborate, fluogermanate,[4] and fluophosphate systems.

A most interesting group of glasses suggested by Sun are those containing no oxides and composed entirely of fluorides. These glasses show the characteristic low refractive index and extremely low dispersion previously available only in fluoride minerals. The refractive index in most of the glasses approximates 1.38–1.39, and the ν-value,

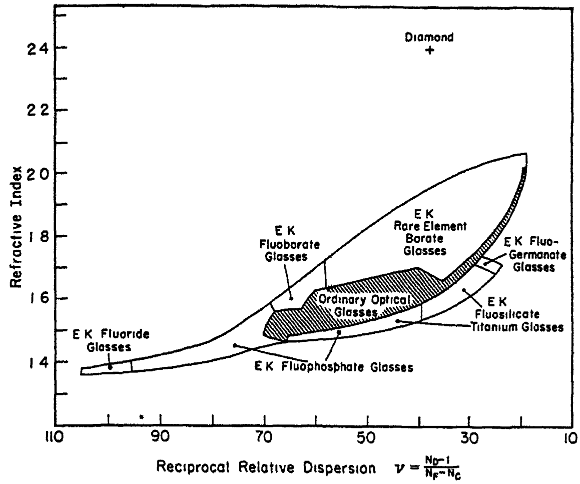

FIG 3 Range of optical glasses before and after 1934

100. Moreover, the glasses are transparent to below 300 mμ in the ultraviolet and to 5 μ in the infrared, so that they may be very useful in the making of instruments requiring optical transparency over a wide range of wave lengths. Difficulties have been met in the production of these all-fluoride glasses, but it is possible that these difficulties may be overcome in the near future.

The extension of the frontiers of optical glass by this work is illustrated in Figure 3. In this figure the range of optical glasses known before 1934 is shown crosshatched; the larger area shows the glasses that can be made at the present time.

REFERENCES

1. British patent 462,304, March 3, 1937; U. S. Reissue patent 21,175, August 15, 1939.
2. U. S. Patents 2,206,081; 2,241,249; 2,434,146; 2,434,-147; 2,434,148, 2,434,149.
3. HUGGINS, M. L. and SUN, K. H. *Amer. Ceram. Soc.*, 1944, **27**, (1), 10–12, 13–17.
4. U. S. patent 2,425,403.

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ATOMIC CLOCK

Once time was reckoned only by the sun,
Held in obedience to its golden strength,
By which a day was ended or begun:
We gauged the morning by a shadow's length.

And then we gathered time into a glass,
The moments falling with the sifting sand;
A man could all but see the seconds pass
And grasp a minute in his naked hand . . .

Until we found that in a tiny spring
Time coiled, and waited only to be free,
And so a pendulum began to swing
To beat the tempo of eternity.

Yet still approximate . . . no accurate chime
Before we tricked the atom's secret power;
But here at last is such exquisite time
A million years won't swerve it by an hour.

MAE WINKLER GOODMAN

BOOK REVIEWS

SOCIAL HISTORY

The Family: Its Function and Destiny. Ruth Nanda Anshen, Ed. xi + 443 pp. $6.00. Harper. New York.

THE aim of this volume, to create a synthesis and understanding of the family, is conspicuously not achieved. The symposium is an aggregation of twenty quite independent papers, each of which may be worth while in its own right, but which contributes little or nothing to an integrated understanding of the family.

In the first chapter the editor pictures the American family as "moving with precipitous speed to greater atomization and destruction" but concludes by expressing the vague hope that "by manifesting a confidence and faith in absolute truth as it should be conveyed through the family in history, man may become willing to accept his historical responsibilities even though his historical strivings involve the deepest suffering and tragedy" (p. 17).

The book is divided into two parts: "Patterns" and "Structure." Family patterns are depicted as they are in Islam, China, India, Russia, and in Latin America, concluding with the Negro family and the American family. This last chapter on The Family: *Genus Americanum,* by the late Ruth Benedict, is a most significant contribution. She points out what everyone, except anthropologists and some sociologists, fails to perceive—namely, that the family in the United States is not in transition to disintegration but to a new form uniquely adapted to the American way of life. As compared with families in primitive, historical, and contemporary societies, American young people have freedom to select a mate by personal choice, a wide range of choices after marriage "about where to live, how the wife shall occupy herself, when to start a family, and a host of other important matters" (p. 112). Also, in this country the young couple have "an incomparable privacy," and "unusual potential leisure because of labor-saving devices, prepared foods, and ready-made clothes." Dr. Benedict emphasizes the correspondence of the distinctive non-authoritarian attitude of parents with the values stressed in our society. She concludes that "the family in the United States is an institution remarkably adapted to our treasured way of life. . . . Americans, in order to get the maximum happiness out of such a free institution as the family, need to parallel their privileges with an awakening responsibility" (pp. 168–69).

A miscellany of chapters is grouped under the heading "Structure": the social structure of the family, its emotional structure, social structure and anomy, the facts of life, education, housing, the crisis of the modern couple, the Oedipus complex, authoritarianism and the family, sex and character, religious values, and the family as conveyance of tradition.

The chapter by R. K. Merton on social structure and anomy is a brilliant analysis of differential reactions of Americans to our approved goal of success by conformity, innovation, ritualism, retreatism, or rebellion; but the relevance of his discussion to the family is only briefly stated. Denis de Rougemont asserts that the crisis of the modern couple arises from the failure of passion and romance as the sole basis for happiness in marriage and pleads for a new social realism, apparently unconscious that this trend is actually under way.

The concluding chapter by the editor signally fails to utilize the contributions to the symposium. She falls back instead upon a philosophical platitude: "The pursuit of truth, again established as the central purpose of society, will lead by its nature to the enjoyment of the good, the love of beauty, justice, and the reintegration of family life."

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MILITARY RESEARCH IN BRITAIN

Science at War J. G. Crowther and R. Whiddington. 185 pp. Illus. 2*s*. 6*d*. H. M. Stationery Office. London. (American edition: iv + 185 pp. Illus. $6 00. Philosophical Library. New York.)

MOST scientists in university laboratories before the war were engaged in "academic" pursuits far removed from engineering applications. Yet these same scientists during the war turned, almost overnight, to engineering investigations and, permeating them with the spirit of modern science, were able to turn out veritable miracles of engineering production. In these war research laboratories there was a peculiar spirit of adventure, detective work, bold imagination, and daring, all tempered and guided by expert and logical mathematical reasoning. In this narrative, released last spring in Great Britain by the Department of Scientific and Industrial Research, through the Scientific Advisory Committee to the Cabinet, the authors try to recapture this spirit of the war research program.

The Advisory Committee selected as authors J. G. Crowther, chairman of the Association of British Science Writers, and R. Whiddington, F.R.S., head of the Department of Physics at Leeds University and one of England's best-known physicists.

The large amount of work and the diversity of subjects have made it necessary to concentrate on a few selected chapters. Almost half the total of 185

pages are devoted to radar, which was perhaps the most striking and effective contribution of British scientists to the war effort. Another 30 pages are devoted to operational research, a new technique introduced into warfare by the British. This is followed by a brief discussion of about 30 pages on the atomic bomb and, finally, by some 60 pages of selected topics under the title Science and the Sea. Britain, being a seafaring nation, had to solve a great many problems in this field which, to such an extent, did not exist for some of the other countries. Discussions such as Detecting Submarines by Various Methods, Sinking the Detected Submarine, The Magnetic Mine, and some of the others, are masterpieces of "case history" in the application of the scientific method, and every science teacher will profit from their study.

The chapter on the atomic bomb gives an excellent popular account of the science of radioactivity and nucleonics.

In the historical outlook on the atomic energy project, the authors, quite rightly, say:

> Through Rutherford and the British school of atomic physics, they made the major contribution in the sphere of physical knowledge of the atom. The British contributed the whole of the intellectual inheritance in nuclear physics to the pool. They contributed developmental work, but in Britain no plant for producing atomic bombs or for releasing atomic energy existed. Her physicists were dispersed and her engineers almost entirely engaged in other things.

Operational research was an entirely new feature, for the first time introduced into the techniques of war by the British. It was made possible because of the understanding for this necessity by such scientific leaders as Blackett, the famous cosmic ray physicist, and Mott, equally famous for his contribution of the theory of solid-state and quantum mechanics. It is interesting to read that this group of physicists, physiologists, mathematicians, etc. "apparently established its title of Anti-Aircraft Command Research Group by making a rubber stamp with the initials AACRG. It is not known whether any more formal authorization or recognition was ever obtained. The group became known as 'Blackett's Circus.' "

"The directors of operation research are generally civilian, and one reason why war in the future will tend more and more to be conducted in a civilian spirit. One reason why Hitler failed is that he was out of date" (p. 120). In its own press release, the Department of Scientific and Industrial Research says:

> The section on getting more out of aircraft illustrates the methods now being used to get more out of boilers and retorts. In the cotton industry, research has already been shown the way in which an overall increase in production of 20% can be obtained, and in the case of a few firms, can be doubled. Similar results are coming to hand in other industries.

The chapter on Radar is a classic example of how the scientific approach and the scientific method can be explained to the layman. A number of excellent drawings and diagrams, accompanied by a series of first-class photographs, both of equipment and of actual radar screens, and of the type of results achieved, are almost indispensable to anyone who wants to understand how it was possible, in an incredibly short time, to translate the knowledge of advanced electrodynamics into engineering practice and apply it to problems of extreme complexity.

> Major scientists such as Cockroft, Dee, Lewis, Oliphant and Skinner [all known to anyone who has ever worked in nuclear physics] helped to conceive military scientific problems of the most profound scientific point of view. . . . The great contribution of these men were the demonstrations of the fundamental importance of exact research. Rutherford was often criticized for training "Atom Smashers" who did not appear to be of any practical use to the nation. How right *he* was. His own nuclear physicists and their colleagues turned to radar, and in a few months helped to revolutionize it [p. 87]. As a comparison with the situation in Germany, we learned that in 1940 the German General Staff believed that they had won the war. They issued an order that no scientific research or development should be pursued which would not be of military use within four months. They drafted scientists into the armed forces to assist in the invasion of Britain. They did not realize that by the end of 1942 new scientific developments would be necessary if they were not to lose the war.

The authors have succeeded in bringing to us not only a picture of the war research and the spirit which is in the British Laboratory, but they also realize, to quote Sir Henry Dale's closing words in his preface:

> The use of science as an aid to war is a perversion from its proper purposes, and its rapidly extending misuse in the recent war, as a direct agent of violence and destruction on a stupendous scale, creates a threat to the survival of civilization. Meanwhile we may find some reassurance in recognizing that much of the discovery and invention which came so rapidly to hand, in response to the tremendous stimulus of the recent war's demands, will find immediate and beneficent use in peace.

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MEDICAL RESEARCH

Racial Variations in Immunity to Syphilis. Chester North Frazier and Li Hung-Chiung. xii + 122 pp. Illus. $2.50. Univ. of Chicago Press.

IS THE response to syphilitic infection manifested differently by Chinese, white, and Negro races? The senior author of this unusual little book reminds us that twenty-five years ago the belief was prevalent that the Chinese response to syphilis was different from the response of Europeans and Americans. This belief was not based on extensive studies of syphilis among Chinese. It was probably based on the old misconception that the manifestations of disease are bound to differ among different races since the mani-

festations of health also differ among them. With the fuller recognition that all peoples are basically the same biologically, the senior author well points out that differences in manifestations of disease among different races are more commonly due to environmental factors than to racial factors.

The book is unusual because the undertaking to collect data on Chinese patients in the Department of Dermatology and Syphilology at Peiping Union Medical College and on white and Negro patients in the Johns Hopkins syphilis clinic in Baltimore is unique. The authors were well qualified to carry out this extraordinary task, based on their extensive medical experience both in China and in the United States. It is particularly fortunate that they recorded their data dealing with the Chinese patients at Peiping. For the war has brought about marked environmental changes in China, and it will undoubtedly take many years before it will be possible again to undertake such careful medical studies among the Chinese.

The authors deal primarily with environmental and racial factors which influence immunity in syphilis. But their discussion reaches out beyond these factors and extends to those affecting health and disease in general. The book therefore should be of interest to both physicians and biologists. In addition, it is written in a popular vein and is exceedingly easy to read. This book makes an excellent companion to the classic little monograph on *Immunity in Syphilis*, by Alan Chesney, published in 1927. Additional monographs such as these two are needed for a more complete understanding of the multifaceted aspects of immunity in syphilis.

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ANIMALS, PLANTS, ROCKS, AND STARS

Fieldbook of Natural History. E. Laurence Palmer. x + 664 pp. 2,000 illustrations. $7.00, leatheroid binding; $5.00, cloth. Whittlesey House. New York.

THE encyclopedic format of this book gives a false notion of its contents. It "is not a textbook in botany, zoology, geology or astronomy . . . nor is it a manual for the identification of most of the objects considered in those sciences." Rather it is a stylized listing and illustration with concise comments on "the things that have interested" Professor Palmer, "his students, and his friends in more than a third of a century of teaching field natural history from New England to Hawaii." In addition to details about stars, rocks, and miscellaneous plants and animals, the *Fieldbook* includes information on domesticated and economically important plants and animals, about "cows, corn, cod and chickens," and various kinds to be found as ornamental vegetation and on display in zoological parks. Some of this material has appeared as special inserts in *Nature Magazine*.

Eighteen pages are devoted to astronomy, with specifications of magnitudes, periods, colors, distances, and positions of stars visible in the Northern Hemisphere, and with tabular data on the planets and their moons. Another 18 pages summarizes the mineral kingdom, with photographs heading triple-column pages, as in the rest of the book. One picture, one mineral, one column, is the uniform treatment accorded, mostly in agate (5½ point) type.

Over 300 pages survey the plant kingdom, with 940 kinds treated, each with a clear line drawing emphasizing general appearance, details of sori, buds, fruits, etc. Of these 33 are algae, 99 fungi, 15 bryophytes, 30 pteridophytes, 33 gymnosperms, 148 monocots, and 582 dicots. The selection is a curious assortment and, despite obvious efforts to the contrary, centers on the native and introduced flora of New England. Some may object to a column being given to the ornamental Mugho pine, whereas Western yellow pine, whose groves grace the West and whose wood builds modern houses, is accorded 4 ½ lines. Tropical plantain and breadfruit trees receive a column each, though users of the fieldbook are less likely to see them or to use their products. The colors of a number of flowers go unmentioned, including Nevada's state flower, the common sagebush.

Another 100 pages includes 955 kinds of animals, all but 27 of them mollusks (118), arthropods (198), or chordates. The photographs of clam and snail shells against a black background are less informative than the line drawings elsewhere. Amphibian egg characteristics, snake scale patterns, bird foot forms and egg details, mammal tracks and hair structure, are very helpful additions. Mammalian dental formulae, gestation periods, postembryonic schedules in many instances, dietary analyses, and in some cases body temperature, pulse, and respiration rates are cited where available. A full index completes the book, with common and generic names alphabetized, species under the appropriate genera.

Plant and animal classification is given inconspicuously in the text itself, to genus and species of each form discussed. Although supposedly exact, this practice may lead users of the book to assume that they can use it to identify species, even though no keys are provided and no claim to completeness is made. In several instances, as in the case of the carrion beetle and log-cabin caddis fly, the generic name follows the not uncommon misspelling. The horned toad illustrated and described (*cornutum*) is stated to be viviparous; actually it is oviparous, though other species of the genus do bring forth young alive. Here oviparity is less frequent than viviparity, but the specific name makes the statement wrong. Eight races of dogs receive a column apiece. Man is accorded a page of text and one of illustration, but no discussion of human races is included.

The book will have a useful place on the reference shelf, to be consulted in connection with reading, field trips, and visits to botanical and zoological

gardens. The pages reflect Professor Palmer's wide experience and his consistent interest in conservation.

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MAYA CULTURE BEFORE THE CONQUEST

The Maya Chontal Indians of Acalan-Tixchal: A Contribution to the History and Ethnography of the Yucatan Peninsula. France V. Scholes and Ralph L. Roys, with Eleanor B. Adams and Robert S. Chamberlain. v + 565 pp. $3.50 paper, $4.75 cloth. Carnegie Institution. Washington, D. C.

STUDENTS of Maya culture and of Middle America as a whole will welcome this scholarly and exciting study of the Chontal-speaking people of the pre-Hispanic province of Acalan, located in the southwestern porton of what is today the Mexican state of Campeche. The region of Acalan has been one of the least-known regions of the Maya culture area, both archaeologically and ethnographically. Prior to the present study there was little agreement even among specialists as to the precise location of this important province, not to mention the lack of knowledge concerning the people, their language and customs.

The book is based upon new documentary materials obtained by Drs. Scholes and Chamberlain in the *Archivo General de Indias,* Seville, Spain, during the thirties. The documents consist of the correspondence of colonial officials, missionary reports, law suits, administrative decrees, and other items. Perhaps the most interesting is the unique Chontal Indian text, the only one we have from the sixteenth century in this language. This text gives the history of the rulers of Acalan, going back six generations before the Conquest, lists the towns which comprised the province, and describes from the native point of view the arrival of the Spaniards under Cortes and the later Spanish activities in the area to 1602. From these documents the authors have reconstructed the aboriginal social, economic, political, and religious conditions in this area and have traced the effects of the Spanish Conquest through the seventeenth century.

The picture of Acalan before the Conquest, as it emerges from the documents, is that of an active and prosperous population of traders and merchants. Indeed, it is one of the important contributions of this study to show that the amount and extent of commercial activities in pre-Conquest times was much greater than in the later Colonial period. Trade relations extended across the Yucatan Peninsula from Tabasco on the west to the Caribbean and the Ulua River of Honduras to the east. Salt, cotton cloth, and slaves were exported from Yucatan in exchange for copper tools, cacao, feathers, gold, and other items. The authors conclude that the entire area from western Tabasco to the Ulua River was ". . . an economic block which, in spite of its political diversity, can be considered as a single commercial empire" (pp. 316–17).

The effects of the Spanish Conquest appear to have been disastrous. From 1530 to 1553 the Acalan population declined by about 60 percent, trade languished, and the people were reduced to subsistence agriculture in a poor land area. An especially severe blow to the native culture was the forcible resettlement of the natives to another area deemed more suitable by the Spanish priests. The authors conclude that disease and the disruption of the native economy were the two major factors in the population decrease. In this connection they make the interesting suggestion that malaria may have been introduced by the Spanish.

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CIVILIZATION BY GEOLOGICAL CONSENT

Sedimentary Rocks. F. J. Pettijohn. xv + 526 pp. $7.50. Harper. New York.

EIGHTY-FIVE percent of the dry-land surface of the earth, and much of the sea bottom, are made of sedimentary rocks, of which sandstone, shale, and limestone are familiar types. A thorough knowledge of these rocks is an obvious necessity, not only because of their practical importance but because their bedded, or "layer-cake," arrangement gives us the framework for interpreting earth history. Yet there are only a handful of books that cover the nature and origin of these rocks in a comprehensive fashion. The present volume is an important addition to this small collection.

More than half of the book is given to a description of the mechanical and chemical composition of sedimentary rocks; this major portion includes eleven chapters. The first is a brief introduction. The second chapter is devoted to texture: size-distribution, shape, porosity, and other small-scale relations of the constituent particles. The third chapter covers chemical composition, and the fourth is given over to structure: larger features such as bedding, ripple mark, and stylolites. Two chapters are devoted to color and classification of sedimentary rocks, and then a chapter to each of the principal types: conglomerates and breccias, sandstones, shales and argillites, limestones and dolomites; and to the nonclastic sediments.

The remainder of the book includes a chapter on weathering, one on transportation of sediments, one on deposition, and a final chapter on lithification and diagenesis: how sediments become "hard" rocks and what may happen to them after deposition.

To this reviewer, Pettijohn's present contribution to the study of sedimentary rocks is twofold: First, he has summarized and brought up to date all the

pertinent material; his references include many foreign and unfamiliar but valuable works. In most cases he has re-expressed the contributions of other authors and with the aid of numerous diagrams and photomicrographs has given their work an added meaning. (A considerable part of this material, however, had been competently treated before.) Second, throughout the volume he has stressed the interrelationship of sediment, environment, and tectonics. In the chapter entitled Deposition he discusses this relationship in an engrossing and masterly fashion.

To whom will the book appeal? First of all, it is a book for geologists, primarily for those geologists whose interests lie directly in the field it covers. The amateur, or the scientist from another field, will find it heavy going. For the teacher and student of sedimentary rocks this volume will be a useful source book.

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ENLARGED EDITION

Crucibles. The Story of Chemistry. Bernard Jaffe. xii + 480 pp. $3.95. Simon & Schuster. New York.

WHEN *Crucibles* was published in 1930 as The Francis Bacon Award (Book) for Humanizing Knowledge, Edwin Slosson described it as "the history of Chemistry told in biographies . . . with the necessary scientific explanation deftly worked in with as few repellent terms as possible."

That characterization may be accepted for this "enlarged edition" which, by minor revisions of two chapters and considerable editing and enlargement of the chapter on Langmuir, continues the story through nuclear fission The three chapters added are entitled: Ernest O. Lawrence, Men Who Harnessed Nuclear Fission, and Nuclear Energy Tomorrow.

The author departs somewhat from the biographical pattern in the last two chapters. The reason for this in Chapter xviii, no doubt, is the difficulty of choosing any single individual whose contribution to the achievement of nuclear fission would give him preeminence in that accomplishment.

The reader is impressed by the new chapters as being vivid yet objective in their account of "a colossal task . . . completed in so short a time." The author pronounces "the goal of the ancient alchemists had not only been reached but had been left far behind." The alchemists had never dreamed there was an almost infinitude of "energy locked up in the heart of the atom."

The theme of the last chapter attempts to answer: What will this energy do to man, or can man do something worth while with it? It is in this closing section of the enlargement that the author lapses from the role of historian and prophet, at times, and becomes the propagandist. Even so, the reader closes the volume with the impression that, although the energy of fission has its terrors, it also has its benedictions if puny man can but make a workable choice of that aspect of its potentials.

Both the index and the section labeled "Sources" make adequate recognition of the enlargement. Those who read the first issue will surely wish, in this edition, to bring "the dramatic story of the atom up-to-date." The book well illustrates the art of making a good volume better.

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THE ENGLISH VIRTUOSI

Scientists and Amateurs. A History of the Royal Society. Dorothy Stimson. xiii + 270 pp. Illus. $4.00. Schuman. New York.

IT IS said that in spite of the world-wide fame that he achieved, the greatest disappointment of H. G. Wells' life was that he was never invited to become a member of the Royal Society. Yet, what he considered to be the churlishness of the Royal Society in confining its membership to scientists who have achieved great distinction in their various fields, represents the main factor that enabled the Society to attain the famed position it now holds throughout the world.

For, in the early days of its existence, a majority of its members were gentlemen of the leisure classes, of no scientific attainments, well endowed with money, and interested in the advancement of learning for its own sake. Their financial support enabled the Society, almost always financially embarrassed, to remain in existence, but they could not by notable scientific achievement effectively reply to the biting satire of many of England's foremost writers of the day—amongst them Samuel Butler, Swift, Shadwell, and Steele. A contemporary of Steele, after reading the Society's account of a child born without a brain, remarked that had it lived it would have made an excellent publisher of the *Philosophical Transactions*. But these criticisms nevertheless had the effect of forcing the Society to put its house in order by avoiding the discussion of trivialities. (No doubt the Senator who recently was dismayed to find a U. S. government publication on the sex of watermelons would have evoked a sympathetic response at this time.)

Again, the Society was beset by political difficulties (not the least of which was the suspicion aroused by its early attempts at international collaboration), difficulties which unfortunately are still only too prevalent in the present day.

By the latter half of the nineteenth century, however, the Society had become firmly established in its own premises and the scientists were at last fully in control of the Society's policy. Financial recognition by the government came in 1850 with first grant of £1,000 which, however, did not alter the fact that the Society remained a private body. But this assistance,

supplemented by that of many private benefactors, enabled it to extend its support of fundamental scientific research by financing laboratories, such as the Mond at Cambridge, and establishing research professorships.

To conclude this short note on Dorothy Stimson's stimulating history of the Society, one may reflect on two odd facts she mentions: The first is that, although the constitutional ineligibility of women for membership was removed soon after the first world war, it was not until 1945, and even then with some dissension, that the first two women members were elected. The second shows that as long ago as just after the Society's formation in 1662 men were thinking of synthetic substitutes for natural materials. The "Ballad of Gresham Colledge," a verse associated with a Mr. Charles Howard, poses a question ". . . wheather since without Barke there may be taning, some cheaper way may not be tryed of makeing Leather without Hyde."

S. G. Rison
British Commonwealth Scientific Office
Washington, D. C.

AVIAN ACTIVITIES

Bird Display and Behaviour. Edward A. Armstrong. 431 pp. Illus. $5.50. Oxford Univ. Press. New York.

THIS is a revision and extension of an early work by the same author published by the University Press at Cambridge in 1942 under the title *Bird Display*. General arrangement in the two volumes is the same, though the 19 chapters and 381 pages of the first edition have been extended to 22 chapters and 431 pages in the present one. As the new volume has a decidedly larger type bed per page, the amount of material is evidently expanded by nearly one third. The author remarks in the preface that he has been able to correct a few errors and to profit "by the comments of critics and reviewers."

The work is a valuable contribution and a volume that will find a useful place on the bookshelf of students of animal behavior and of ornithologists, both professional and amateur. The pages are replete with interesting and well-selected examples of avian activities expressed clearly and tersely, with a minimum of the useless technicalities found in some sources. A simple scheme refers to the extended bibliography, where further details may be located in the original texts. Discussion is under common names, there being an appendix that gives the scientific names for those who wish them.

The discussions outline personal opinions of the author in a field where there may be disagreements, but they are on the whole conservative. The book may be definitely recommended as a reference work for thoughtful and pleasant reading.

Alexander Wetmore
Smithsonian Institution
Washington, D. C.

BRIEFLY REVIEWED

Science Outpost. Papers of the Sino-British Science Co-operation Office, 1942–1946. Joseph and Dorothy Needham, Eds. xi + 313 pp. Illus. 25 *s*. Pilot Press. London.

The struggle of China's science and technology to survive and develop in the face of the last war is described with insight in this volume. *Science Outpost* takes the form of a potpourri of reports, excerpts from letters and diaries, radio broadcasts, addresses, articles, and some remarkable poems. The editors call it a miscellany, and it is, but it carries the force of a thoroughly unified work because the contributors, of whom the most frequent and one of the most interesting is Professor Needham himself, have succeeded in this collection in catching the personal quality and spirit of an indomitable effort. The book is well illustrated with sixty photographs and contains in addition maps and diagrams of the organization of science in China, a useful list of scientific papers by Chinese scientists transmitted by the SBSCO for publication in the West, and an index of scientific personnel in China.

Howard S. Mason
National Institutes of Health
Bethesda, Maryland

Health Instruction Yearbook, 1948. Oliver E. Byrd, Ed. x + 320 pp. $3.50. Stanford Univ. Press. Stanford, California.

The 1948 *Yearbook* is the sixth annual compendium of current information briefed from 321 magazine articles published during the year in 100 different health publications, including *The American Journal of Public Health, The Journal of the American Medical Association*, and *The Congressional Record*.

The material is organized in an excellent manner, and it covers a field so large that the student will find readily accessible such varied and pertinent facts as mortality rates; need for 14,000 more psychiatrists; lack of well-trained persons for school health programs; current life expectancy; density of population; superiority of meat to legumes; how a person can combat fatigue; number of admissions to mental hospitals; time spent by boys reading comic books; new streptomycin treatment of TB; preparation for an approaching hurricane; name and qualifications of the new Surgeon General; dishwashing; juvenile delinquency; famine in India; current public health trends; etc.

The format of the book is enhanced by a long bibliography, a list of magazine sources abstracted, an author index, a subject index, tables, numbered paragraphs and sentences, and outlines. Each of the 20 chapters is preceded by an explanatory comment.

I believe that Dr. Byrd, who is professor of health education at the School of Education, Stanford University, has again succeeded in making the *Yearbook*

both a textbook and a reference work for nurses, physicians, students, laymen, and others interested in public health. It promulgates current experience, current research, and current opinion.

JULIAN M. SCHERR

The Medical Library of Bellevue Hospital
New York

Boy's Book of Snakes. Percy A. Morris. viii + 185 pp. Illus. $3.00. Roland Press. New York.

The book is dedicated "to boys and girls of the out-of-doors." Uncritically, it is an excellent book for the general information of youth on our most abused and misunderstood animals. It is well illustrated, interestingly written, and should be a real help to the young scientist. As a matter of opinion, it seems unfortunate that the technical names are listed at the back of the book instead of under the illustrations and through the text, where they would give the beginner a good start on scientific nomenclature.

It is also unfortunate that Mr. Morris apparently failed to have his manuscript reviewed by a herpetologist, for the book contains too many trivial misstatements, which the reader will eventually discover if he continues the study of herpetology. Two examples: Sea snakes are said to come ashore to lay; actually some have their young born at sea. Illustration of a rattler's rattle labels the broken terminal segment the "button." Actually, the "button" is the first segment which is present on a newborn snake.

CHAPMAN GRANT

San Diego, California

Wild Animals of the World. Mary Baker and William Bridges. 272 pp. Illus. $4.95. Garden City Pub. Garden City, N. Y.

Mary Baker made the pictures, 252 of them, attractive and with a style of her own, an outstanding series of animal portraits.

Each is accompanied by a short, pithy, journalistic account of the animal figured, with a description and a mixture of jungle and zoo lore collected by William Bridges, the able curator of publications of the New York Zoological Society. Bridges, trained as a journalist, has lived among animals, in and out of the zoo, for a number of years, and acquired a sympathetic understanding of them which is shown in these lively paragraphs.

With 101 different kinds of pocket gopher known, and 70 kinds of guenon monkeys, the book cannot be a complete natural history, as there is room for only one pocket gopher and one guenon monkey. Arranged alphabetically from *aardvark* to *zebu,* the book might be considered a short encyclopedia of the principal mammals of the world. Informative and well-written, it is good reading, and the illustrations (many of them in color) will delight both children and adults.

W. M. MANN

National Zoological Park
Washington, D. C.

The Chemical Arts of Old China. Li Ch'iao-p'ing. viii + 215 pp. Illus. $5.00. Journal of Chemical Education. Easton, Pa.

The scientific and historical shortcomings of this pleasantly printed and delightfully illustrated book are mentioned by Tenney L. Davis in his foreword. This makes it the easier to enjoy these reports about a strange part of our world where the old methods and attitudes are still alive, where "even today in every Chinese industrial chemical plant a god of the respective industry is installed, and seasonal sacrifice is piously offered" (p. 4). Only the fantastic tales of alchemistical efforts belong entirely to the past. Chinese inventions opened up new fields of chemical manufacture in early times and then remained stationary for many centuries. The two-thousand-year old Chinese rig for drilling salt wells was recently cited as a model for the modern cable rig of today's oil fields (*The Lamp,* November 1948). Gunpowder, paper, and porcelain were made in the old original ways until quite recently. The old fermentation process to produce soybean sauce requires more than one year for completion. In this field, recent efforts at modernization have been made, particularly by Togano, who introduced the use of the pure culture of *Aspergillus orizae.* Where Professor Li's sympathies lie becomes clear from his remark: ". . . Togano's method still fails to yield a sauce which will match the delicate flavor of those produced by the older process" (p. 179).

Ceramics are described in the longest of the fifteen chapters; one might have liked to see an additional one on the textile industries. Metals, ink and dyes, and fermentations are treated quite extensively. Here, as in the shorter articles on, among other things, salt, gunpowder, oils, cosmetics, sugar, paper, and leather, old tales and practical procedures are given.

We are vitally interested in China, and we need to understand it. This book, besides having great aesthetic value, contributes to the knowledge of a culture in which time seems to play an entirely different role than in ours. The impact of modern industries on such a culture is a serious matter.

EDUARD FARBER

Washington, D. C.

CORRESPONDENCE

DEMOCRATIC NOMINATION

(Dr. Taylor adds this supplement to his article of last year.)

An article on "Three Methods of Voting," in THE SCIENTIFIC MONTHLY for October 1948, prompted a number of readers to remark that sound voting is impossible without sound nomination. Of course those readers are right. We therefore, in what follows, consider nomination.

Under ideal conditions, any of the usual methods of nomination may put up satisfactory candidates. Often, however, the conditions are so far from ideal that these methods put up candidates that fail to represent and interest the voters; consequently, these methods discourage many potential candidates and voters, both about particular elections and about democracy.

The usual methods include nominations by a committee, nominations from the floor, and nominations by written petition. Nominations by a committee may seem, or be, dictatorial. Nominations from the floor are often made by the most impulsive or specially interested few. As other members wish neither to seem critical of those nominations nor to cause "too many nominations," someone moves that the nominations be closed. The motion is seconded and carried. Written nominations, each endorsed by a given number of voters, may represent specially interested groups. Thus nominations by any of the usual methods, if the voters are not unusually alert, may leave the voters at the mercy of special groups.

Probably no method of nomination can be perfect; nevertheless, there is a simple way for members of an organization of not more than a few hundred members to put up representative candidates. The method is Proportional Representation.

By this method as applied to nomination, each voter receives a slip of paper with as many blank lines as there are offices to be filled, or 5 percent of the membership, whichever is larger, up to 6 lines. (An organization in which "every soldier is a general" would have to allow more lines.) The voter is asked to write the names of the persons he would like as candidates, his first choice on the first line, his second on the second, etc., so far as he wishes within the number of lines. To give him time to learn and think about possible candidates, he is allowed several days in which to return the slip.

Since the voter knows that the slips are to be counted according to Proportional Representation, he is free to vote for whomsoever he wishes. In other words, he does not fear lest he "throw away his vote," or "split the party," or nominate someone he does not want; he knows that his slip will help to nominate either his first choice, or one of his later choices, according to how many of the other voters agree with him.

Let us suppose that this method is used by an organization of more than a hundred members. This organization has the rule that the final slate must contain twice as many names as there are offices to be filled; or if the voters fail to propose that many names, the slate must contain all the names that the voters do propose. The organization needs nominations for candidates for 3 vacancies on the executive committee. The secretary gives each member one slip with 6 blank lines on it. One hundred members write in names and return their slips. These slips together list, as first choices, 16 different names.

This organization's rule requires that the final slate contain, for the 3 offices, 6 names (if the voters propose that many). To find out which 6 of the 16 names represent best the 100 voters as individuals, the tellers array the slips according to first choices, determine the quota necessary to nominate a candidate, and transfer otherwise superfluous or ineffective slips according to those slips' subsequent choices, all as prescribed by definite rules. (See references, previous article.) The result is a fairly representative slate of six nominees. Thus, if 30 of the 100 slips presented names of conservatives, 56, moderates, and 14, radicals, then, as the rules work out, 2 conservatives, 3 moderates, and 1 radical are nominated.

If the voters know all the possible nominees well, and agree beforehand that their slips shall count as final ballots, the tellers can determine the larger quota and count the ballots not for nomination but for election. In the example before us, according to the rules for Proportional Representation, 1 conservative and 2 moderates would be elected. (If only one office were to be filled the counting should follow the rules not for Proportional Representation but for Majority Preference, as outlined in "Three Methods of Voting.") Usually, however, the voters need to learn who are the actual nominees and to gather information about them before choosing between them. To this end, a formal statement of each nominee's qualifications, furnished either by the nominee or his advocate, or by some perhaps more trusted source, can make for intelligent voting.

Elections, not for all offices, but for a basic few—e.g., for the members of an executive committee who appoint other officers—encourage the voters to adopt suitable methods of nomination and election, to make good nominations, and to vote intelligently.

Intelligent nomination and voting should advance both democracy and science, since, properly understood, democracy and science are inherently allied.

WILLIAM S. TAYLOR

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Smith College

ERRATUM

I am very much chagrined to discover that the formula for melanin in my article on "The Genetic Approach to Human Individuality" (SMO, March 1949) contains several errors. The carbon atoms at the left of each ring in the bottom row (p. 168) should not have a hydrogen atom attached to them. Also, in two instances the double bond attached to nitrogen should be a single bond.

LAURENCE H. SNYDER

University of Oklahoma

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THE

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AUREOMYCIN

PERRIN H. LONG

Dr. Long (M.D., Michigan, 1924) has been on the medical staff of The Johns Hopkins University since 1929, except for service as Colonel in the Army Medical Corps during World War II. He has been professor of preventive medicine and head of the Department since 1940. In the fields of bacteriology, filterable viruses, and chemotherapy he has served as an associate of the Rockefeller Institute, as consultant to the Secretary of War, and on many committees.

SOME twelve years ago a prominent member of the staff of Queen Charlotte's Hospital in London, who had reviewed the cases of childbirth fever in that hospital that had been treated with Prontosil (the first sulfa drug) by Leonard Colebrook, made the following statement: "It is a matter of personal fancy which of these alternative explanations appeals to any of us as individuals, but the accumulated experience of the past shows that a fortuitous change in the virulence of a prevalent organism is no more improbable than the discovery of an effective chemotherapeutic substance." As a rule, the saying that "the old order dies hard" is as true in medicine as in any other line of endeavor, but, at the time the above words were being published, this for once was not true, and the early sulfa drugs were receiving wide acceptance by physicians and were being demanded by their patients.

The success of the sulfonamides was instant and spectacular, and, as new derivative after new derivative was synthesized, each with wider therapeutic properties, and with greater or lesser toxic potentialities, it was not strange that scientists began to search for other substances, which might be more effective and less toxic, for use as therapeutic agents in combating infectious diseases. There can be little doubt that the success of the sulfa drugs played an important role in stimulating a much more rapid development of the antibiotics than would otherwise have occurred.

The idea of using bacterial or mold-growth products for treating infectious diseases is not a new one. For more than fifty years physicians have dabbled off and on with substances derived from microorganisms. In 1929, Fleming, after describing the antibacterial effects of penicillin, definitely spoke of the possibilities of its use in wet dressings for combating certain types of infection, and later the work of Rene Dubos with Gramacidin showed that microbial products could exert powerful anti-infective effects. So it was not exactly surprising to those who were interested in the field of antibiotics to find the Floreys describing in 1940–41 the beneficial effects of penicillin in experimental and clinical infections. One must hasten to add that this is in no way a disparagement of their work, which ushered in one of the great eras in the treatment of infectious disease. It is only to show that the utilization of the antibiotic effects of penicillin was not a completely unexpected event.

The advent of penicillin, with its extraordinary ability to cure certain diseases, its relatively low toxicity, and its appearance during wartime when its therapeutic potentialities made it a military supply of extreme importance, stimulated work in

the field of antibiotics well beyond that which probably would have been witnessed in peaceful times. Biologists both at home and abroad began exploring bacteria, molds, fungi, and higher plants for products that might serve as antibacterial agents. Soil samples from their own front yards and from remote parts of the world, and stock cultures of bacteria, molds, etc., were tested by microbiologists in attempts to discover new, more potent, and more widely effective antibiotics. It is interesting to note, however, that until recently only one really effective agent—streptomycin—was developed, although the amount of work involved in this search was prodigious.

This is not surprising. Penicillin, although highly effective against spirochetal and coccal diseases, was not very effective against diseases produced by certain gram-negative bacilli. Then, too, although it could be taken by mouth, the favored and most effective way of administering it to the patient was by injection, a method most people find disagreeable. Streptomycin was very effective against gram-negative bacillary infections and was the first of the antibiotics to show any real value in the treatment of tuberculosis. It, too, had to be given by injection, it had the further disadvantage of producing certain disagreeable toxic effects, and microorganisms often become quickly resistant to its effects. Thus it can be seen that there was a real reason for looking for an antibiotic that would have a wide range of action, little serious organic toxicity, and that could be given by mouth.

In 1947, in the laboratories of Dr. B. M. Duggar, the consulting mycologist to the Lederle Laboratories Division of the American Cyanamid Company, tests were being carried out on specimens of bacteria and molds that had been isolated from soils obtained in many parts of the world. Among them was actinomyces No. 377, which had been isolated from a sample of soil sent to his laboratory from Illinois. The cultivation of this soil upon appropriate media revealed the presence of a golden-yellow mold, which was quickly identified as a member of the Streptomyces family, and was found to produce an antibiotic which, in the test tube or on agar plates, inhibited or retarded the growth of more than fifty species of microorganisms. Then a study was begun to determine what variety of a Streptomyces this new strain was, because it was known that several species of Streptomyces were capable of producing a yellow pigment. Exhaustive studies soon showed that this mold represented a hitherto undescribed species of Streptomyces, to which was given the name Streptomyces aureofasciens.

It is not to be supposed that the other research laboratories in Lederle's were marking time while Dr. Duggar was identifying and classifying the new mold. Far from it, in fact. Under the able leadership of Dr. Y. Subbarow, the chemistry, toxicology, pharmacology, and experimental therapeutic effectiveness of the new antibiotic, to which the laboratory number of A-377 was given, were being investigated. From the toxicological laboratory came the report, "Relatively non-toxic in acute and chronic toxicity tests in mice, rats, and other animals." From the pharmacological laboratory: "A-377 is absorbed into the blood and excreted in the urine when it is given by mouth to animals." From the chemical laboratory: "A-377 has been analyzed and its components determined. While its chemical structure has not yet been defined, it is possible to crystallize the hydrochloride salt of the antibiotic in a fairly pure state." From the rickettsial laboratory of Dr. Harold Cox: "A-377 inhibits the growth of rickettsia in chick embryos." From the experimental therapeutic laboratory: "A-377 cures experimental pneumococcal, streptococcal, Friedlander's bacillary, and other infections in mice and chickens." And from the viral laboratory: "A-377 inhibits the growth of lymphogranuloma venereum and of psittacosis in experimental infections in chick embryos."

While these tests were being conducted in the laboratories at Pearl River, comparable experiments with aureomycin were being done in the Thorndike Memorial Laboratory at the Boston City Hospital and at Johns Hopkins. Interested groups in those medical centers confirmed and extended the observations made at Lederle's. It was not, however, all smooth sailing. At first, in one of the laboratories it appeared that, when comparisons were carried out to determine the relative effectiveness of aureomycin and penicillin in experimental streptococcal or pneumococcal infections in mice, the latter was always much more effective. After considerable experimental work, however, it was found that this was only apparently true, and, later, when clinical tests were at hand, it was noted that aureomycin almost invariably proved to be more effective in treating infections in human beings than one would have been led to suspect from the results obtained in treating experimental mice, rats, or rabbits. This was a very interesting point because, with the sulfa drugs and other antibiotics, the reverse was frequently true.

Clinical tests with aureomycin began in January 1948, when suitable samples of the antibiotic were sent to the Presbyterian, New York, and Harlem Hospitals in New York City; the Boston City Hospital; Johns Hopkins; the George Washington

Medical School in Washington; and the University of California Hospital. The first patients treated were suffering from a venereal viral disease known as lymphogranuloma venereum. The virus causing this disease is classed as a "large" virus and belongs to the group which includes the viruses of parrot fever, pigeon fever, etc., and in which it is believed the yet uncharacterized virus of primary atypical pneumonia belongs. Prior to the advent of aureomycin, moderately satisfactory therapeutic results had been obtained by using sulfadiazine or penicillin in lymphogranuloma. It became quickly evident that aureomycin was superior therapeutically to either of these agents, and also more rapid in its curative effect. Acute and subacute instances of this disease were arrested within a few weeks after treatment with this new antibiotic was begun.

As clear-cut evidence had been obtained in the Laboratory that aureomycin was capable of producing an antibiotic effect against Rickettsia, it was obvious that a clinical test was indicated in Rocky Mountain Spotted Fever, Eastern variety. This disease is carried by the wood tick *Dermacentor variabilis* and is found over the eastern half of the United States. It is especially prevalent in Maryland, the District of Columbia, and Virginia. It occurs commonly from June to September, and the infection results from rubbing the excreta of infected ticks into the microscopic wound in the skin which the tick makes while taking its blood meal from human beings. In the past, about 20 percent of the individuals who contracted this disease died. In 1946, this case fatality rate was decreased following the use of para-amino-benzoic-acid in the treatment of this disease. This latter agent, although quite effective as a therapeutic agent, did, however, cause a number of serious toxic reactions. As was to be expected, aureomycin proved to be effective in this disease. The duration of fever was markedly shortened, the rash disappeared quickly, the distressing symptoms of head- and backache were short-lived, the complications of the disease were reduced almost to nil, and in the reported series there were no deaths.

Another rickettsial disease that is apparently increasing in importance in this country is Q(uery) fever, which was first described about fifteen years ago in Australia. It takes the form of a debilitating pneumonia and, although the death rate from this disease is not great, it can be a difficult disease for the patient and physician alike. It can occur in epidemic waves, and, although the infecting organism has been found in milk, its method of spread (especially in epidemics) is not well understood. The therapeutic effects of aureomycin in this disease have been excellent, as demonstrated by workers in the hospital of the University of California and the California State Viral Laboratory, and one can now say that there is a cure for it. In other rickettsial diseases such as endemic (rat) typhus, epidemic typhus, scrub typhus, and Brill's disease, equally satisfactory results have been obtained when patients ill with these diseases have been treated with aureomycin. As a matter of fact, physicians can now feel quite confident that they and the patient need have little fear as to the outcome of a rickettsial infection if treatment with aureomycin is started before the patient is at death's door. This marks another milestone in the conquest of disease.

Undulant fever (Malta fever, brucellosis) can be one of the most miserable of the diseases that affect mankind. It is often insidious in its onset, it may be very difficult to diagnose definitely, it can be characterized by recurrent bouts of fever and malaise occurring sometimes for years, inbetween the periods of fever its sufferers often experience lassitude and numerous minor but aggravating symptoms, and although few people are recorded annually in the mortality statistics as dying from undulant fever, this disease has ruined the productive life of many individuals. In this country it is contracted primarily from drinking unpasteurized milk from cows that have the disease, although it may result from eating undercooked pork or beef, or from contact with infected cows, pigs, or horses. It is a disease of rural people, stockyard workers, and veterinarians. It is not known how many people have brucellosis and recover completely after the initial attack. Although sulfa drugs and combinations of these drugs with streptomycin had some curative or arresting effect in this disease, their uses as therapeutic agents left much to be desired. It would appear from the reports now available that aureomycin has a real curative effect in patients acutely ill with this disease. The temperature comes down to normal within two or three days in patients who are adequately treated, and, after the course of therapy is completed, there is every reason to believe that in the majority of patients the cure effected is permanent.

In chronic undulant fever the results of treatment with aureomycin have not been as clean-cut. To begin with, unless one has a very definite history of a proved acute attack of the disease, and then typical symptoms and signs of a recrudescence together with diagnostic laboratory tests of value, the diagnosis of chronic undulant fever is fraught with difficulties and error. It seems quite likely that a number of people in this country today, who believe they have the chronic form of the disease, have nothing of the sort, and represent individuals

in whom the diagnosis is in error. It is for this reason that the results that have been obtained so far from the use of aureomycin in chronic undulant fever have been spotty. Obviously, the antibiotic will be ineffective if the patient treated does not have undulant fever.

Another disease in which, astonishingly enough, good results from treatment with aureomycin have been reported is primary atypical pneumonia. This is an interesting disease, because at least as far as its recognition by physicians is concerned, it can be classed as a "new" disease. It was first recognized and described clinically in Hawaii, Texas, and Pennsylvania about fifteen years ago. It occurs sporadically and also in epidemics. It is a common ailment, is rarely fatal, but it can produce a disabling, painful, annoying illness. The best evidence points to a virus as its cause, but this viral agent has escaped characterization up to the present time. It is believed, however, that the virus of primary atypical pneumonia resembles those of lymphogranuloma venereum, psittacosis, ornithosis, and others that may produce pneumonic changes. Thus there was good reason to test the therapeutic effects of aureomycin in this disease. So far the results of treatment as reported from the Boston City, Presbyterian, New York, and Johns Hopkins hospitals have been excellent. When adequate, vigorous therapy is used, the temperature of the patient becomes normal within twenty-four to forty-eight hours, cough and pain in the chest disappear, the consolidation in the lungs resolves quickly, and in a short time the patient is cured. Thus, another disease for which no specific method of treatment existed prior to aureomycin is now amenable to therapy with this antibiotic.

Earlier, it was pointed out that aureomycin was effective in the test tube and in experimental infections in mice produced by various gram-positive and gram-negative bacteria. Its real range of activity against human infections produced by these microoganisms is not yet known, because, in most instances, treatment with the sulfonamides, penicillin, or streptomycin produced excellent results in these bacterial infections. For this reason, in the first year of the clinical testing of aureomycin, most of the available antibiotic effort was expended upon its evaluation in infections for which adequate and specific therapy was lacking. Evidence is in hand, however, that aureomycin has a specific effect in a considerable number of bacterial infections. It is excellent (although whether as effective as penicillin is another question yet to be decided) in certain streptococcal, pneumococcal, and staphylococcal infections. Currently, it would appear that in infections produced by Group D streptococci in the urinary tract, blood stream, or in the meninges, aureomycin is somewhat superior to penicillin in its therapeutic effects. On the other hand, in the treatment of gonorrhea, which is produced by a gram-negative coccus, penicillin is definitely superior. Reports concerning the use of aureomycin in infections produced by the meningococcus, such as epidemic meningitis or meningococcemia, are so scanty that outside of the fact that it has been demonstrated as being curative in a few instances nothing can be said.

In systemic infections produced by gram-negative bacilli, aureomycin, on the basis of present evidence, would appear to be relatively ineffective in those produced by Ps. aeruginosa (Pyocyaneous bacillus), P. vulgaris, and H. pertussis (whooping cough bacillus). It appears to be an excellent remedy for the treatment of systemic, localized, or urinary tract infections produced by coli-aerogenes groups of microorganisms. In tularemia (rabbit fever) the drug appears to be curative, as it does in infections produced by Kl. pneumoniae (Friedlander's bacillus). Its use in typhoid fever is questionable. There can be no doubt of its bacteriostatic effect against S. typhosa in the test tube, but sharply defined results from its therapeutic use have not been obtained in the treatment of typhoid fever, even though large doses were used Little or nothing is known of its value in bacillary dysentery or in the treatment of acute food poisonings produced by microorganisms of the Salmonella group.

Experimentally, aureomycin has been shown to have a therapeutic effect in Weil's disease (a type of infectious jaundice). It also has some curative effect in syphilis, but the range of its use in this disease will take years to define. To begin with, in early syphilis, a considerable number of suitable cases will have to be treated and then followed carefully for at least eighteen months before its comparative value as a therapeutic agent can be initially assayed. Still, it is interesting that aureomycin has demonstrated some curative action in this infection.

The preferred way of administering this antibiotic is by mouth. Since it is produced as the hydrochloride salt, it is very painful and irritating when administered by the intramuscular or subcutaneous route. It can be given successfully by the intravenous method, but this way of using aureomycin is still in the stage of experimental development, and it remains to be determined just what the most satisfactory diluent for it will be.

Patricia Todd and Dr. Eleanor A. Bliss read results of a therapeutic experiment in a Johns Hopkins laboratory.

To date it appears unwise to inject the antibiotic directly into the spinal canal in the treatment of meningitis.

Not much is known about the absorption, distribution, and excretion of aureomycin when it is administered in curative doses to patients. This situation results from the fact that the exact chemical structure of aureomycin is still unknown, and there is no chemical test that can be used to detect it in the body fluids of tissues. One can get a rough approximation of how much of the antibiotic is in the blood, urine, spinal fluids, etc. by testing the ability of these fluids to inhibit the growth of certain microorganisms, but this biological method is very crude. Also, aureomycin deteriorates when in solution, and this again makes its accurate determination by biological methods difficult. In fact, there is no method of assaying this antibiotic which can be called quantitative in its results.

Another interesting thing about aureomycin is its apparent lack of serious or disturbing toxic effects. To date the only reactions reported from its use have been nausea and vomiting, and it is still not clear whether these side effects are produced by the antibiotic itself or by some impurity in it. In some patients, the nausea and vomiting have been severe enough to force them to stop taking the drug. Practically everyone who is given aureomycin develops some looseness of bowel movements, and in certain individuals diarrhea results. For this reason, it is believed that one should use caution in administering aureomycin to individuals who have colitis or irritable lower intestinal tracts. Again, the cause of this phenomenon is not completely clear. There can be little doubt that aureomycin suppresses the growth of intestinal bacteria, and this lack of bacterial activity in the gut may produce the large bulky stools noted by individuals who take this drug. On the other hand, however, this antibiotic may have some direct irritating action on the bowel. The interesting thing is that aureomycin, at least up to the time this is being written [April 1949], has not been reported as harmfully affecting the blood or blood-forming organs, the kidneys, or the skin. None of the disturbing toxic reactions that have been noted in the course of the therapeutic use of the sulfa drugs or certain other antibiotics have made their appearance to date in patients receiving aureomycin. Time may change this statement, but so far experience indicates that aureomycin is an antibiotic of low toxicity.

Finally, it would appear that infecting microorganisms do not develop resistance to the anti-infective properties of aureomycin at a rate comparable to that noted in the case of the sulfa drugs, or penicillin, or streptomycin. It is difficult by using current experimental methods to make microorganisms resistant in vitro to this antibiotic The same is apparently true in so far as the etiological agents in clinical infections are concerned. This, then, constitutes another advantage, because one of the disadvantages, especially when streptomycin is being used, is that the infecting microorganisms frequently become resistant to its antibiotic effects.

The past fifteen years have witnessed revolutionary and extraordinary changes in our concepts respecting the prognosis and treatment of infectious diseases. Illnesses such as streptococcal meningitis, which formerly were almost universally fatal, are now rarely seen and are generally promptly amenable to treatment. A disease such as early syphilis, which previously required months, even years, of treatment, now appears to be cured in the majority of cases after a relatively few days of treatment with penicillin. In aureomycin, we have an antibiotic that can be given by mouth with a low instance of serious toxicity and with a curative effect in a wide variety of diseases. The lives of millions of people have been saved by the use of the sulfas and the antibiotics.

ABSOLUTE ELECTRICAL MEASUREMENTS

HARVEY L. CURTIS

After thirty-nine years of service with the Bureau of Standards, Dr. Curtis (Ph.D., Michigan, 1910) retired two years ago from his work as chief of the Interior Ballistics Section. His major scientific achievement has been in absolute electrical measurements, and his conclusions formed the basis for the United States proposals in establishing a fixed relationship between the absolute and international electrical measurement units at the International Electrical Congress last year.

SCIENCE is concerned with the prediction of the result to be expected when some known event occurs. In order to accomplish this, quantitative data must be available for all the pertinent factors that will be called into play when the event happens. Such quantitative data can be obtained only for those aspects of man's environment that are now measurable. The notable progress that has taken place in the fields of physics, chemistry, and engineering has resulted from the development of methods of measuring the most significant characteristics of physical objects. But basic measurements on living things are now being greatly expanded, profoundly influencing the biological and social sciences and hence playing an increasingly important part in such applications as agriculture, medicine, psychology, and economics.

In general, the making of a measurement involves a comparison between the object to be measured and some commonly accepted unit that can be used as a "measuring stick." The names of some of our familiar units of length and other simple quantities have come down to us from the days when the properties of easily available objects from everyday life were taken as standards. Of these, the "foot" and "stone" are still units of length and weight. The "watch" was once an important unit of time, but now is seldom used except on shipboard.

Mankind early discovered the convenience of combining units for the simpler quantities to obtain new units for related quantities. Thus, areas were measured by combining two units of length, and volume by combining three length units. It was also quite natural to express velocity in terms of length and time. The use of more complicated combinations of units did not begin, however, until comparatively recent times. The first step in the development of the modern units of physical measurement was taken in 1638 by Galileo, who expressed acceleration as velocity divided by time. It remained for Newton to show in 1687 that force can be measured as a product of mass and acceleration. The conception of energy as the product of force and distance, and of power as a time rate of doing work, took form slowly in the hundred years following Newton. Many men contributed to the development of these ideas, but Descartes and Leibnitz were perhaps the most outstanding. By the time these principles had become established, a complete system for measuring mechanical quantities had been evolved.

Meanwhile, the electrical units were being slowly developed during the first half of the nineteenth century. One of the first was the unit of capacitance, for which a Leyden jar of a particular size and shape was taken as a standard. When the telegraph was introduced in 1844, emphasis shifted to units of resistance and electromotive force. The resistance of an iron wire of specified diameter and length was at first used for a unit of resistance. When the resistance of such a wire was found to depend on the particular type of iron used, standards of copper wire were proposed; and when these were found to vary, a thread of mercury of definite size and length was employed. As a standard of electromotive force, a newly constructed Daniell cell made of high-grade materials was proposed at this time.

As a result of the contributions of Helmholtz and others, the principle of conservation of energy developed during the first half of the nineteenth century and by 1850 had become generally accepted. This made possible the extension of mechanical units to other fields of physics—such as heat, electricity, and magnetism—through consideration of transformations of mechanical energy into heat or into electrical or magnetic energy.

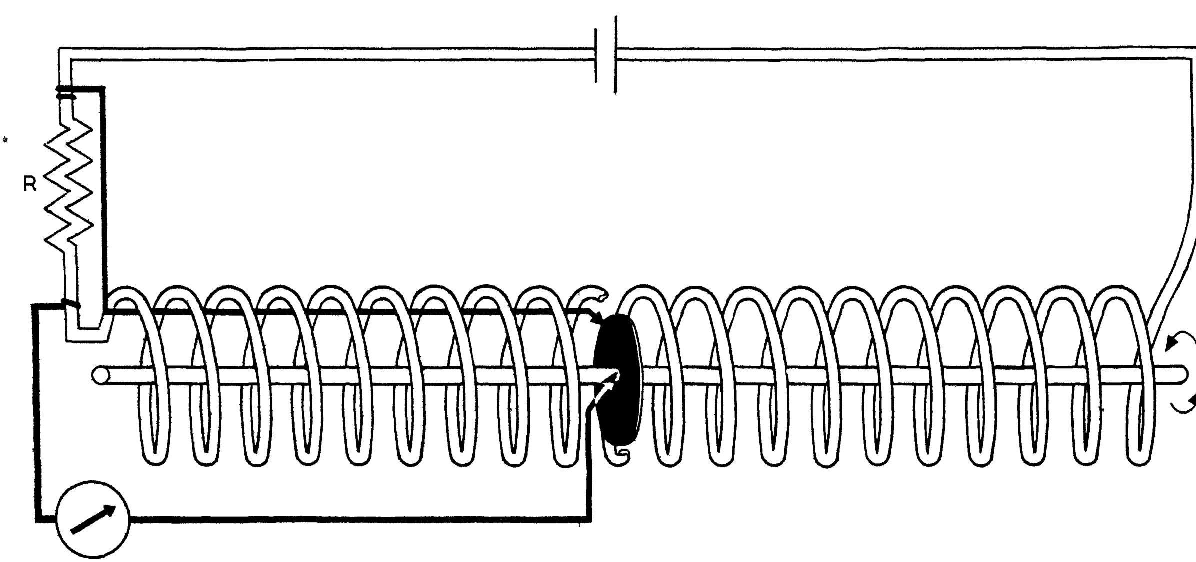
The possibility of basing the electrical units on a mechanical system of units was first demonstrated in 1851 by William Weber, who showed that this could be accomplished by accepting the principle of the conservation of energy and by assigning an arbitrary value—say, unity—to some electrical or magnetic quantity. If the permeability

of air (a quantity indicating the ease with which a magnetic field can be set up in this meidium) were fixed, the result would be an electromagnetic system of units On the other hand, assignment of a value to the dielectric constant of a vacuum (a quantity indicating the ease with which an electric field can be set up in free space) would provide an electrostatic system of units.

It was nearly ten years later that this proposal was first given serious consideration. In 1860 the British Association for the Advancement of Science appointed a committee to consider the whole question of electrical units. This committee, which included Clerk Maxwell, Lord Kelvin, Joule, and others who later became famous, studied the several different proposals and eventually laid the basis for our present absolute system of electrical and magnetic units.

The British Association committee considered carefully the advantages of an arbitrary system of electrical and magnetic units. In such a system, two electrical units would be chosen for definition, and all other electromagnetic units could then be derived from them by relatively simple electrical measurements. The system of arbitrary units which met with greatest favor among committee members was based on units of resistance and electromotive force. The unit of resistance was to be the resistance of a thread of mercury one meter long and one square millimeter in cross section, maintained at the temperature of melting ice. For a unit of electromotive force, the e.m.f. of a Daniell cell made with highly purified chemicals and used under standard conditions was suggested. These defined units were then to be related to the mechanical system by careful electromechanical measurements that would give factors by which the mechanical effects of any electromagnetic phenomenon could be determined from measurements of the electric and magnetic quantities involved

Although the arbitrary system of units had many advocates, it was finally rejected. Instead, the committee recommended a system of electrical and magnetic units based directly on the mechanical units and called this system an "absolute" electromagnetic system. The units of the absolute system are derived from the fundamental mechanical units of length, mass, and time by means of accepted principles of electromagnetism, with the permeability of space taken as unity. The com-



FORMULAS

$B = H = 4\pi I/p$

$E = \pi r^2 nB$

$E = RI = 4\pi^2 r^2 nI/p$

$R = 4\pi^2 r^2 n/p$

NOMENCLATURE

- B MAGNETIC INDUCTION
- H MAGNETIC INTENSITY
- I CURRENT IN HELIX
- p PITCH OF HELIX
- E INDUCED ELECTROMOTIVE FORCE IN DISC
- r RADIUS OF DISK
- n NUMBER OF REVOLUTIONS PER SECOND
- R RESISTANCE

FIG. 1. Diagram illustrating the Lorentz method for absolute measurement of the ohm.

mittee also sponsored experiments to develop suitable concrete standards of electrical quantities and to determine their values in absolute units.

In the absolute electromagnetic system, the units of electromotive force and current are defined in terms of mechanical units. Resistance, on the other hand, is defined by Ohm's law as the ratio of electromotive force to current in a circuit, a purely electrical relationship. Hence, if units of any two of these quantities are determined in terms of electrical or mechanical units, the third may be derived by an electrical measurement. In absolute measurements, resistance and current are usually measured directly in terms of electromechanical quantities, and electromotive force is then obtained by Ohm's law For the standardization of electrical instruments, however, it is customary to establish units of resistance and electromotive force and then obtain current by Ohm's law. Thus, the role of the three primary electrical quantities in electrical measurement depends somewhat on the field in which they are to be applied.

ABSOLUTE VALUE OF THE OHM

A basic measurement in establishing the absolute system of units is the absolute determination of the ohm. At the time the British Association committee was at work, Maxwell developed a method of making such a measurement; his results were used for a quarter of a century, until more precise and simpler procedures were developed, of which the Lorentz and self-inductive methods are typical.

Lorentz proposed a very direct method of measuring resistance. A disk at the center of a helical coil carrying current (Fig. 1) rotates in a plane perpendicular to the axis of the coil. The magnetic field within the coil due to the current is perpendicular to the disk and can be computed from the dimensions of the helix and the magnitude of the current. The disk may be considered as an infinite number of filaments coinciding with its radii. Each of these filaments cuts the lines of magnetic force at the same rate, producing an electromotive force between the center and circumference of the disk. According to electromagnetic theory, the electromotive force thus produced is numerically equal to the rate at which the magnetic lines of force are being cut and can be computed from the magnetic field within the coil, the radius of the disk, and the rate of rotation of the disk. This electromotive force may be balanced against the fall in potential in a resistance R as the current from the helix flows through it. The electromotive force, expressed in terms of the current in the coil and various mechanical measurements, may thus be set equal to the fall in potential, expressed as the product of the current and the resistance R. Since the current appears on both sides of the equation, it cancels out, and the resistance is given in terms of mechanical measurements.

A modification of the Lorentz method was developed somewhat later at the British National Physical Laboratory. The apparatus, one-half of which is shown in Figure 2, is still maintained at

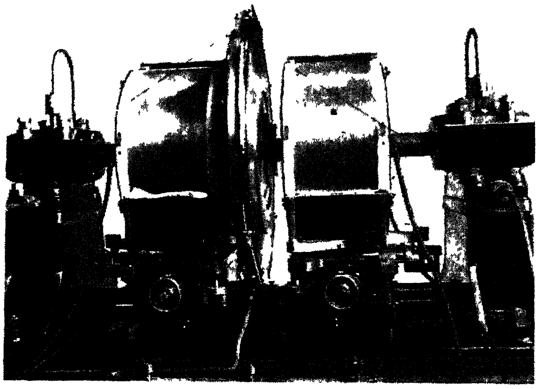


FIG. 2. Portion of the apparatus developed at the British National Physical Laboratory for absolute measurement of the ohm by a modification of the Lorentz method. An electromotive force is induced in the large disk (*center*) as it rotates in the magnetic field of the two coils on either side. A similar disk (*not shown*) is mounted between two coils on the same shaft at a distance of 1.7 meters from the first disk.

this institution. It employs two disks mounted 1.7 meters apart on a long shaft. For each disk, there is a pair of short helical coils, one on either side of the disk, carrying an electric current which produces the magnetic field in which the disk turns. The four coils are connected in series, but the current in one pair of coils is clockwise, in the other pair counterclockwise. Thus the electromotive force induced in one disk is in the opposite direction from that induced in the other. As the disks are connected electrically, the external circuit is from the edge of one disk to the edge of the other. The disks are of such size that the magnetic field about their edges is approximately zero; hence, their diameters and thicknesses need not be accurately known.

The electromotive force developed in the two disks by a unit current in the coils can be computed from the dimensions and positions of the coils, provided the permeability of all materials is the same as that of air. Resistance may thus be measured in

much the same way as with the earlier apparatus. This method is one of the most direct so far developed for the absolute measurement of electrical resistance, but it is difficult to apply because of thermal electromotive forces generated by sliding contacts at the circumferences of the disks.

The self-inductance method of determining resistance, first used by Greneisen and Giebe at the German national laboratory, has been refined at the National Bureau of Standards to the point where an accuracy of ten parts in a million is obtained. The success of this method requires the construction of a self-inductor whose inductance can be very accurately computed from its mechanical dimensions. Such an inductor was made at the Bureau by winding a helix on a threaded glass tube with a 3-inch wall.

The self-inductance method is an indirect procedure: it determines the correction factor that must be applied to an approximate value for a standard resistance in order to give an accurate absolute value. Three bridges are required: a Wheatstone bridge, an alternating-current bridge, and a pulsating-current bridge. The latter two are shown in Figure 3. The Wheatstone bridge is used to compare the resistances in the different arms of the other two bridges with the resistance of a standard resistor, the known approximate value of which is R. The resistances in the bridge arms are thus expressed as multiples of the resistance R. In the pulsating-current bridge, a vibrating tongue T charges and discharges the condenser C a certain number of times per second. When the tongue touches the upper contact, charging of the condenser begins, and a part of the charging current passes through the ballistic galvanometer B. When the condenser becomes charged, the current through the galvanometer is reversed and continues flowing in the reversed direction while the tongue moves to the lower contact and discharges the condenser. When the tongue again touches the upper contact, the cycle is repeated. The resistances (P, Q, S) in the bridge are varied until the average galvanometer current in one direction is the same as the average in the other direction. Then, because of the inertia of the heavy galvanometer coil, there will be no observable galvanometer deflection. The bridge is then "balanced," and the capacitance of the condenser is given in terms of the rate of vibration of the tongue and the resistances P, Q, and S.

The condenser is then transferred to the alternating current bridge, which uses a vibration galvanometer V (or perhaps a telephone receiver) as a detecting instrument, and the resistances of its branches are varied until the galvanometer reads zero at every instant. Then the inductance of the self-inductor is given as the product of the previously measured capacitance and the resistances R_1 and R_4. The exact value of the inductance may also be computed from the mechanical dimensions of the inductor. Thus a correction factor is obtained by which the inductance determined from the bridge measurements must be multiplied to give the value computed from mechanical measurements. If all measurements have been carefully made, the discrepancy between the two values of the inductance can only be due to the small error in the approximate value R taken as the resistance of the standard. Hence, the correction factor determined for the inductance may now be applied to the value R to give an accurate value for the resistance of the standard resistor in absolute ohms.

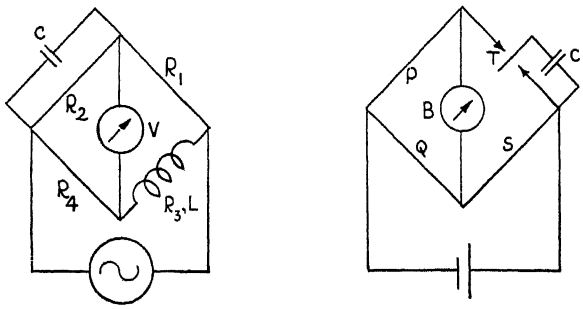


FIG. 3. Diagram illustrating the self-inductance method for absolute measurement of the ohm. In the a.-c. bridge at left, C is a capacitance; L is the self-inductor; R_1, R_2, R_3, and R_4 are resistances. In the pulsating-current bridge at right, C is the same capacitance as before; P, Q, and S are resistances; T is a tongue which vibrates n times per second. From the a.-c. bridge, $C=\frac{L}{R_1R_4}$; from the pulsating-current bridge, $C=\frac{Q}{nPS}$. Thus the measured inductance $L=\frac{QR_1R_4}{nPS}$. If Lo is the inductance of the self-inductor computed from mechanical measurements, then the corrected factor to be applied to each resistance is given by the ratio L/Lo.

The precision with which the absolute value of the ohm may be determined has markedly increased in recent years. The original determination by Maxwell was in error by more than one percent, but the most recent results by the self-inductance method do not differ among themselves by so much as two parts in a hundred thousand. This is perhaps the most precise measurement of any nonmechanical quantity in terms of mechanical units.

ABSOLUTE VALUE OF THE AMPERE

A current can be measured in absolute amperes by determining the very small mechanical force between two parts of the circuit in which it flows. Current is thus "weighed" at the National Bureau of Standards by means of the Rayleigh current balance, which measures the force between coils of wire carrying current (Fig. 4). In the center of two large fixed coils, a small coil is hung from the pan of a sensitive balance. When the three coils are connected in series, the force acting on the small coil per unit of current can be computed from the dimensions of the coaxial coils and their distance apart (In practice the current is reversed in the coil attached to the balance pan, thus doubling the force per unit current.) The total force due to the current is found by adding or removing sufficient weights from the balance pan to restore the balance to its original position. This observed force divided by the computed force per unit current gives the current in absolute units. Changing of the weights, reversal of the current, and reading of the balance are accomplished by an operator in an adjacent room, separated from the balance by a glass partition.

The maximum variation in the results of the three most accurate determinations of the absolute ampere that have been made to date is a little less than one part in a hundred thousand. Although a larger number of independent determinations have been made for the ohm than for the ampere, this agreement is quite as satisfactory as that obtained for the ohm.

An absolute measurement of current is also used to determine the electromotive force of a standard cell in terms of an absolute resistance and an absolute current. Here the electromotive force of the standard cell is compared with the fall in potential which the measured current produces in a resistance whose value is known in absolute units.

PAST AND FUTURE OF THE ABSOLUTE SYSTEM

Although the concept of an absolute system of electrical units based on the mechanical units and the properties of a vacuum originated in the middle of the nineteenth century, the development of accurate measurement methods for obtaining concrete electrical units from this concept has been a slow process within the past hundred years. During this time there were several electrical congresses and conferences that gave consideration to the electrical units. They all reaffirmed the basic nature of the absolute system, yet each recognized the need of practical units to avoid the difficulty and expense of absolute measurements in the ordinary laboratory. Thus the scientific literature of this period contains results in the "British Association ohm," the "true ohm," the "legal ohm," and the "international ohm," as well as corresponding units for current, electromotive force, and other electrical quantities.

There was a feeling at most of these conferences that reproducible standards should be adopted so that any laboratory could easily prepare its own electrical standards. Eventually, the Chicago Electrical Congress of 1893 recommended an "international" system of electrical units based on this principle. The ampere was defined as that current which would deposit a given amount of silver in a stated time from an electrolytic solution; the ohm was defined as the resistance of a specified column of mercury; and the volt was given in terms of the electromotive force of the Clark standard cell. The international system was legalized in the United States by an Act of Congress of July 12, 1894; it also became legally recognized in most other countries.

As the years passed, however, two defects were recognized in the international system. First, as the demand for accuracy increased, the units were not sufficiently reproducible; and, second, three reproducible units were specified, whereas most laboratories preferred to use only two units, obtaining the third by Ohm's law. Thus, with the establishment of national standardizing laboratories in several of the larger countries near the beginning of the present century, the problem of maintaining the standard electrical units was entrusted to them. Any laboratory could have a standard certified in terms of the units of one of the national laboratories. But there still remained some differences among the standards of these na-

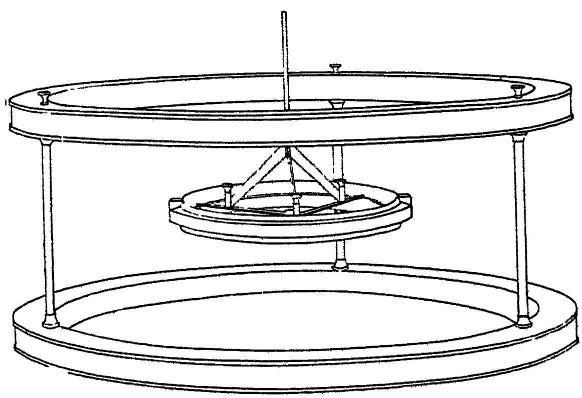


FIG. 4. Coils of the Rayleigh current balance used at the National Bureau of Standards for the absolute measurement of the ampere.

tional laboratories. To insure uniform electrical units throughout the world, the treaty of May 20, 1875 (known as the International Metric Convention and ratified by all civilized nations), was amended in 1923 to vest authority over the electrical units in the General Conference of Weights and Measures. Temporarily, the Conference took the international ohm and the international volt as the mean of the values of six national laboratories. Finally, on January 1, 1948, this system was superseded by the absolute system, incorporating the latest absolute determinations of the ohm and the ampere.

Although the definitions of the absolute units and the methods of fixing their magnitudes are different from those of the international system, the changes in magnitude were so small as to affect appreciably only measurements of high precision. For example,

1 international ohm	= 1.00049 absolute ohms.
1 international ampere	= 0 99985 absolute ampere.
1 international volt	= 1 00034 absolute volts.

As this is written, it seems improbable that any future modifications in the electrical units that may result from more accurate experimental determinations will be larger than two or three parts in a hundred thousand. Doubtless such modifications will be made if the results of future absolute measurements indicate a need for such a change.

DESERT RAIN

Through June the unvaried daily changes run
True to their sharply etched and classic form:
The blue-edged brilliance of the desert sun
Clips day from dark; the blazing silence ends
In chirping babble. Then the cold descends
Like perfume through the starlit quiet air,
Pervading every nook not blanketed.

As dew point rises, so the clouds begin
High over mountain, thunderhead and rim
Each day spread wider till the rains descend
From peak to slope and valley. Living things
Respond in floral torrent, insects swarm
While toad and reptile gorge against the norm
Of dust and heat in leaner days to come.

The day of riches over, plains return
To creosote and cactus (soft leaves burn).
Odd palo verde loses leafy fronds,
The dry arroyo holds mesquite alone,
And sharp against the evening sky are thrown
The flaming spikes of ocotillo wands.

Though lure of sand and sun and rock remain,
Most desert beauty lies in desert rain.

JOHN G. SINCLAIR

MAN'S SIX-LEGGED COMPETITORS

E. O. ESSIG

Professor Essig, who is chairman of the Division of Entomology and Parasitology of the University of California at Berkeley, has been with the University since 1914. Several of his articles on the history of science have appeared in this journal.

EVER since man first laid aside a few acorns, a store of seeds, some dried meat, and an extra skin to wear, insects have shared his thrift. The development of gardens, grainfields, orchards, pastures, and even forests has created much more favorable living conditions for the myriads of insects than would be available to them in a natural state. The growing of single agricultural crops in vast contiguous areas has made life for insects, feeding on each crop, easier in every respect; thus, in the production of wheat, corn, rice, cotton, apples, peaches, oranges, alfalfa, sugar cane, potatoes, and literally hundreds of other crops, man has bettered the circumstances of insect life. Never before in all the long history of the world have such extensive areas of luscious food been available to insects. The irrigation of immense semiarid regions, which under natural conditions could support but scant vegetation and sparse insect populations, has suddenly changed these barren wastes into Gardens of Eden for our small and versatile six-legged competitors.

In hotels, stores, restaurants, flour mills, food warehouses, and homes, insects have taken full advantage of the food and protection afforded them by man. In cold countries, the heating of human habitats during the winter has made it possible for a host of insects to continue their association with people where they could not otherwise have survived. Wherever man goes, the insects follow. Many of them have become his most dedevoted domestic animals.

In spite of man's claim to superiority over nature, and his so-called subjugation of the world, insects still haunt his person, his home, his fields, forests, and domestic animals. They have so far been inescapable. Because of their much longer sojourn on earth (more than 50 million years), their great numbers and kinds, their complex and efficient development, their ability to survive unfavorable weather, and their wide range of food, insects are apparently much better adapted to the exacting conditions of this world than man. It may well be true that the last surviving creature on earth will be an insect.

INSECTS AS PESTS

Insects contaminate as well as consume food products. In recent years, a great deal of attention has been given to the elimination of insects and their body parts, excrement, and webbing from all types of food products. Concerned in this endeavor are public-health authorities, entomologists, insecticide manufacturers, food processors, and canners. The most concerted action ever attempted is now under way to insure wholesome, insect-free fresh, dried, canned, and frozen fruits, cereals, and vegetables for the consumption of the public. New and more effective insecticides are being investigated and used by entomologists throughout the entire country and much of the world for this single purpose—the control and, in some cases, the complete eradication of insect pests.

Although insects exact a large toll of agricultural crops, they are a check on the overabundance of many plants and may serve as a balance to prevent the overcrowding of many noxious species of plants and weeds, and thus aid in the preservation of the very plants necessary for their own existence.

Many insects, such as termites and wood borers, aid in the rapid reduction of declining and dead trees and smaller plants, and thus contribute to their conversion into humus and food for other generations of plants. Plant-feeders in general furnish excrement and eventually their own bodies to aid in the supply of plant food. Thus insects are conservationists as well as consumers of plant life, just as is man.

Infestation of agricultural food crops by insects necessitates, for their control, the use of many insecticides that are incompatible with human health. To protect and preserve these crops in the fields, as well as in storage, it is extremely important that the entomologist exercise great skill

and good judgment in using only those means of control that will insure a reasonable margin of safety to human and animal consumers. There is a wide variation in the persistence and effectiveness of insecticides. A poison spray or dust that hydrolyzes or decomposes shortly after being applied may be effective in killing insect pests and lose its poisonous properties before the crop is harvested. Other, more stable, compounds may be employed on dormant fruit trees or upon very young plants, without danger to the health of the consumer. Still other sprays and dusts may be readily removed by washing. It is also important that all insecticides be free from unpleasant odors and taste and that they should not leave an offensive residue not easily removed.

Another device employed by entomologists to control injurious plant- and animal-infesting insects is biological control. By judicious liberation in sufficient quantities and at appropriate times, predaceous and parasitic insects may be used to destroy their objectionable relatives that are so destructive to man, as well as to his crops and domestic animals. Such insects are obtained by introduction from foreign countries or by artificial propagation in insectaries, and in this way the control of certain destructive insects may be accomplished wholly without the use of insecticides and their attendant dangers.

Those who are not familiar with all the intricacies of insect control are likely to be amazed at the prodigious problems that confront the entomologist, and the great amount of training, experience, and care necessary for the proper protection of foods from insect attacks and contamination on the one hand and for the employment of the many safety techniques for the protection of human health on the other.

INSECTS AS FOOD

Although insects are the commonest and most abundant forms of animal life associated with the human race, they are the least known and the most grossly misunderstood by even highly civilized peoples. Aboriginal races, however, apparently understood and appreciated the many values of insects; they used a great many of them for food and also ate various insect products, such as honey and honeydew as sweets.

I am especially desirous of calling attention to the fallacious belief that has generally been ingrained in most of us that insects such as aphids, caterpillars, and fragments of all other kinds are "filth." As a matter of fact, the minute, delicate, soft-bodied aphids, thrips, and the like, have throughout the ages been commonly eaten by human beings and other animals along with vegetables and fruits, without ill effects and no doubt even to the advantage of the consumer.

Aphids and other small species, which are common on nearly all vegetable crops, have in recent years caused great concern and much financial loss to growers and canners of spinach owing to the virtual impossibility of producing this crop without its being infested by some of these small and obscure insects, and thus subject to rejection or confiscation because of contamination by insects after canning or other processing. Even though repeated applications of insecticides are made and the harvested crop is thoroughly washed and otherwise treated to eliminate the insects, it is practically impossible to remove all their invisible parts from the canned product. It is even more difficult to process frozen vegetables that are entirely free from some of these microscopic insects. The fresh vegetables that the housewife procures directly from the home garden or from the market—though ever so carefully washed—probably contain many more insects than the frozen or canned vegetables, which are washed and cleaned much more thoroughly by machinery than they can possibly be in the household kitchen.

As a very small boy on the farm, I once inquired as to why black pepper was always applied to cooked cauliflower and was told that it was useful in covering any cooked aphids that may have escaped washing prior to cooking and that much resembled the small black specks of this well-known condiment. (I was also assured that the cooked aphids would not injure me in any way.)

In the long-time protection of the public health, is it better to allow people to follow the custom of our ancient and immediate ancestors in consuming harmless quantities of minute insects, or small parts of large insects that are normally associated with and contaminate food products, or to accept the alternative: the consumption of residual quantities of the numerous poisonous insecticides that are now being used to control insects—the use of certain of which has been made legal? The answer would seem to be an earnest attempt on the part of all of us to accept the least-hazardous procedure and to consider this difficult situation in as perfectly sane and reasonable a manner as may be warranted after adequate research.

This discussion would not be complete without some reference to the actual use and value of insects as food for man. Many species have been

consumed by all races, past and present. Examination of historical documents, including the Bible and numerous works on ancestral man, reveals the important dietary uses of insects. Our University of California anthropologists tell us that three fourths of the food of certain of our West Coast Indians was vegetable and one fourth animal, the latter of which consisted chiefly of insects.

Probably the most generally available and preferred insects the world over were locusts, or migratory grasshoppers. They often appeared unexpectedly from unknown lands in countless numbers, and, although in most cases they destroyed the local vegetation, including crops, they became food for hungry human beings. Local and migratory locusts were a regular article of diet to savage and civilized peoples in Asia, Africa, North and South America, Australia, and certain of the Pacific Islands. Even in recent years the abundance of grasshoppers in the Philippine Islands may be ascertained from their retail price in the public markets of Manila and other cities. Because grasshoppers are quite large and easily captured, they are readily available with little effort on the part of the laziest person. Dr. A. C. Baker, U. S. Bureau of Entomology and Plant Quarantine, writes to me as of July 28, 1948: "The market in the city of Oaxaca [Mexico] sells grasshoppers prepared for food. They are all ready to use and a sort of brownish red. They are quite good."

Many kinds of fresh-water insects are also consumed by both Eastern and Western peoples. In China and other parts of Asia the vast populations living on the banks and in houseboats on the lower reaches of the great rivers are favored during certain parts of the year by an inexhaustible supply of the large predaceous water beetles. They are consumed fresh, cooked, dried, pulverized in soups, and in other ways. Species eaten at the present time by the Asiatics include *Hydrous pallidipalpis* MacLeay, of North China and Tibet; *H. bilineatus* MacLeay, of South China and Indo-China; *Cybister bengalensis* Aubé; *C. guerini* Aubé; *C. japonicus* Sharp; *C. limbatus* Fabricius; *C. sugillatus* Erickson; and *C. tripunctatus* Olivier (Hoffmann, 1947). In China at the present time no less than fifty species of insects are not only consumed in the daily diet, but make up a considerable part of the total food intake. Some species are also considered to have unquestionable medicinal value.

The water bug known as the giant fish killer, *Lethocerus indicus* Stål, a preferred dietary item of tropical China, India, and Australia, attained a length of 4 inches. A similar water bug, *Lethocerus grande* (Dimmock), of South America, is even larger, measuring up to 4.5 inches, in North America *L. americanus* (Leidy), the commonest species, were and are still eaten by man.

In Mexico the water bugs (*mosco*), especially the Aztec water boatman, *Krizousacoria azteca* Jaczewski, the Mexican water boatman, *K. femorata* Guérin, and the commercial boatman, *Corisella mercenaria* Say, together with the Texcoco back swimmer, *Notonecta unifasciata* Guérin (Ancona, 1933), and their eggs (*ahuautle*) and young, occur in tremendous quantities in Lake Texcoco and Lake Chalco near Mexico City. They have been used for human food from ancient times, and are still so used in Mexico. They are also exported by the ton to other parts of the world, where they are sold for fish, turtle, and bird food. Concerning the use of these, Dr. Baker further writes: "The eggs of the water bugs are gathered, and you can make a sort of caviar out of them. I have a jar of them over at the house now. Here in the laboratory at the moment, I happen to have a paper bag full of the water bugs themselves which are sold in markets in some of the towns for chicken feed."

In California, the Indians of the Mono Lake region gathered and ate the pupae of the ephydrid fly, *Ephydra hians* Say, which were washed up in windrows on the shores of Mono Lake by windstorms. They are the size of kernels of wheat and are apparently very nutritious. Wild animals that ate them grew fat during the periods of their availability, or so it has been said.

The large fat grubs and also the pupae and adults of many beetles have been consumed fresh, dried, or parched. The most striking examples were those of the palm rhinocerus beetle, *Oryctes rhinoceros* (Linnaeus), of tropical Asia; the American sugar-cane beetle, or *grou-grou, Rhynchophorus palmarum* (Linnaeus), and the Hercules beetle, *Dynastes hercules* (Linnaeus), of the tropical Americas; and the giant atlas beetle, *Chalcosana atlas* (Linnaeus), of tropical Africa.

Of the caterpillars of butterflies and moths, all kinds were eaten either fresh, toasted, dried, boiled, or roasted. Hairy tent caterpillars of North America—especially California—which were abundant in the spring, were singed to remove the hairs and roasted before the fire by the Indians. The large larvae and pupae of the pine pandora moth, *Coloradia pandora* Blake, were dried and consumed alone or mixed with acorn meal by the aborigines of Western North America.

In Mexico City one of the delicacies often

praised by visiting tourists is *los gusanitos del maguey*. These are large white caterpillars that mine the fleshy leaves of various species of *Agave*. They are often offered for sale along with pulque. These caterpillars, or maguey worms, are usually fried; they are also canned and sold in grocery stores. The adult is a skipper butterfly scientifically known as *Acentrocneme hesperiaris* Kirby (*Aegiale kollari* Felder, *Teria agavis* Blasquez).

Termites have been an important food wherever these insects occur in tropical and temperate regions. All castes and states of them were eaten—mostly alive. The large queens of the mound-building species, occurring in tropical Africa, Australia, and elsewhere, were considered a very great delicacy.

Various species of lice infesting the bodies of humans were eaten by all aboriginal races. They were devoured not only because they had a pleasant "nutty" flavor, but also as a ritual among many people.

Countless other kinds of insects not only satisfied the appetite, but they also no doubt furnished the much-needed vitamins and other nutritional requirements of our ever-hungry ancestors. The recent discovery of vitamin F in fireflies, or lampyrid beetles, is but one instance of other valuable uses of insects.

In addition, insects furnished honeydew, the much-appreciated "manna" of the Israelites, and also honey, the most important sweet available up to about 1420, when cane sugar became a commercial product. The honeybee is probably the oldest animal domesticated by man. It was cultivated by the ancient Egyptians and Chinese, and, with the beginning of recorded history, this remarkable insect appears in the literature of all the important civilizations of the world. Many strains of the honeybee have been developed in different countries in the African-Eurasian continental areas. This important insect was unknown to the aborigines of the Americas and Australia, although there are small, inferior, stingless honey-producing bees in South and Central America. Until the time of Queen Elizabeth honey was the only readily available sweet and was used only by royalty, the clergy, and the upper classes.

Bristowe (1932) investigated the insect-eating habits of the Laos peoples in Siam and joined with them in eating many kinds of insects, spiders, centipedes, and other invertebrates. Of insects, the natives regularly ate the young and adult forms of grasshoppers, crickets, mantids, cockroaches, termites, dragonflies, moths, butterflies, beetles, weevils, ants, bees, wasps, and many others. Bristowe found most of them palatable and nourishing and liked many of them.

The total ash, or dry weight, of insects varies from 0.41 to 15.49 percent, there usually being more in the adults than in the larvae and pupae. The body chemicals include potassium, sodium, magnesium, calcium, zinc, copper, boron, aluminum, silicon, titanium, lead, phosphorus, arsenic, sulphur, chlorine, iodine, fluorine, manganese, iron, and nickel.

The *nitrogen* content of insects fluctuates during the development period and in the same insect may vary from 11.23 percent in the caterpillar to 8.81 percent for the pupae, and 10.49 percent for the adults by live weight.

The *protein* percentage in live weight varies from 13.22 percent to 17.13 percent.

Fats, which are the accumulating reserves, are very important components in the developing larvae and pupae of insects and may also constitute a major part of the adult body, especially the mature females. Silkworms contain 17.78 percent; their pupae, 43.43 percent; the moths, 24.21 percent; honeybee larvae, from 4 to 21.3 percent; and dry Melolontha beetles, from 10 to 11.5 percent.

Among the *carbohydrates*, glycogen is universally present in insects and may be very high: up to 31.1 percent in the larvae of the horse botfly, and to 33.48 percent in the larvae of the honeybee. Glucose is reported present in some larvae and is always present in adults. Sugar is found in small quantities in the blood.

Enzymes of various kinds are to be found in insects. Lipase, acting on fat, has been recorded from many orders. Cellulose, cytase, xylanase, inulase, diatase, glycogenase, dextrose, raffinase, melezitase, invertase, maltase, trehalase, lactase, glucosidases, formizyme, and others have been studied. Enzymes acting upon proteins and their derivatives, including coagulating types, rennet, proteases, trypsin, pepsin, erepsin, fibrin protease, fibroni, protease, asparaginase, haemolysius, and others, also occur in insects. In addition to enzymes there are also oxylases, tyrosmase, catalase, aldehydase, and zymase.

Important vitamins are found in insects, but these are not necessarily the same as those occurring in human beings.

Other reasons why insects must constitute an important source of nutritious food are indicated by the fact that many species of birds are able to rear their young only upon insects. Insects also are the chief food for many other land and aquatic

animals. Brues, in his *Insect Dietary* (Harvard University Press, 1947), indicated the value of insects as food for man in a somewhat different way: "Crude materials that are transformed into the bodies of our various food animals, especially fishes, and birds whose flesh later finds its place in culinary art."

INSECTS AS MEDICINE

Civilized and uncivilized peoples all over the world have employed insects for medicinal purposes almost as extensively as for food. Practically all those previously mentioned for culinary purposes were used for many real and imaginary ailments of the human body. In addition, many species that were not particularly palatable, and some that were downright horrible-tasting, were employed by housewives, conjurers, and doctors, often with surprising results.

INSECTS AS FILTH

There are a considerable number of insects that may justly be regarded as filth. All those species that live in manure, sewage, and other environments that may be sources of bad odors, tastes, and diseases should be so classified. Examples are cockroaches, earwigs, maggots of certain blowflies, flesh flies, botflies, warble flies, carrion flies, and dung flies. Others, sucking lice, bird lice, bedbugs, and fleas, are also obnoxious. The excrement of all insects and other objectionable products, such as spittle, webbing, offensive and poisonous glandular fluids, stinging hairs, and the like, may certainly be classed as filth.

Great masses of insects, however, are probably as nutritious and as wholesome as are oysters, crabs, lobsters, and even other types of meats ordinarily eaten by civilized human beings. Extensive investigations should be undertaken to explore the true value of insects as possible sources of food for the peoples of the world, as well as their possible harmful effects upon the human system if used in the dietary.

THE THEOBALD SMITH AWARD

After a lapse of five years, owing to the war, the Theobald Smith Award in Medical Sciences, established in 1936 by Eli Lilly & Company, will again be given at the Annual Meeting of the AAAS. Fellows of the Association should submit names of proposed recipients to Dr. Gordon K. Moe, Medical School, University of Michigan, secretary of Section N, with full information (in triplicate) concerning personality, training, and research work of candidates. The award will be $1,000 and a bronze medal, given for "demonstrated research in the field of the medical sciences, taking into consideration independence of thought and originality." An additional amount of $150 is available toward traveling expenses. The recipient must be less than thirty-five years of age on January 1 of the year in which the award is to be made and a citizen of the United States.

Past recipients are Robley D. Evans, Charles F. Code, Albert B. Sabin, Herald R. Cox, and Sidney C. Madden.

THE SCIENCE THEATRE

Scientific laboratories—academic, government, or industrial—or any organization having new and exceptional 16mm scientific motion pictures—either with or without sound—may arrange showings of their films at the Annual Meeting of the AAAS, New York City, December 26–31, 1949, by writing to

The Science Theatre
American Association for the Advancement of Science
1515 Massachusetts Ave., N. W.
Washington 5, D. C.

ORIGINS OF GEOLOGIC TERMS

FREDERICK A. BURT

Professor Burt (M.S., Chicago, 1929) is professor of geology at the Agricultural and Mechanical College of Texas, where he has been on the staff since 1921.

GEOLOGISTS whose basic undergraduate studies have been in liberal arts, and college students in the classical curricula who take geology as a science elective, are sometimes struck by the fact that geologic nomenclature has drawn much more heavily from the Greek than from the Latin roots and combining forms. Among beginning students the understanding of new terms is facilitated by an explanation of their basic meanings. As an illustration, a few days ago a class in Historical Geology was being introduced to the subject of Ordovician life. The term *Orthoceras* was explained as coming from two Greek roots meaning "straight" and "horn." The members of the class were then reminded of the mineral term *orthoclase,* with which they were already familiar, as being derived from the same root for "straight" combined with the root meaning "break," referring to the smooth right-angle cleavage of the mineral. Experience has shown that such explanations serve to fix attention upon some diagnostic characteristic of the object or process under consideration.

Among advanced undergraduates, whose training is usually technical rather than liberal, there is often a manifest desire to become acquainted with the inherent meanings of technical terms for the better appreciation of the thing for which the term stands. This is in part, perhaps, the expression of an unconscious desire for a touch of liberal learning. The greatest interest I have met has been displayed by my graduate students in the History of Geology Seminar. To these graduates the adoption of a name for a newly discovered feature or structure, or to apply to a newly determined process, is a vital part of the history of the subject. It is also a part of that intangible "academic knowledge" that is fascinating to even the most rigorously trained scientific mind.

Technical geologic terms may be grouped, on the basis of their origin, into five classes, as follows:

1. Terms that are adoptions and adaptations of words from the vernacular.

Such terms are more abundant, after excluding the names of formations and the specific names of minerals and fossils, than those of any other class. They come from all the important European and from many of the non-European languages. They occur most frequently as narrowly limited usages of English, e.g., *joint;* German, e g., *graben;* French, e.g., *dune;* or Italian, e.g., *lava.*

Physical geography has played an important part in the establishment of such terms. This is especially true in geomorphology. The percentage of words from the French vernacular descriptive of glacial phenomena, such as *moraine,* is very high and reflects the early growth of glaciology in the Alps. The influence of the physical environment is shown again in the surprisingly high percentage of terms descriptive of aeolian processes and desert environments that have come from the Arabic, e.g., *hamada*; and from the Ural-Altaic group of languages, e.g., the Turkish *barchan.* Scientific explorers sometimes introduced such terms into the vocabulary from the languages of primitive peoples; for example, *nunatak* from the Greenland Eskimos.

The seventeenth- and eighteenth-century industrial backgrounds greatly influenced the development of vernacular-derived terminology in mineralogy and in physical geology as applied to mining. This is evidenced by such words as the English *fault,* the Cornish *gossan,* and the French *gangue* from the German *gang.*

2. Words created by changing a proper noun to a common noun or adjective standing for a type of phenomenon exemplified by the geographic feature which the proper noun designates.

Thus we have *meander* from the Meander River and *monadnock* from Mount Monadnock. This type of term formation is illustrated in paleontology by such fossil names as *Beltina* from the

Little Belt Mountains; in mineralogy, by *labradorite* from Labrador; in petrography, by *syenite* from Syene; in geomorphology by *monadnock*; and, in economic geology, by *artesian* from Artois.

The outstanding development of this type of terminology is seen in stratigraphy. This method of forming terms acquired great importance after the work of William Smith, who made it a policy to apply place names to formations such as his *Cornbrash* and *London,* from type areas in which the formations outcropped. Although the idea of stratigraphic place names did not originate with him, he deserves great credit for the impetus he gave to their usage.

3. Names given to geologic structures or materials for the purpose of honoring an outstanding member of the profession.

Such names are most common in paleontology, where a genus name is sometimes so formed—as *Murchisonia* after Sir Roderick Murchison—but more frequently in the formation of specific names—as *Oterceras woodwardi* for John Woodward. The method is occasionally seen in the construction of mineralogical terminology, as is evidenced by *wollastonite* for F. J. H. Wollaston. It is only in rare cases that we have a term in physical geology, as *louderback* in honor of G. D. Louderback, which originated in this way.

4. Words adopted by geological science from physics, chemistry, biology, and other sciences.

Such terms are especially common in mineralogy and paleontology. In these places they often make up the bulk of our nomenclature, as, for example, the chemical names in determinative mineralogy and the solid-geometry terms so common in crystallography.

These sciences have influenced geologic usage not only in the lending of their technical terms but also in the method of name formation. For example, paleontology has in the main followed the biologic method of drawing upon the Greek for genetic and the Latin for specific names.

5. Coined words.

We come here to those terms which have, from time to time, been coined from Greek and Latin roots and which have become established. To answer the question as to why there is this apparently great number of Greek as compared with Latin derivatives, I at one time attempted to arrive at some approximate ratio between the two. To accomplish this a list of words was set up, using as sources one of the standard college texts in each of the following subjects: physical, historical, structural, and economic geology. This list was supplemented by a search of the issues of the *Journal of Geology* and the *Bulletin of the Geological Society of America,* each for the years 1940 and 1941. Only words of obvious Greek or Latin root origin were listed, and all generic and specific names of fossils and names of minerals were excluded. All hybrid terms such as *metaigneous* and *peneplane* were omitted, the latter being a case in which the spelling has not been standardized (the controversy over *-plane* and *-plain* never having been settled to everyone's satisfaction). Such terms as *orogenic, orogenetic,* and *orogenesis* were counted as one, being considered as variants of the same word. Words merely cognate to others, however, were included. By this method it is believed as representative a cross section of the nomenclature was obtained as would be possible without much exhaustive study. Terms of Greek origin were found to make up 73.6+ percent, and Latin 26.3+ percent, of the whole. Although all sciences have numerous terms of both Greek and Latin origin, the superficial evidence, based on impressions rather than statistics, is that outside of geology there is no such preponderence of Greek-derived words as is here shown.

Studies in the history of geology have led me to adopt the viewpoint that this emphasis on Greek is due partly to the prejudices of some of the founders of the science. At the time geology was in its adolescence a great many British clerics were attracted to it as an avocation. Many of these men developed into professional geologists, and others made large contributions as amateurs. This period coincided in part with the English theological controversies culminating in the so-called Puseyism movement (after its later leader, the Rev. Edward Bouverie Pusey). The struggle was between High- and Low-Church Anglicism. The feeling that developed within the Low-Church element, to which these geologic clerics belonged, against both Roman Catholicism and the High-Church party was intense and included an emphasis on the original Greek origins of the New Testament and the conduct of services in the English vernacular in contrast with Latin masses and translations. This attitude was reflected in stress on the Greek classics and a prejudice against Latin origins of all forms. These theologically educated geologists, of whom William Buckland, William Conybeare, John Mitchell, and Adam Sedgwick were but four outstanding examples among many,

contributed much to geological nomenclature. It seems more than likely that the impetus given to preference for Greek over Latin in these early days of geologic science has continued down to our own times, not only through the carry-over of early terms, but also through the momentum toward Greek origins then established.

Something may be in order here concerning a considerable number of such terms as *drift* as applied to glacial deposits, and *Tertiary,* as a name for a stratigraphic system. These terms came into usage at a time when our interpretation of facts was such that their inherent meanings were believed to be appropriate. The advance of our knowledge has necessitated new interpretations, but the terms have stayed with us—with greatly modified meanings. They have thus become dissociated from everything suggested by their underlying meanings. Many attempts to replace such terms have been made, but in these and some other cases the words have become so firmly embedded in the nomenclature that replacement seems well-nigh impossible, and perhaps now inadvisable.

An occasional change in the diametrically opposite direction seems regrettable. Such a change is illustrated by the almost complete replacement of the meaningful English *boulder clay* by the less obvious Scotch *till.* The former term has been criticized on the basis of the fact that not all unassorted drift is a mixture of boulders and clay. No term, however, fits perfectly in its final analysis, and *boulder clay* has the virtue of inherent clarity throughout the English-speaking world. The general picture it presents of the variability of size range among its constituents is significant.

The field of etymology as applied to geology is a very small one, but it can serve as an interesting area of study capable of contributing many values to the science.

DUNES: MADE, UNMADE

Compounded of coral dust,
Of silica and dead seashell,
These gull-winged hills
Affirm the living matrix of their birth.

Like deer before the hounds,
They flee in frenzied flight
Before the wind and sea,
But fail to throw their scent.

Upthrusting in the mounting light,
Their winded flush gives way
To purple gasps, then pallor
As they trip, down-dip.

They roll and rise
To sounds of spray and air
Which warn of new assaults,
A never-ending chase.

JOSEPH HIRSH

EFFECTS OF MECHANIZATION ON AMERICAN AGRICULTURE

ROBERT T. McMILLAN

Dr. McMillan (Ph D., Louisiana State, 1943) is professor of sociology and rural life at Oklahoma Agricultural and Mechanical College, where he has been on the staff since 1937. He has also been rural sociologist with the FERA and the Land-Use Planning Division, U. S. Resettlement Administration.

THE mechanization of agriculture has been under way for hundreds of years, but it has broadened and accelerated greatly with the invention and use of the tractor. The process includes any shift from hand or animal power to motor power and any use of implements and machines in the operation of farms. Tractor-powered plows, planters, cultivators, and harvesters differ from horse-powered units in the degree of mechanization. Automobiles, trucks, stationary engines, and numerous other mechanical devices, whether power-driven or not, further increase the scope of mechanization.

What are the effects of the tractor and other farm machines generally upon American agriculture? This paper will summarize the major social and economic changes associated with increased farm mechanization during 1920–45 from analyses of selected census data for the United States as a whole, 67 randomly-chosen counties of the nation, and all 77 counties in Oklahoma. It is recognized that the social scientist cannot isolate completely the variable "farm mechanization" from other factors of farm technology nor identify precisely its cause-effect relationships, but he can approximate its concomitants within reasonable margins of error.

The method of study consisted of stratifying states or counties into groups according to the number of tractors per 100 farms as in Figure 1, and of comparing specified items between the longest time intervals—usually 1920–45—for which comparable data were available. The number of tractors per 100 farms was used as a measure of farm mechanization because its correlation coefficients with the average value of farm implements and machinery per farm were 0.89 for the United States, 0.92 for the 67-county sample, and 0.93 among counties in Oklahoma. This index was found to be more satisfactory than other measures which have been used or suggested, such as percentage of farms with tractors, number of tractors or value of farm implements and machinery per 1,000 acres of cropland, and frequency of trucks, automoblies, or electricity on farms.[1] Because of fluctuating dollar values, it is a more suitable index than value of implements and machinery. Generally, the findings were in agreement among the three areas studied, thus meeting the requirement of verification so essential to scientific research.

It should be kept in mind that the period included in this study was marked by violent economic fluctuations resulting from the deflation in agriculture following World War I, the general depression of the 1930s, the New Deal, and the second world war boom. Whether these uncontrolled factors limit the validity of the findings of this study can only be conjectured.

INCREASE AND DISTRIBUTION OF TRACTORS

First manufactured commercially in 1903, tractors numbered about 1,000 by 1910.[2] The number reported in the first census count in 1920 was 246,000. The census reported 2.4 million tractors on 34 percent of the nation's farms in 1945, and a preliminary estimate of the USDA indicated 3 2 million tractors on farms in January 1948.

Tractors appeared on wheat farms in the Dakotas and Montana before 1920 and thereafter spread rapidly through the Great Plains, Corn Belt, Lake, and Middle Atlantic states. The South has lagged far behind other regions in tractor use, owing partly to slow adaptation of mechanized methods to cotton production. Heavy losses of croppers and laborers since 1935, resulting from the crop-adjustment programs and alternative employment opportunities, have given impetus to farm mechanization in that region. Both the anticipation and the appearance of tractors on planta-

tions probably motivated many agricultural workers to seek employment in nonfarming occupations. Farm mechanization is now spreading more rapidly through the South than elsewhere, and this accelerated trend there probably will continue for several years.

FARM POPULATION

Decreases in farm population are associated with increased farm mechanization. Between 1920 and 1945, the rural-farm population of the United States decreased 25 percent. During the same period the median decreases among quartiles of states arranged in descending order according to number of tractors per 100 farms were: first, 29 percent; second, 25; third, 18, and fourth, 25. From 1925 to 1940, when the nation's farm population increased generally, losses were reported in 8 of the 12 states with the largest amount of farm mechanization.

The rural-nonfarm population increased proportionately less in the first quartile of states on the farm mechanization index than in the remaining states between 1930 and 1940. This suggests that there is less need for village and other rural-nonfarm people to perform services for the diminishing farm population.

Farm mechanization affects the age composition of farm population by reducing the numbers of children and persons in the younger working ages and, consequently, increasing the proportions of total population in the older ages.

Data to partially support this hypothesis are shown in Table 1 The percentage decreases in farm population from 1920 to 1945 generally were larger for each age group up to forty-five years old in the most mechanized farming states, and in Texas, Oklahoma, and New Mexico, than elsewhere. Other data show that gains in percentages of total population forty-five years old and older during the period were considerably greater among states most affected by farm mechanization. It is conceivable that mechanization, by easing the labor of farming, will lead to a longer earning life among agriculturists. On the other hand, it might enable farmers to retire at earlier ages than in the past

The state-by-state changes in numbers of children under five for each 1,000 women fifteen to forty-four years of age in the rural-farm population between 1930 and 1940 show a slightly higher average reduction among the leading farm-mechanized states than among states in the remaining quartiles. Analysis of variance on changes in

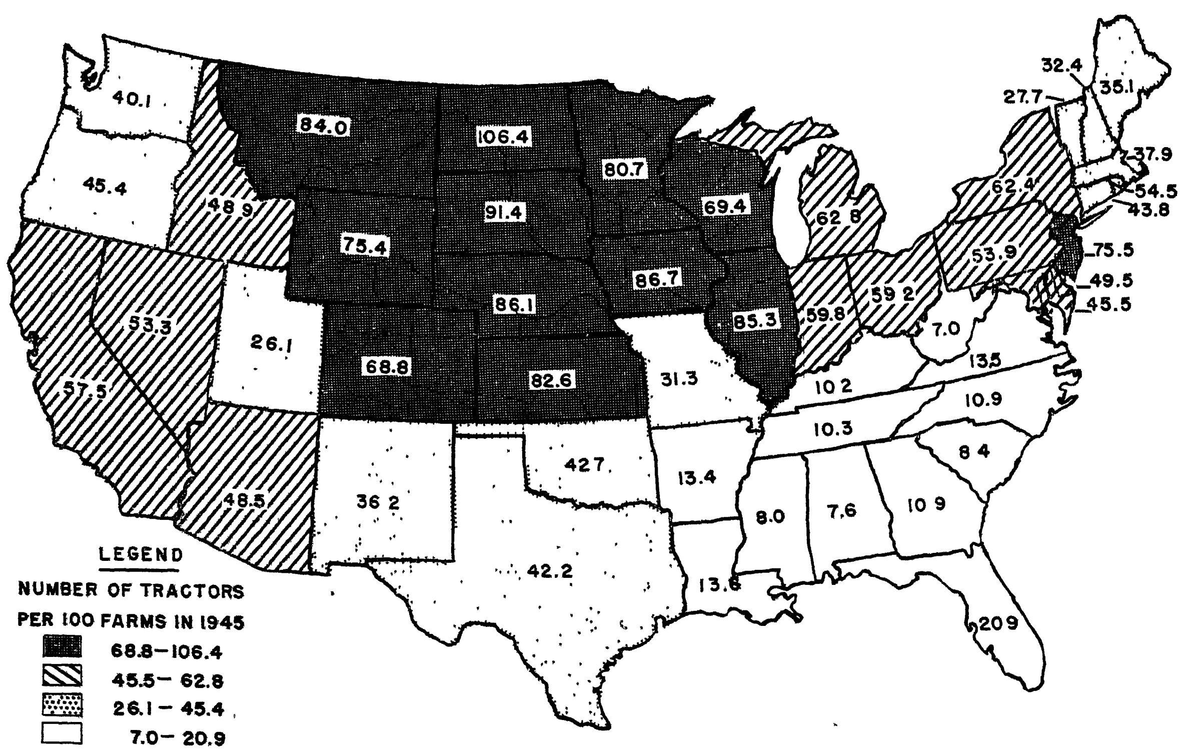


FIG. 1. Number of tractors per 100 farms, by states, 1945.

TABLE 1

PERCENTAGE CHANGES IN RURAL-FARM POPULATION WITHIN EACH AGE GROUP FROM 1920 TO 1945, BY STATES ACCORDING TO FARM MECHANIZATION INDEX

Farm Mechanization Group	Age Group, Years					
	All Ages	Under 15	15–24	25–44	45–64	65 and Over
All states	−23	−34	−39	−23	+ 9	+25
Quartile 1	−25	−28	−41	−28	+14	+37
2	−20	−30	−32	−24	0	+13
3 (nine states)	−13	−26	−36	−15	+17	+36
Texas, Oklahoma, and New Mexico	−33	−46	−53	−29	+17	+40
Quartile 4	−24	−33	−38	−19	+ 6	+20

these fertility ratios, however, reveals nó significant differences among the several states owing to mechanization but highly significant differences owing to urbanization.

The greater reduction of nonwhite than of white farm operators was influenced little by farm mechanization up to 1945. The number of white farm operators in the United States decreased 6 percent, and nonwhite operators 27 percent, between 1920 and 1945. Relatively smaller losses of nonwhite farmers were reported in states with high tractor indexes than for the nation as a whole. Very few nonwhite operators reside in the most mechanized farming areas, however. In Oklahoma and Texas there were comparatively large reductions of nonwhite farmers, but, again, few such farmers ever lived in the mechanized portions. Other casual observations indicate that nonwhite farm operators have not been displaced or replaced in relatively larger numbers than white farmers by farm mechanization. Whether this situation will continue as farm mechanization becomes more widespread in the South remains to be seen.

NUMBER OF FARMS AND FARM TENURE

Decreases in numbers of farms are also associated with farm mechanization, and other factors are operating in the same direction. Although farm mechanization appears to be chiefly responsible for the displacement or replacement of farm families, crop-adjustment programs, alternative employment opportunities, and droughts led many thousands of low-income families to leave agriculture during the period studied.

The number of farms in the nation declined 9 percent between 1920 and 1945, although more farms were reported in 1935 than in other census years. The corresponding state-by-state decrease averaged 12 percent for the upper quartile of states on the tractor index. The trend toward fewer farms has been accompanied by two divergent tendencies: an increase in small part-time or subsistence farms, chiefly near cities, and an increase in large farms. Between 1920 and 1945, the number of farms in the nation with 500 acres and over increased from 217,000 to 287,000, a gain of 32 percent. The increase in the 12 states with the highest tractor indexes, though smaller relatively than for all states, accounted for 45 percent of all large farms added during the period. Another 29 percent of the total gain of farms with 500 acres and over occurred in the Plains states of Oklahoma, Texas, and New Mexico, where farm mechanization has spread rapidly in certain areas.

A direct relationship exists between number of acres per farm and number of tractors per 100 farms in each size group.

Changes in farm ownership and tenancy appear to be retarded by farm mechanization. Of all farms in the United States, 68 percent were owner-operated in 1945 as compared with 61 percent in 1920. Among the 12 states with the highest tractor indexes, there was less than one percentage point gain in the proportion of farms operated by owners. Larger gains were reported for all other groups of states. In the 67-county sample and in Oklahoma, increases in proportions of farms owner-operated were much smaller in highly mechanized farming counties than elsewhere during the period studied.

Other data disclose that share and share-cash tenancy strongly persists in states with the most farm mechanization. Cash tenancy and other forms of rental agreements have become increasingly popular in the less mechanized areas, though share tenancy still is most prevalent in the United States.

Census data fail to support the popular belief that farms operated by nonresidents are increasing

more rapidly in the states with the most mechanization.

Farm laborers probably have been affected more than any other tenure class by farm mechanization. The numbers of farm wage laborers in the nation decreased 29 percent between 1930 and 1940. The reductions varied directly with amount of farm mechanization, the most-mechanized farming states losing 41 percent. Although the definition of wage laborers and the age groups included in this occupational class differed in the two censuses, the data are comparable between areas.

Changes in numbers of farm operators working off the farm and in the time spent in this employment are responsive to farm mechanization. Relatively fewer farmers were employed in work off the farm in 1944 than in 1934, the reduction being much greater in the top 12 states on the mechanization index than elsewhere. Also, the number of farm operators working 100 days or more off the farm increased much less in the leading farm-mechanized states than in other states. During the 1930s many farmers supplemented low farm incomes with work off the farm; in the prosperous 1940s they were devoting more time to their farms, especially where units were becoming larger along with increased mechanization.

LAND USE

Increases in total acreage of farms and relatively small decreases in cropland acreage accompany farm mechanization. The greatest gains of total acreage in farms between 1920 and 1945 were made in states of quartile 1, and in the plains of western Texas and eastern New Mexico, where mechanized farming is widespread.

Total acreage of cropland in the 12 leading farm-mechanized states decreased less than in the remaining group of states from 1919 to 1944, and in the latter year accounted for 45 percent of the nation's total cropland as compared with 41 percent 25 years earlier. More land was under the plow in the Great Plains states, Kansas and Oklahoma excepted, in 1944 than in 1919.

Cultivable land has been shifted to those crops adapted to farm mechanization. The 12 states with the most farm mechanization had 59 percent of the nation's wheat acreage harvested in 1944 as against 57 percent in 1919. Corn rather than wheat is now the leading crop of the highly farm-mechanized states, with 51 percent of the nation's total acreage of this crop harvested there in 1944 as compared with 40 percent in 1919. In Oklahoma, the numbers of acres planted to cotton and wheat have declined precipitously in counties with the least farm mechanization and have risen sharply in areas where farm machinery is used extensively.

Increases in numbers of beef cattle, milk cows, and sheep were associated with increased farm mechanization during the period studied. Despite large decreases in numbers of horses and mules, the 12 states with the most farm mechanization, and Oklahoma, Texas, and New Mexico, had nearly as many "animal units" in 1945 as in 1920, and the average number per farm had increased absolutely and relatively more than in other groups of states.

OTHER FACTORS

Commercialization of farms is a concomitant of mechanization. In 1944, 90 percent of the farm products of the nation, by value, was sold, the remainder being used on farms. On farms in the upper half of states on the mechanization index, 93 percent of the farm products went to market. The trend toward sale of larger proportions of total products raised is accentuated by farm mechanization. Although commercialization of agriculture generally is considered favorable economically and socially, it does result in increased dependence of the farmer's welfare upon fluctuations of the business cycle and upon government, with subsequent loss of traditional self-sufficiency and autonomy.

Labor costs are reduced through use of labor-saving machines. Comparable census data on hired labor are available for the years 1924 and 1944. In the latter year the total amount paid for hired labor per 1,000 acres of land in farms of the United States was 174 percent higher than two decades ago. In the 12 states with the most farm mechanization, the corresponding increase amounted to 124 percent, indicating a savings in hired labor costs of nearly 30 percent. Competition for labor in the more urbanized states of the second quartile resulted in larger-than-average increases in cash expenditures for farm wage work there.

Analysis of data for 67 sample counties shows that cash costs of hired labor comprised the same proportion of the total value of all farm products in each of the two upper quartiles of counties in both 1929 and 1944. For the less mechanized farming counties, the ratio of expenditures for farm labor to total value of farm products increased between these censal years. In general, farm labor costs constituted a much higher proportion of all farm costs in 1944 than in 1929, owing in part to

the limited availability of such items as feed, fertilizer, and machinery in the critical war year.

Wealth and income. Farm mechanization correlates fairly closely with distributions of farm wealth and income. Stated in another way, adequate capital resources are a prerequisite of mechanization. Among all states, correlation coefficients of 0.65 and 0.64 were found between the tractor index and, respectively, the average value per farm of land and buildings in 1945 and average value of all farm products in 1944. These correlations were considerably higher for the 67 sample counties and Oklahoma.

Future analyses of farm mechanization may show that it results in geographical concentration of farm wealth and income. At present the evidence is inconclusive. The 12 states with the most farm mechanization accounted for a smaller proportion of the total value of land, buildings, livestock, and machinery in 1945 than in either 1920 or 1925. One reason for this is that the deflation of land values following World War I was most severe in the West and East North Central states where farm mechanization is general. The aggregate value of farm property increased in the second quartile of states but less than in the lower half of states on the mechanization index. Will history be repeated in the present readjustment period?

With reference to distribution of gross farm income in 1944 and 1929, the 12 states with the most farm mechanization accounted for exactly the same percent (35) of the national total in both years. In Oklahoma, data for the years 1929, 1939, and 1944 indicate concentration of farm wealth and income in counties with most farm mechanization but inconsistent results with reference to the second group of counties.[1]

There is a very definitely vertical concentration of wealth and income associated with farm mechanization. Within each quartile of states the ratio that the value of land and buildings on farms of 500 acres and over is of the total value of those assets on all farms increased between 1925 and 1945 as follows: first quartile, 56 percent; second, 53; third, excepting Oklahoma, New Mexico, and Texas, 49; and, fourth, 47. For the 3 states in the Southern Great Plains, the increase in this ratio was 88 percent.

Large production at high prices accounted primarily for sharp increases in farm income during the recent war and postwar boom. But farm mechanization also played an important part. The number of farms with farm products sold or used valued at $6,000 or over increased 231 percent between 1929 and 1944 in the 12 states with the most farm mechanization. The corresponding increase for the nation was 193 percent.

Level of living. The level of living of farm operator's families correlates closely with amount of farm mechanization. The coefficients of correlation between Hagood's level of living index for farm operators in 1945 and the tractor index were 0.70 for the United States, 0.67 for the sample counties, and 0 89 for the counties in Oklahoma;[3] however, the relative increases in proportions of farms with telephones, electricity, and running water in dwellings, and automobiles, between 1920 and 1945 were less in the 24 states with the highest mechanization indexes than in the remaining states. The incidence of these items is related more closely to degree of urbanization in each state than to mechanization.

Several possible reasons can be offered for the relatively slow improvement in levels of living of highly mechanized farming areas. The gains which might have been expected to accompany increased incomes from mechanized farms were not realized because, first, the recent transition to larger and more highly mechanized farms, owing to large capital outlays involved, may tend to retard a direct rise in the level of living; second, the cash costs of operating mechanized units may be so high as to reduce the *proportion* of total farm income available for family living, especially in periods of relatively low or falling prices for farm products; and, third, with rising price levels in recent years, farm families have been liquidating indebtedness on farms and chattels incurred during the depression of the 1930s. When the initial costs of farm mechanization and additional land purchases are paid off, possibly the relative amount of farm income available for family living will increase.

Farm families probably have more time for participation in leisure, religious, and other social activities as a result of mechanization. In Oklahoma, farm mechanization has facilitated consolidation of small country schools with village and city systems. The largest losses of these schools have occurred in that part of the state most affected by widespread use of the tractor and other farm machinery. Open-country churches have all but disappeared in those areas where the invasion of power machinery has displaced much of the population. This townward movement of schools, churches, and recreational facilities suggests that farm people, through increased contacts with vil-

lage and city populations, will become more urbanized in attitudes and behavior.

FUTURE OF FARM MECHANIZATION

Probably the nation is past midway in its farm power revolution, exclusive of possibilities relating to atomic energy. One authority predicts that by 1975 there will be 5 million farm tractors, other than garden tractors.[2] The continued invention of useful machines and implements, and increased electrification, should give further impetus to farm mechanization.

The diffusion of farm tractors and other machines into new areas may proceed more slowly than in areas already mechanized because of the following factors, if applicable:

1. Prevalence of relatively small farms and limited working capital per unit;
2. Greater density of population on farms;
3. Uneven to hilly topography;
4. Diversified or general farming, which sometimes is not easily adapted to use of tractors; and,
5. The plantation system, with its traditional dependence on comparatively cheap labor.

Farm mechanization has produced savings in labor, increased efficiency of farming operations, and resulted in greater productivity, consolidation of many uneconomic farming units, and improved levels of living among farm families remaining in agriculture. On the other hand, with mechanization have come a capacity to produce more food and fiber than can be consumed under effective peacetime demand, potential unemployment of agricultural workers, concentration of farm ownership among fewer landholders, increased social stratification of tenure classes, and depopulation of farms and villages, which are service centers for farmers. As yet, the full impact of farm mechanization has not been felt because of World War II.

REFERENCES

1. McMillan, Robert T *Social Aspects of Farm Mechanization in Oklahoma.* Oklahoma Agri. Exp Sta. Bull, in process.
2. Cooper, Martin R., Barton, Glen T., and Brodell, Albert P *Progress of Farm Mechanization.* U.S.D A. Misc. Pub. 630, Washington, D. C., Oct. 1947.
3. Hagood, Margaret Jarman. *Farm Operator Family Level of Living Indexes for Counties of the United States, 1940 and 1945.* U.S.D.A., Bur. of Agri. Econ., Washington, D. C., May 1947. (Mimeo.)

CANCER RESEARCH

A grant from the Atomic Energy Commission, through the Office of Naval Research, will enable the University of Delaware to continue research of potential significance in cancer control. Studies in progress in the Department of Biological Sciences, by Dr. Mary A. Russell, on the embryonic development of corn seedlings have revealed certain growth interference phenomena resulting from treatment with X-rays, neutrons, and mustard gas compounds. "The understanding of this interference upon actively growing cells," according to Dr. James C. Kakavas, chairman of biological sciences at the University, "will enhance our knowledge of the behavior of embryonic tissue to physical and chemical agents. These observations will have direct bearing on the cancer problem, since cancerous tissue simulates embryonic tissue development."

The Commission's grant is the third received by the University of Delaware in recent months. The other two were made by the American Cancer Society for a preliminary study of the effects of beta rays on living cells, a project directed by Dr. Harold Feeny, associate professor of physics.

THE APPROACH TO THE ABSOLUTE ZERO OF TEMPERATURE

JOHN F. ALLEN

A Canadian (Ph.D., University of Toronto) who has done research in low-temperature physics both in this country and abroad, Dr. Allen became professor of natural philosphy at St. Andrews University in 1947. His article is based on his inaugural addresss.

IN ALL the ages of recorded history, man appears to have striven for ultimates or extremes of qualities or quantities. At different times the striving has been for different things, but in the present age perhaps the greatest intellectual excitement has been manifest in the pursuit of the ultimate in knowledge of the physical world.

Among the more abstruse of these searches for the ultimate is the exploration to its farthest limits of that strange region of complete cold, where the perpetual St. Vitus dance of the atoms is stilled and where new and unforeseen phenomena which are obscured by the thermal agitation of the atoms at higher temperatures remain to be found.

If we want to put the St. Vitus dance in more pedantic language, we can say that heat consists in the random or disordered translational motion of atoms or molecules, and that the intensity, or the energy of that motion, is a direct measure of temperature; in fact, it is the definition of temperature. We stress the word *disordered*. Ordered molecular motion would simply appear as, say, a stream of gas or liquid. Such ordered motion represents energy which is capable of doing work, but it has no meaning from the point of view of heat or temperature. Temperature is thus a statistical concept, arising from consideration of the purely random and, in detail, unpredictable motion of the very large number of molecules usually present in any physical system which we are accustomed to contemplate.

Since temperature is a measure of the average random energy of a molecular assembly, we can fairly safely assume that there is no upper limit to it, for there is no reason to suppose that there is any upper limit to the kinetic energy which a molecule may possess. At the other end of the scale, we can conceive a definite limit. If the molecules are at rest, they obviously have no kinetic energy and therefore no temperature, and we can therefore define an absolute zero of temperature in such terms. Not only can we define such an absolute zero, but we can predict its position with considerable accuracy. Such a phenomenon is unusual in the physical world, and the absolute zero is in fact one of the few singularities, and probably the most important singularity, in nature.

The odd thing about the absolute zero is that, although we can calculate its position fairly accurately, so that we know where it is, we cannot and never shall be able to reach it, although we are able to get very close to it indeed. If we determine its position on the centigrade scale of temperature we find it at $-273.16°$, with some uncertainty in the last figure.

The centigrade scale is, of course, purely arbitrary, and it has now been replaced in physics by the absolute, or thermodynamic, scale, derived by Lord Kelvin, which properly gives the value zero to the absolute zero and therefore reckons all temperatures as positive.

If we examine the absolute scale of temperature, the first thing we notice—and it is obviously a trivial point since the scale extends upwards indefinitely—is that we are living at a temperature fairly close to the bottom—in fact, about halfway between zero and the melting point of lead.

The next point to which we must draw attention, and this is the crux of the matter, is that although it is relatively easy to move up the temperature scale, it is much more difficult to move down, and it becomes progressively more difficult the farther down we get. We cannot here illustrate both these statements directly, but the first is simple enough. If we strike a match, the comparatively small amount of work done in friction on the match head, combined with chemical action, produces at once a source of heat at a temperature of something like 2,000° absolute (K).

If we try, however, to achieve low temperatures by chemical action, say, by employment of the heat of solution, we could not get very far below the freezing point of water. To achieve lower tempera-

tures than this we must use purely physical means, and to attain really low temperatures, say, those of liquid hydrogen or helium, the apparatus must be quite bulky and powerful.

I

Why should it be so much more difficult to attain low than high temperatures? To understand the reason we must look again at the connection between temperature and the random motion of the atoms or molecules. Heating a body not only increases the energy of all its constituent particles, but it also increases the disorder in their motion. Conversely, if we cool a body, we diminish the energy and also—and this is really more important—we diminish the disorder in the system.

We give a name to the degree of disorder in a system. We call it the entropy, and entropy is a direct function of the temperature. We can illustrate this by means of a rather crude experiment. Suppose we have a tray containing white and red balls, which represent the molecules of a gas. Initially they are in a state of perfect order, but as soon as we shake the box, they begin to take up random motions and acquire a measure of disorder, and hence entropy. No matter how much or how long we shake the box, the balls are most unlikely to come back to their former ordered position. In other words, the probability of their coming back is small, or we may say that a disordered state is a highly probable one and is a state of high entropy. Furthermore, since any change anywhere in the universe is likely to take the form of the most probable one, then one can state a fundamental postulate of physics, that the entropy of the universe is continually increasing. This does not mean that the temperature of the universe is increasing, but that the degree of disorder is increasing since, although high temperature implies high disorder, high disorder does not necessarily imply high temperature. We shall enlarge on this point shortly, but meanwhile it might be instructive to ponder whether this fundamental postulate of a tendency to increasing disorder in the universe does not extend beyond the world of the physical into that of the sociological.

We see, then, that to lower the temperature of a body we must diminish its entropy. If entropy were a function of temperature only, we should never be able to attain any temperature lower than that our surroundings, but we have just mentioned that if such were the case it would imply a continually increasing temperature in the universe, which is absurd.

Luckily, the entropy of a system is a function of other things, of which one of the most important for our purpose is the volume. If we diminish the volume occupied by a given number of molecules we increase the order and hence diminish the entropy. One can see it in the following way. The degree of order can be expressed as the probability of finding a molecule at a given spatial position in a given volume. If the volume available to the molecule is reduced, then the probability of finding the molecule at the given position is increased, or, in other words, the degree of order is increased.

Since by doing a certain amount of work we can diminish the volume occupied by a given quantity of gas, we can therefore reduce its entropy. But, since the total entropy in the universe can never be reduced, the local reduction of entropy we have achieved must be balanced by a corresponding increase elsewhere. If we compress a gas, the "disorder" entropy becomes converted into "temperature" entropy, and the gas becomes hot. This heat can then be transferred to some cooling agent, say, a water bath. In thermodynamical terminology, we have removed some entropy from the gas and given it up to the rest of the universe.

If we now have the compressed gas back at room temperature, we can let it expand quickly so that it disorders itself in space once more. But it can only do that at the expense of its remaining "temperature" entropy, and in consequence the gas cools. The whole sequence is called the method of "external work," and if the process is repeated many times sufficient heat can be removed from the gas so that it liquefies. All the gases have been liquefied essentially by this means, and by this means we have been able to attain the temperature of boiling liquid helium at only 4° above the absolute zero.

These ideas and achievements were not, however, reached in a short space of time, and we must now go back into the past and see how man strove by speculation and experiment to understand the phenomenon of cold.

II

Robert Boyle was probably the first philosopher who so speculated and experimented, and to him the phenomenon was most puzzling. In 1682 he described some of these experiments to the Royal Society in a paper entitled *On Observations Touching Cold*. He discovered the refrigerating properties of an ice-salt mixture and observed that salts which did not dissolve in water produced no cooling. He observed the expansion of water when it

turned to ice and was mystified by the force with which the freezing water burst even strong iron vessels. Describing cold, he confessed that "he never handled any part of natural philosophy that was so troublesome and full of hardships."

Amontons, in 1702, discovered the air thermometer and conceived the idea of an absolute zero, where the volume of air in the thermometer would apparently vanish. He deduced the zero of temperature to be at what corresponds to $-240°$ C. He was ignored until in 1779 Lambert repeated his experiment more accurately and obtained a value corresponding to $-270°$ C, which he called the point of absolute cold.

There was some disagreement with this, and Lavoisier and Dalton did not admit that the gas thermometer was an absolute instrument. They tended, for reasons that are somewhat obscure, to put the absolute zero at what would be about $-3{,}000°$ C. It was not until 1848, when Lord Kelvin produced his celebrated treatise on the thermodynamic temperature scale, that the ideal gas thermometer was proved to be an absolute instrument, and the absolute zero fixed in the neighborhood of $-273°$ C.

As for the history of the production of cold, it is practically synonymous with the liquefaction of gases. Late in the eighteenth century, Cullen, teacher of Black, showed that the temperature of ether could be lowered by rapid evaporation, and early in the nineteenth century Leslie and Wollaston succeeded in freezing water by this means. In the early 1830s Faraday discovered that some gases would liquefy simply by compression at room temperature. If the pressure were allowed to fall over the surface of such liquids, then rapid boiling took place, accompanied by a drop in temperature. Using some of these gases as refrigerants while compressing others, Faraday, Davy, and Thilorier succeeded, between 1830 and 1844, in liquefying all the known gases except hydrogen, nitrogen, oxygen, carbon monoxide, nitric oxide, and methane. In this period the lowest temperature reached was $-80°$ C, with a mixture of solid CO_2 and ether.

In the 1860s and 70s the work of Andrews and Van der Waals showed that it would be possible to predict approximately the boiling points and critical temperatures of these gases that remained to be liquefied, and in the same period Joule and Kelvin published their famous paper on the cooling of a gas on passage through a porous plug. This phenomenon, where the molecules of a gas lose energy by what may be called the principle of internal work against their own attractive forces, has become one of the most powerful tools in the production of cold. All the remaining gases were liquefied by this means.

Within a fortnight of each other, and working independently, Pictet and Cailletet in 1877 announced the production of a few droplets of liquid air; but to Wroblewski in 1883 must go the credit for the production of the first liquid oxygen boiling quietly in a vessel, at a temperature of 90° K. Incidentally, Wroblewski started his career as a spectroscopist, but on being exiled to Siberia for some years by the Czar Alexander, he developed an interest in low temperatures After his success with oxygen he tried for ten years to liquefy hydrogen, but in this he failed.

It was left to Dewar, who invented the glass vacuum vessel for the purpose, to produce liquid hydrogen in 1896 using the Joule-Kelvin process, first compressing the gas in contact with liquid air, and then allowing it to expand through a fine valve, where it partially liquefied at a temperature of 20° K.

More than nine tenths of the way to the absolute zero was thus covered, and the sole remaining unliquefied gas was helium. Observed first in the solar atmosphere in 1868, and then discovered by Ramsay in small quantities in certain minerals containing uranium, it was soon isolated in sufficient quantities for investigation. Olzewski and Dewar both made attempts around 1900 to liquefy helium by allowing it to expand from a pressure of 100 atmospheres and at an initial temperature of 20° K, but they had insufficient thermal insulation and the experiments failed. They did not then realize that the amount of heat required to vaporize 1 cc of liquid helium is about one six hundredth of the amount required to vaporize the same quantity of water. They were able to show, however, that they had achieved a temperature of less than 8° K.

It was Kamerlingh Onnes who, in 1908, first produced liquid helium boiling quietly in a flask. He had spent the previous ten years in detailed calculation of the expected properties of the liquid and in making his apparatus. Careful measurements, which he made on the isotherms of helium, indicated that it would have a critical temperature of between 5° and 6° K, (that is, the liquid could not exist above that temperature), and that its boiling point would probably be between 4° and 5° K.

He first cooled the compressed helium gas with liquid nitrogen and then cooled it further by means of a bath of liquid hydrogen boiling under reduced

pressure at 12° K The gas was then expanded through a Joule-Kelvin valve, where it was further cooled and partially liquefied.

He determined the boiling point at 4.2° K, and the critical temperature at 5.2° K, and on almost the first occasion of liquefaction he tried to freeze the liquid by rapid evaporation. He lowered the vapor pressure to approximately 1 mm of mercury and reached a temperature of roughly 1° K, but produced no solid.

Thus only forty years ago a mere 1° of the 300°-odd down from room temperature remained to be covered. A fairly long pause ensued before any further lowering of temperature was achieved. The pause was a fundamental one, since no further gases remained to be liquefied, and so other means had to be sought to extract further entropy from matter. There was also the difficulty we have mentioned before. One of the fundamental laws of physics, the so-called third law of thermodynamics, states that the absolute zero is unattainable. It states, futhermore that the closer one gets to the zero, the more difficult it becomes to get closer still.

The twenty-year pause was, of course, only a pause in the search for lower and lower temperatures, and during that period there was a spate of research on the various new phenomena that were discovered at liquid-helium temperatures. The pause was also punctuated by one or two attempts further to reduce the vapor pressure and hence the temperature of the helium. In the middle twenties Keesom reached 0 7° K by attaching a series of great mercury diffusion pumps to a thimble-sized flask of liquid helium.

(It must be remarked at this point that such a low temperature as this is unique in the whole universe, since the radiative equilibrium temperature of interstellar space is in the neighborhood of 3° K.)

Since virtually all the entropy arising from the disordered motion of atoms and molecules has disappeared at 1° K, in order to achieve still lower temperatures we must seek some other and more subtle form of disorder from which we might extract entropy.

In 1926 Debye and Giauque, working independently, gave the hint as to where this disorder might be found. There is a group of substances which exhibit the phenomenon known as paramagnetism. Such substances only show magnetic effects when they are in the presence of a magnetic field, and therefore differ from iron in this respect.

Each ion in a paramagnetic crystal has what is known as an unbalanced electron spin, which means that the ion is in fact a little elementary magnet. Although the ions themselves are arranged in an orderly way in the crystal lattice, thermal agitation keeps the orientation of these ionic magnets in a completely random state, so that we have a species of subatomic disorder. The application of an external magnetic field will tend to turn all the ionic magnets in the direction of the field, and hence the magnetic disorder will tend to disappear, and the entropy will be correspondingly reduced. If the entropy can be reduced in this way, we might by suitable manipulation succeed in cooling a paramagnetic substance below the temperature of its surroundings.

At room temperature such an effect would be very feeble, but Debye and Giauque pointed out that by Curie's law the paramagnetism increases inversely with the temperature, and so at 1° K the effect would be three hundred times as great as at room temperature.

It was not until 1933 that successful experiments were performed both by Giauque in California and by de Haas in Leiden. The substance chosen was a rare earth salt, gadolinium sulphate, which was predicted to be suitable for the purpose. A specimen of several grams' weight was used. This was placed in a container in a bath of liquid helium at a temperature of about 1° K. The container also had in it an atmosphere of helium gas which could be removed by a vapor pump. When the apparatus was placed between the poles of a magnet and the field applied, the work done by the field in aligning the electron spins or ionic magnets, that is, the energy of magnetization, appeared as heat, according to the first law of thermodynamics. The salt therefore warmed up and was immediately cooled again by the atmosphere of helium which transferred the heat to the helium bath. Then when the salt was back at 1° K, the helium atmosphere was removed and the salt was thermally isolated. When the magnetic field was removed, the ordering agency no longer existed, and therefore the spins were free to disorder themselves. But now the only energy to do this could come from the crystal lattice itself, since none could come from outside, and therefore the substance cooled down below the bath temperature. The whole process is now known as magnetic cooling, or adiabatic demagnetization.

Although the earliest experiments achieved a low temperature something like 0.5° K, other, more suitable, and also less costly substances were quickly found, and with copper ammonium sul-

phate demagnetizing from a field of about 50,000 gauss, Ashmead in Cambridge in 1939 was able to attain a temperature of between 0.0015° and 0.003° K.

Of course, by this method the temperature drops quite suddenly and in one step, and then gradually rises again as the salt warms up to the bath temperature. There is no means at present of maintaining such a low temperature indefinitely. However, the relatively great contribution of the magnetic specific heat to its total specific heat is such that a specimen of salt may take ten or twenty hours to warm up from its lowest temperature to 1°K.

In an ideal paramagnetic substance in the absence of a magnetic field, the disorder would persist down to the absolute zero. That would mean, however, since we can remove the disorder magnetically, that demagnetization would take us right to the absolute zero itself, which is contrary to the third law. Actually, paramagnetic substances ultimately provide their own ordering processes, so that the low temperature limit with this technique is about 10^{-4}° K.

III

Such experiments as have been described have now been done many times, and the region of temperature in the neighborhood of 0.01° K is very familiar to low-temperature physicists. One might think, having got so close to the absolute zero, that we would be content, but of course this is not so. The region between room temperature (i.e., 300° K) and 3° K is relatively as spacious and full of interest as that between 3° K and 0.03° K, and the same is true of the region between 0.03° K and 0.0003° K.

What, then, is the next step? By perfecting present technique one might get down to 0.0003° K by removal of electron spin disorder, but that would be the limit. As far as we know, we are left with only one further important source of entropy of disorder, and that is in the atomic nucleus. The nuclei of many kinds of atoms are very feebly paramagnetic, and they also obey Curie's law; that is, their nuclear paramagnetism varies inversely with the temperature. The effect is inappreciable at room temperature, and it can only be observed with some difficulty at 1° K. But at 0.01° K it is quite pronounced. If one could first cool a suitable substance, say, a compound of lithium, down to 0.01° K in the presence of a large magnetic field of about 100,000 gauss, then demagnetization under such conditions would give one a reasonable chance of reaching a temperature of 10^{-5}° K or 10^{-6}° K.

Can this be done? We don't know, but experiments are already in progress in Cambridge to find out. The apparatus may be roughly described as follows. A large cylinder of some paramagnetic salt, say, copper or ferric sulphate, is fixed at the upper end of a metal rod six or eight inches long. At the lower end of the rod is fixed the nuclear substance, say, lithium fluoride. The rod with its two appendages is in a container in a helium bath, and both are in the presence of a very large magnetic field. The magnet is slowly lowered so that first the sulphate comes out of the field and is demagnetized, and its temperature—and by conduction that of the nuclear substance—is lowered, and cools to the region of 0.01° K. The magnet is further lowered so that now the nuclear substance is demagnetized, this time from a starting temperature of 0.01° K. Whether the experiment will be a success should be known within the next year or two.

It would appear from all that has been said that we are really getting very close to the absolute zero. So we are, in one sense, but it is instructive to look at a temperature scale plotted logarithmically instead of linearly, as is normally done. One can see on such a scale of temperature, which is the only satisfactory scale to picture at these very low temperatures, that the absolute zero has receded to minus infinity, where in fact it has always really been, and where it will remain, unattainable.

IRRIGATION AGRICULTURE ALONG THE NILE AND THE EUPHRATES

FRANK M. EATON

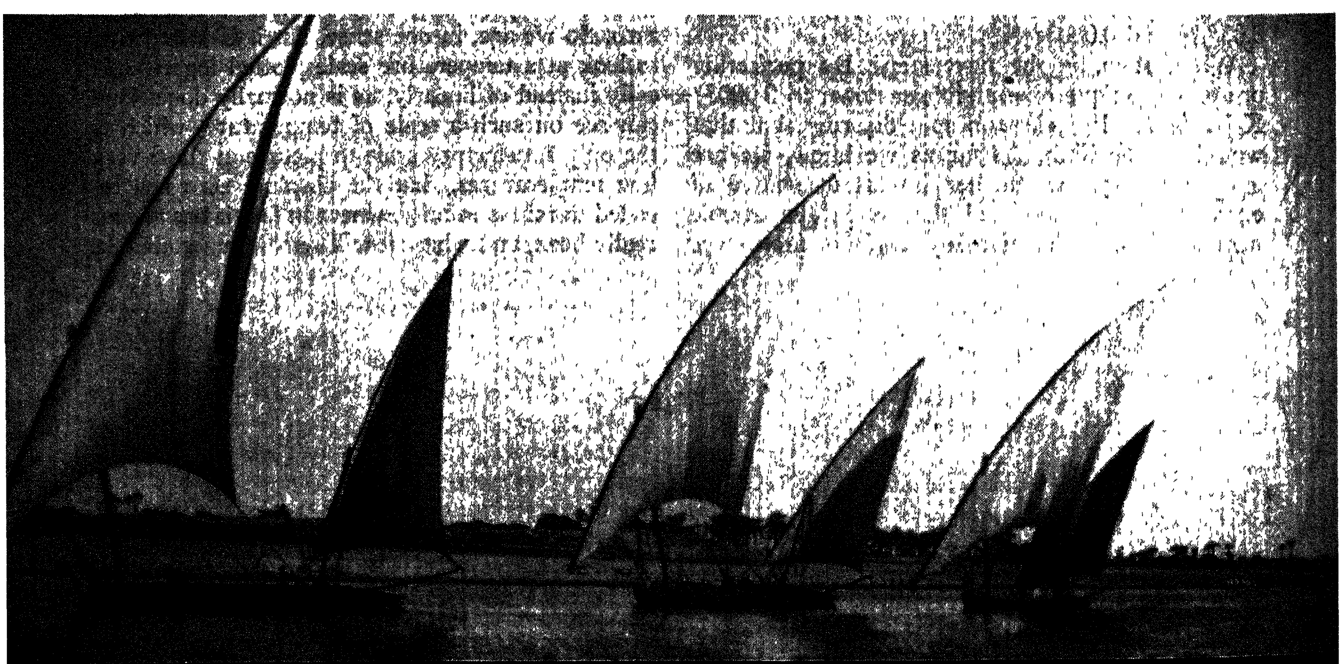
Dr. Eaton (Ph D., Minnesota, 1926) has been with the U. S. Department of Agriculture since 1915, with time out only to teach at the University of Minnesota. He is at present Principal Plant Physiologist of the Cotton Division, with headquarters at the Texas Agricultural Experiment Station.

STUDENTS of irrigation, as well as laymen, have sometimes voiced questions regarding the permanence of irrigation agriculture. Such questions are natural because of the failure or low productivity of a good many soils in the United States which have been, or are currently, irrigated. Although modern irrigation technologists in most instances are able to diagnose the soil conditions responsible for nonproductiveness, the antecedent causes for an unfavorable condition are not always so tangible. It is only reasonable that an element of uncertainty should exist, therefore, as to the ultimate life or degree of success of some of the now-profitable irrigation projects. This subject is of more than passing interest because the prosperity, and in a measure the habitability, of the arid West is dependent upon a productive irrigation agriculture. Irrigation as a supplement to rainfall is coming to be a great stabilizing factor eastward from the Rocky Mountains on to the Plains and, in fact, throughout the United States wherever summer droughts occur and water is available.

To anyone who has long been concerned with the effects of mineralized waters on soils and plants, it is a natural desire to observe at firsthand the conditions in the world's oldest irrigated lands. Such an opportunity was recently afforded me by a request from the Food and Agriculture Organization of the United Nations to accept a temporary assignment as cropping adviser to the Near East. The general request for this undertaking having been initiated by the member countries of the Near East, excellent contacts and opportunities for observation and discussion were provided throughout my visit. The time between December 10, 1947, and March 10, 1948, was divided between the Nile Valley and the valleys of the Tigris and Euphrates and lesser streams in Iraq, Lebanon, and, very briefly, Turkey.

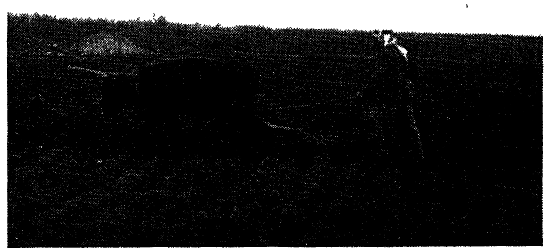
In this paper much of general irrigation interest will be passed over in order to dwell upon a rather

Countless lateen-rigged boats carry people, goods, produce—and the memories of centuries—up and down the Nile.



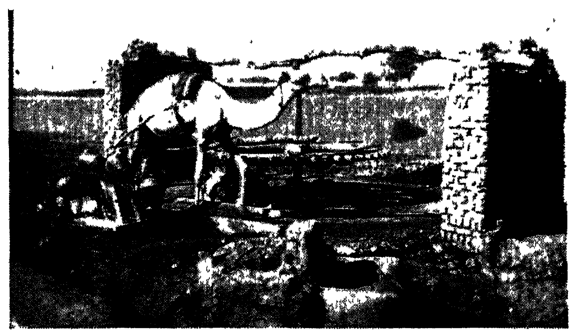
outstanding relation found in two of the countries visited between the occurrence of black alkali in soils and the presence of residual sodium carbonate in irrigation waters. By residual sodium carbonate is meant the sodium carbonate reported as such in the old-fashioned procedure of recording analytical data in hypothetical salt combinations; a procedure which was outmoded by the development of concepts of ionization. Beyond the subject of black alkali there will be some discussion of the trend of events in Egypt and Iraq as they have been influenced by the salts of the irrigation waters. A renewed recognition of sodium carbonate in irrigation waters as a factor in black-alkali formation should provide for a little more inclusiveness in water quality appraisals and, thereby, for increased foresight into things that may lie ahead in a number of irrigation enterprises. Here and there the formation of black alkali has continued to be one of the most difficult features of irrigation agriculture —not only from the standpoint of understanding its cause and mode of formation, but also from that of its avoidance and the reclamation of the affected lands. Black alkali, as is generally known, is the descriptive name applied to soils that show an alkaline reaction in the range of about pH 8.4—10 or more. With increasing alkalinity in this range, organic matter of the soil is brought into solution, and, because of it, the soil takes on a dark-brown to blackish appearance. Black-alkali soils may or may not be saline, but they are rather impervious to water movements; i.e., under the influence of the absorbed sodium the soil's granular structure breaks down and the individual soil particles become dispersed and subject to packing. Although much work has been done on the chemical and physical properties of black-alkali soil and its reclamation, an antecedent cause in the chemical characteristics of irrigation waters has generally escaped attention since the time of Hilgard and Stabler at the turn of the century.

The waters of the Tigris and Euphrates in Iraq and of the Nile in Egypt have been used for many centuries as a basis for the agriculture supporting two great civilizations. One cannot say accurately just how long these civilizations have been depending upon irrigation, but it is well established that during recorded history the Nile Valley of Egypt and the lower stretches of the Tigris and Euphrates valleys in Iraq have been far too arid to have supported farming without irrigation. The annual rainfall at Bagdad and Basra in Iraq is about 7 inches. Cairo receives less than 1.5 inches annually, and Aswan, which is in upper Egypt, is within the so-called rainless belt.



Reworking the land with wooden plow in northern Syria

Life seems simple in this village near Basra as seeds are spread to dry.



The *Sakia,* with its endless chain of water pots and endless groaning and squeaking, may be operated by ox, horse, camel, or donkey. Animal is usually blindfolded so that he does not become dizzy.

In his book *Twin Rivers,* Seton Lloyd (1947) concludes that the beginning of the first dynasty of the Sumerian Kings of Iraq, almost exactly three thousand years before Christ, corresponded in time with the first Egyptian dynasty. This was the beginning of history in terms of countries ruled by kings whose names are known. As he says,

> Everything before this is more or less convincing speculation, arising out of the results of archaeological researches. In the case of Iraq, some of the most striking material which excavations have produced belongs to the dark period before written records begin, and any history would therefore be incomplete without an initial discussion

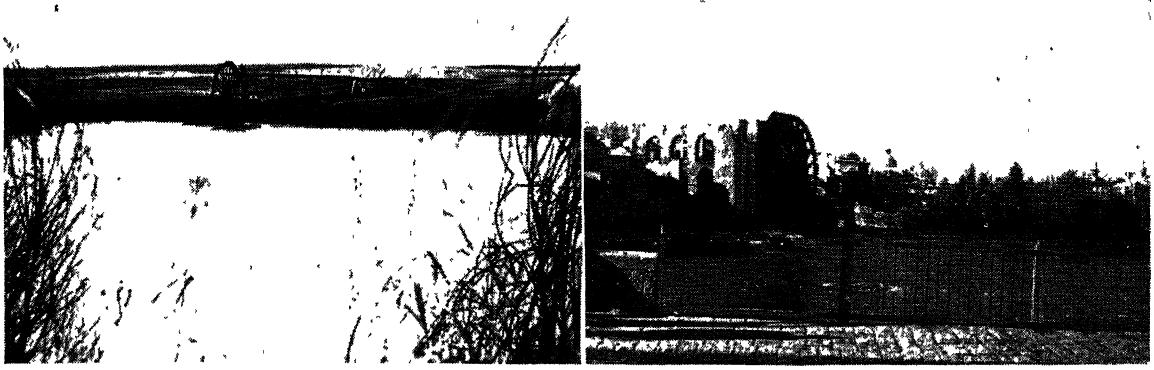
of it. The first conclusion to which an old-fashioned archaeologist reluctantly comes is that Mesopotamia is not the "Garden of Eden," either Biblically or figuratively speaking. The ancestors of the first human beings who settled in Iraq in any numbers had, for hundreds if not thousands of years, been living in houses and known the use of comparatively advanced conveniences, such as pottery.

The length of time a particular parcel of land in either of these countries has been irrigated must remain conjectural. As a consequence of floods and water diversions, silt is still being deposited and the soils deepened in the valleys of the Nile and the Tigris and Euphrates. The Nile mud varies from a depth of about twenty feet in the valley to forty feet or more out on the delta. One figure for the rate of silt deposition along the Nile is a twentieth of an inch per year. At such a rate, the surface soil of a thousand years ago is now four feet below the present surface. In Iraq a similar heavy deposition of alluvium has been going on. The geologists know, for example, that the Persian Gulf at one time extended well north of Bagdad, or nearly three hundred miles inland from its present head. Both in Iraq and in Egypt the building of the alluvial valleys has progressed through the centuries. The older soil, which is now covered, can nonetheless, by its permeability, affect the course of the agriculture in the overlying soil. But, like the waters that loosened the soil particles and bore them to the valleys, the upper and lower alluvial depositions are, by and large, of the same genesis. The watersheds have continuously contributed silt, salt, and water. The silt adds new wealth to the land, but, if the agriculture is to survive, the salt and some of the water to carry it, must move on to the sea.

The first use of irrigation in these regions to support the growth of planted crops was not in keeping with the meaning we attach to the term irrigation here in the United States. We regard irrigation as consisting of applying water to land or to growing crops by diverting or pumping it from rivers, lakes, or underground aquifers; a system of ditches or pipes is usually involved. Actually, the first use of stream waters to supplement rainfall was not accomplished, probably, by diversion but rather by planting seed promptly after the subsidence of flood waters. Following the recession of the Nile, I observed while at Luxor, Egypt, the fellahin rubbing seed of lentils and barley into slippery soil with their hands after its surface had been roughly scratched. The construction of diversions and ditches to carry water to the land, like the various primitive devices for lifting water, have come later than the primitive practice of planting on flood-wetted soil. In lower Iraq, one form of primitive irrigation was probably similar to the present subirrigation near the Persian Gulf; the land is networked with ditches, which fill and drain with the rise and fall of the river as its water is backed up by the tides.

In Egypt, black alkali constitutes one of the major soil problems, whereas in Iraq black alkali does not occur—or at least not in the great valleys of the Tigris and Euphrates. In Egypt the existence as well as the formation and spread of black alkali, and with it the partial or complete loss of productive capacity in the most affected spots, is characterized by impermeability to water. The condition is not necessarily even recognizable by salt accumulation. The Egyptian investigators regard the first link in the chain of black-alkali formation to be the activity of sulphate-reducing bacteria

This ancient masonry dam across Shatt Al Adhaim eighty miles north of Bagdad, was in use 2,000 years ago. *Right:* water too salty for long use on the same land, is lifted from open wells in goatskins. First plantings are vegetables, but when irrigation is started little cuttings of the salt-tolerant tamarix are pushed into the soil. Their roots go down to the ground water. After the third year, there are crops of wood to be cut and sold in Az Zuhair or Basra. Except for these plantings (*see background*) and the dates along the riverbanks, the delta is dry, flat, and barren.



Primitive water wheel on the Khabour in northern Syria. This wheel, run by the current of the river, discharges water from cans on its rim into trough and irrigation ditch. At Homs, Syria, another great water wheel spills water into an ancient aqueduct.

brought about by the rise of the water table. They list the differences between the alkali and surrounding fertile soil as a predominance of absorbed sodium, a heavy increase in precipitated calcium carbonate and magnesium silicate, a great reduction in organic matter, an abnormally high carbonate-bicarbonate titration of the water extract, a high silica content of the clay which becomes retentive of water and difficult to dry, and an often-present black skin of organic matter on the surface of the soil, giving rise to the term black alkali, or *Armout.*

Because it has accentuated the drainage problem, the extension of perennial irrigation through the use of the increasingly elaborate system of diversion dams (barrages) and canals has made the formation of black alkali more serious in Egypt; corresponding with the greater need for drainage, more alkali is found in the delta than in the basin, or flood-irrigated lands south of Cairo.

The absence of black alkali in Iraq was pointed out early in the discussions at Bagdad by Darwish Al Haidari, Director General of Agriculture. His statements were entirely in accord with later first-hand observations made in the central area of Iraq and northward beyond Mosul and southward beyond Basra. Black alkali probably does not exist in lands watered from the Tigris and Euphrates.

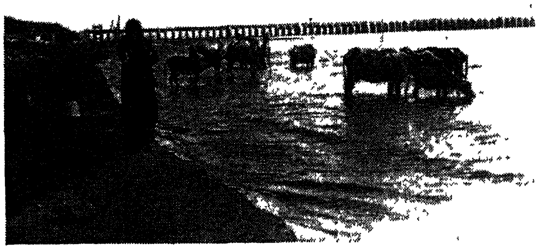
By any of the usual criteria of quality of irrigation waters, those of the Nile, Tigris, and Euphrates would be placed among the world's very creditable ones. In fact, the Nile water, at least at flood stage, would be classed as excellent. The concentrations of salts in these rivers, as in most if not all streams, become greater as the rivers recede. Measured in milliequivalents per liter, the concentrations of principal constituents as reported in some of the available analyses of the Nile, Tigris, and Euphrates are shown in Table 1.

TABLE 1

	Ca	Mg	Na	HCO_3-CO_3	SO_4	Cl	Na %
Flood:							
Nile, 4 months	0.64	0.57	0.50	1.44	0.16	0.14	29
Tigris, 3 months	2 41	0.90	0.36	2.76	0.40	0 51	10
Low:							
Nile, 8 months	0.87	0.72	2.04	3.03	0.23	0.46	56
Tigris, October	3.93	1.99	0.44	3 00	1.81	1.55	7
Euphrates, "	3.42	2.84	0.66	2.92	1.79	2.21	10

Each of these waters is relatively low in total salts and each has a "percent sodium" value within the range usually regarded as safe. The use of "percent sodium" (Na x 100/total bases) in the evaluation of irrigation waters has followed as a natural sequence to the development of information on the base-exchange phenomenon and recognition of the adverse effects which absorbed sodium has on soil structure. There has continued to be good agreement that irrigation waters with much more than 60 percent sodium are apt to impair soil permeability; but initially and since then it has been necessary to recognize that in addition to important factors of length and manner of use, a given percentage of sodium in waters does not necessarily have the same effect under all conditions. On the basis of the consideration here given the subject, the major deciding factor in the interpretation of percent sodium would come to rest on proportions of the anions present in the water.

The percentage of sodium in some waters increases very much more rapidly than in others as evaporation and plant use take place: (*a*) If a water contains little HCO_3-CO_3, then with evaporation the concentration of all ions will tend to change in the same general proportions and there



Camels, donkeys, and buffaloes here drink together from the Nile below the dam at Assuit—200 miles upstream from Cairo. From this dam flood waters are diverted out through a canal system over a million acres of Egypt's land.

will be no residual sodium carbonate. The small change in percent sodium will be that resulting from a little loss of $CaCO_3$ and possibly $CaSO_4$. There likewise will be little change in the *p*H of the solution. (*b*) If the water contains much Ca and Mg and a like amount or a little less of HCO_3-CO_3, there will be a large increase in the percent sodium of the solution as the carbonate precipitates out; but, again, there will be no residual sodium carbonate, nor any great change in *p*H. As a third category, (*c*), if the water contains more HCO_3-CO_3 than it does Ca plus Mg, then with evaporation the Ca and Mg carbonates are precipitated and there remains sodium carbonate, and Na is the only important base. Since the strong base, Na, is present with the excess of carbonate, a weak acid, the solution becomes strongly alkaline. It is the presence or absence of this residual sodium carbonate that now appears to furnish a criterion of whether black alkali can or cannot develop in irrigated soils.

There is need for recognizing the foregoing three classes of waters because these waters, in the absence of good drainage or when used too sparingly, give rise in sufficient time to three corresponding kinds of soil, each of which is undesirable: calcium saline soils, sodium saline soils, and alkali soils (either saline or nonsaline).

The waters of the Tigris and Euphrates are in the first-mentioned category: they contain no residual sodium carbonate. Where there has not been sufficient drainage for an adequate use of water, the soils are saline, but calcium predominates. With the provision of drainage, the lands can be easily reclaimed. The Nile water, on the other hand, belongs in the third category. It is low in total salt, but where the drainage, or water use, for the removal of the salt residues from the land has been inadequate, the soils have become saline. During periods of low water the Nile water has a *p*H well above 8.0. The water has such a large proportion of HCO_3-CO_3 that when it is greatly reduced in volume by evaporation it precipitates much of its Ca and Mg as carbonates and silicates, giving rise to an alkaline solution with little else than sodium salts of carbonate, chloride, and sulfate. Such a solution washing onto a soil from neighboring land, or rising from below and moving through it, would bring about the replacement of exchangeable calcium and magnesium, produce a high *p*H, establish impermeability, and, in other words, create those conditions that are descriptive of black-alkali soils. A soil leached until impermeable with Nile water from which the Ca and Mg have been precipitated would not necessarily retain enough of any salt to make it very saline. It is a universal characteristic of black-alkali soils that they yield sodium carbonate when taken up in water. It seems doubtful that black alkali ever appears in initially fertile irrigated land unless the irrigation water contains sodium carbonate or sodium carbonate rises into it from the subsoil.

The mixed conditions of highly fertile soils, saline soils, and alkali soils under the Nile provide ample evidence that the use of an irrigation water that yields sodium carbonate upon evaporation does not always lead to black-alkali formation. Furthermore, soils that contain some black alkali, but which are not yet entirely impervious, can sometimes be reclaimed without the use of soil amendments, by providing drainage and leaching with the water that produced the condition. This has been accomplished in Egypt and also in the United States. In fact, a continuous fluctuation in the degree of alkalinity in black-alkali soils must always be in progress, depending on the amount of leaching that takes place. The loss of soluble organic matter from alkali soils and its return to streams would, upon its oxidation, contribute substantially to the nitrate that is found in some waters such as the Nile. The Nile serves irrigation in the Sudan and also floods great upstream marshlands. These areas undoubtedly contribute both to the loss of calcium and magnesium and to the nitrate of the Nile water.

There are in the United States large irrigated acreages in which black alkali occurs. If it is true, as the observations here outlined indicate, that residual sodium carbonate in an irrigation water is prerequisite and conducive to black-alkali formation, then the composition of waters now in use predicates that there are large areas of irrigated lands that are subject to its formation. Likewise, by

the nature of the irrigation waters in use, there are also large areas that must be regarded as immune. If the possibility of black-alkali formation can be anticipated by recognition of the ionic relations of the water supply, advantage can be taken of the facts, not necessarily as a basis for condemning a water, but rather as a means of establishing the need of precautionary measures. Productivity can be maintained by adequate water use and drainage at less expense than it can be restored by reclamation. By the time reclamation is needed the owners have all too commonly lost both capital and confidence.

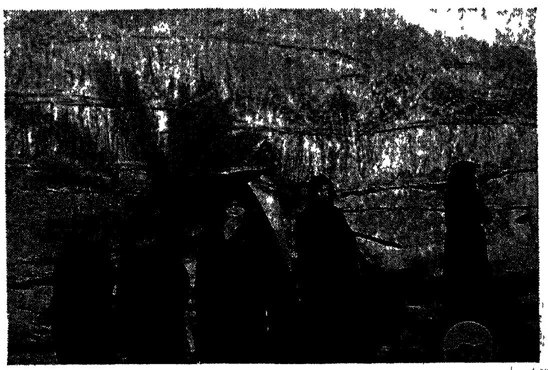
Characterizations of the river waters of the irrigated regions of Western United States as regards residual sodium carbonate should be made on the basis of averages of a number of samples or, much better—particularly if flood waters are impounded in reservoirs—on the basis of means weighted for discharge. In a table of analyses of nineteen Western streams of the United States, which were mostly single analyses at single points, five were found by computation to have carried residual sodium carbonate (the Kings, Owens, Payette, Rogue, and Yakima); nine others (the Gila, Salt, Colorado, South Platte, Arkansas, Rio Grande, Pecos, Sevier, and Big Horn) were without it; and five others were marginal to varying degrees.

Among a group of 444 well and stream samples collected in the San Joaquin Valley of California, slightly more than half were found to have carried residual sodium carbonate. As classified by groups, a small but significant positive correlation of 0.19 is found between residual sodium carbonate and percent sodium and a similar negative correlation of 0.20 between residual sodium carbonates and electrical conductivity. Professor R. Earl Storie has kindly compared maps of the San Joaquin Valley marked by the writer to show the occurrence of "black-alkali waters" with his records of the black-alkali soils. He reports: "In general I find nothing conflicting with the alkali conditions shown on our soil maps; in fact, as mentioned previously in this letter, your red dashes check very closely with the black alkali soils of the valley."

Notwithstanding the occurrence of black-alkali and substantial salt accumulations in many Nile lands, that agriculture is one of the most productive in the world. The fifteen or sixteen million people of Egypt are almost entirely dependent for their food and clothing upon the products of about seven million acres of irrigated land.

In Iraq the situation is quite different. Throughout its history, Iraq has no record of the construction of any canal intended to drain or carry away the salt residues from its irrigated lands. Some drainage is due to the proximity of a part of the land to washes, streams, and rivers, but for many centuries the salt applied in irrigation and flood waters has far exceeded the removal. The valleys of Iraq—like Lot's wife—have turned to salt. But, unlike her, the people, until perhaps recently, seem never to have looked back, or around them, or ahead to observe the consequence of their works. If they have, they have failed either to understand or to take advantage. And so it is with much of the valley land along the Euphrates in Syria and its great tributary from the North, the Khabour. After driving over these lands—which seemed to me to abound with geographical and potential agricultural wealth—there remains a mental picture of abandoned canals and villages but great undeveloped resources—or, more strictly—resources waiting to be developed again. My attention was called by a young Los Angeles pilot on one of the Syrian airlines to an abandoned town of good size which we passed over between Hasseche and Aleppo. The eastern outer walls were oval in outline, representing old Syrian, whereas the western section, partially set into the oval, was rectangular, marking it as Roman. The Romans had been there, but, like the other inhabitants, they had given up. All the walls and houses, like the irrigation canals in the country outside the city, have become nothing but slowly melting mud. The water carried by the canals has come and gone, but the residual salt of the water remains behind in the land.

Until the past century, there has probably been little or no intentional drainage in Egypt. But there is a great difference between the physical conditions of Egypt and Iraq. The Nile mud is under-



At Basra, near the Persian Gulf and the Tigris, patties of manure are flattened against the side of grass huts to dry for fuel.

As one goes northward from Homs and into the country drained by the Euphrates in upper Syria, there may be a city or, within a wall, a little group of these strange domed houses of white clay.

Sheep of northern Iraq start on the long trek to Damascus, fattening as they go on spring grass, along a route prescribed by centuries of tribal custom.

At Anshas, in the orchards of the Egyptian King, the hay may be composted to fertilize the oranges, mangoes, and bananas During the hot, dry summer, 12 acre-feet or more of water will be pumped onto these orchard soils

lain by gravel beds penetrated by the bed of the river. This gravel provides drainage from the land to the river when the Nile is low. When the river is high, the hydrostatic pressure within the gravel (the land slopes away from the river) sometimes forces water upward through the soil, carrying the salt residues of previous irrigations back into the root zone to injure the crops. Out on the delta the need for improved drainage and salt removal becomes ever more pressing. In the valley lands above Cairo the extension of pumping from the gravel beds to supplement basin irrigation will eventually aggravate an already existent, but now spotted, salt problem.

A complete, or nearly complete, file of analyses of the ground waters of the Nile Valley above Cairo shows an average of about 100 parts per million of chloride —or about 3 milliequivalents per liter. My own computations indicate that chloride content of the ground water of any section of the valley when fully equipped with pumps will increase by an average of about 1 ppm of chloride yearly. This would double the present average concentrations in about one hundred years. When the Nile subsides after each flood there is a substantial return movement of water from the gravel to the river. This return movement is helped by the sheet of flood water that is spread over the country. On the average about 40 percent of this flood water is

thought to return to the stream—either as surface runoff or as deep percolation. With this return there is a substantial (but never complete) return of the annual salt residues. If this annual return of salt had in the past been complete, the well waters would resemble river waters, but many wells are now too saline for use, and furthermore salt appears on the surface of the land along the valley margins. Annual pumping will lower the water in the gravels, and the next rise of the river will replace the water that has been used by pushing the residual water of the gravels out laterally from the river toward the valley margins. This lateral movement of water will, of course, carry with it the salt residues that have been leached downward from the overlying soil. The fact that pumping reduces the volume of gravel-stored water will increase the amount of new water that enters with the flood. The last water to enter the gravel from the river as it rises is, after probably only a little mixing, the first to leave as the river falls. Since the river water entering the gravel has only a little salt, the wells closest to the river may tend to improve, but to a greater extent the wells further away will become worse.

Under these conditions the accumulation of salt in the gravels and overlying soil will necessitate a great expenditure for drainage ditches along the margins of the valley to the sea—a distance of 550 miles from Aswan to the Mediterranean. Egypt, like Iraq and the United States, has been more intent in the past upon planting and watering a new crop than in saving one or making those a few years ahead more worth while.

As has been implied, one may travel for miles through the valley lands of Iraq almost continuously observing the remnants of abandoned irrigation works. Nearly everywhere there is salt—if it is not lying white on the surface of the land, the soil itself is usually impregnated. Southward from Bagdad and westward from the Tower of Babylon there are the remnants of a great canal, once served from the Euphrates, which measures eighty feet across its floor. Other great canals that once diverted water from the Tigris or its tributaries are found northward from Bagdad toward ancient Nineveh. These once served beautifully lying lands that are now salt-impregnated. Also to the north there stands a partially washed-out masonry dam on Shatt Al Adhaim. Both the canals and this dam were in use more than 2,000 years ago.

The irrigated agriculture of Iraq is at a low ebb, but there is an abundance of water, and the climate is well suited to dates, cereals, cotton, and many other crops. The date palm is especially salt-tolerant, and it has come to be of great importance to the economy of Iraq. Where the natural drainage is good, excellent crops may be produced, but such locations are now limited. An irrigated grain crop is produced each second year on some of the valley lands. The intervening year permits of a little fall in the ground water and with it perhaps a little drainage of salt and, further, a fixation and accumulation of a little nitrogen. (Commercial fertilizers are practically unknown in Iraq.) Where they can be grown, the oranges and mandarins of Iraq are excellent and free from scale insects. On the naturally drained lands—as along washes and rivers—the dates are twice as productive as on the highly saline soils. Palms come into bearing in seven years where they can be watered from the surface with fresh water, whereas fifteen years are required when subirrigated with water backed up with the rise and fall of the tide. On the way back from Basra I looked out of the train window about three o'clock on a moonlit morning. I first thought that it had been snowing, and yet I knew it was too warm. The countryside was covered with white salt, and save for a few scattered palms there was no vegetation.

Iraq is said to have had at one time a population of forty million people, but at present there are less than five million. Many of these are Bedouins and others who derive their living from the unirrigated hill lands in the northern part of the country. From these hills droves of sheep are driven yearly toward Damascus or the Mediterranean, feeding and fattening en route on the grass that covers the land in the spring. Opinions differ as to when and why this supposed much larger population of Iraq disappeared. One interpretation of the disappearance is that the people were killed during the Mongolian invasion of the twelfth century. Possibly so, but, as I view it, it is most improbable that these lands with the salt they now carry could support many more people than they are at present supporting. Until proved otherwise, it is logical to believe that Iraq provides a striking example of the decline of a great civilization—all because of a little salt in the water and the want of a few ditches. Compared to the magnitude of this slow-moving event, our dust bowl was but a passing incident.

Iraq has the heritage of tremendous oil reserves and two great rivers. For the waters of these rivers there are waiting an estimated fourteen million acres of arable land; but also waiting is an excess of about one billion tons of salt, which must be transported through drainage canals to the Persian

Life begins at Aswan. Here for the first time in Egypt, the Nile encounters a great barrage that checks its flow. Electrification of this gigantic dam is now taking place, and it will shortly provide 354,000 horsepower, for the benefit of all Egypt.

Gulf. Who can say how soon those who administer this potential wealth will decide to invest oil royalties in drainage canals and new irrigation works? Should such a start be made on a sizable scale, the result should be like the course of a snowball from the top of a hill. With adequate support, the Iraqian agriculturalists and engineers could within the span of a few decades literally create a new Garden of Eden. And, of perhaps greatest importance, these agricultural gains could conceivably proceed rapidly enough to outstrip the population gains. Throughout much of the world, agricultural improvements have little or no opportunity to move much if any faster than the increase in the number of people. Actually, hungry mouths are already waiting upon the agricultural gains. Under such circumstances, the people as individuals remain just as poor as ever. But in Iraq, as in northern Syria, there could be developed communities of people with sufficient wealth and income for education, social enjoyment, and security far beyond those of their neighbors.

MAN AND HIS PREJUDICES

JOSEPH B. GITTLER

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MUCH has been written about the threat of prejudice in undermining our democratic institutions. Specific instances can be cited in regard to the growth and magnitude of man's antipathy to man. It seems worth while, therefore, to analyze carefully the fundamental nature of these antipathies in order more sanely to comprehend these savage undercurrents in our daily lives and so attack the problem at its most vulnerable point.

The universality of men's aversion for one another is shocking in its implications. Negativism, once a problem relegated to the confines of early childhood, has become the order of the day among the adults of every nation. Group is pitted against group: the Protestant attacks the Catholic, the Catholic attacks the Jew, the Jew mistrusts the Gentile, the white persecutes the Negro, the Negro turns on his white enemy. Whatever the manifestation of these antagonisms—whether it be racial, ethnic, religious, or class—a common thread runs through the entire web of intergroup relations.

There are many general types of prejudices, but two loom largest on the horizon, dwarfing all others. The first of these is easily recognizable. It is the kind that expresses itself in overt forms of group opposition. It is the type of prejudice that keeps certain groups from living in designated areas of a city, bars them from schools, deprives them of voting privileges, and excludes them from hotels, clubs, theaters, and so on.

This sort of prejudice is vicious from the standpoint of democratic principles, but it is in a relative sense capable of being handled, since it expresses itself in tangible forms—usually in terms of ordinances and statutes. Given enough people who desire a change, the laws can be repealed. It would appear that there is an ever-growing number of people who are sincerely interested in effecting such a change.

Even if this overt form of prejudice does not express itself in law but in the form of a "gentlemen's agreement" to keep members of various minority groups from obtaining their rights of citizenship—in gaining admittance to hotels, schools, and stores—it is still concrete enough to allow for direct treatment. It involves specific persons who are *principally* responsible for this form of discrimination, and these, skillfully approached through influential community leaders who are truly desirous of mitigating the problem, can be made to modify their rulings.

Thus, by group action to change the law, or through the pressure of public opinion, some of these discriminatory practices may be obviated.

There is another type of prejudice, however, which is more basic and more insidious, difficult to rout from its hidden recesses, and almost impervious to any form of concrete action. It is subtle, covert, and noninstitutionalized. An illustration will serve to indicate the nature of this form of prejudice.

> I once boarded a city bus and chanced to sit next to two women, residents of a southern city. One lady was telling the other that she had just visited Mrs. R—— at the hospital.
>
> The other lady said, "Oh yes, I know who she is. She is your neighbor, isn't she?" "Yes," was the reply. "And she is a Catholic, isn't she?" "Yes," was the reply again. "And on the other side of you lives Mrs. S——, doesn't she? And I think she is a Catholic, too?"
>
> "Yes, she is," the first lady replied. And then, with a sigh of resignation and suffering, she revealed her attitude by adding, "and you know what it feels like to be hemmed in by two Catholics."

No objective listener could deny that here at least was one prejudiced individual. It is true that this type of prejudice is unlike the first. The "hemmed-in" woman would probably not attempt to have instituted laws, ordinances, or even agreements that would keep her Catholic neighbor from visiting any hospital she might please. Their children probably play with one another; they may attend the same school. And one would not imagine that she would be ready to institute, or formally banish, the Catholic children from the neighborhood or theaters.

The revealed prejudice had not reached the form of outright discrimination, true. But prejudice was there. It was not formalized, but rather

it existed as a kind of folkway prejudice, a kind of "general feeling of againstness." It reflects the general folk atmosphere.

Although it is unlike the first type, sometimes, if not quite frequently, it may lead into the first type, given an economic or political crisis, when the general negativism might easily turn to outright discrimination, scapegoating, or persecution.

Nazi Germany has given us our best case in point. In the days of the Weimar Republic the Jews enjoyed legal freedom and political tolerance. Laws forbade educational, economic, and legal discrimination. Legal guarantees, however, were not safeguards against folk feeling. A preponderance of evidence exists to prove that many Germans continued to feel differently toward the Jew than they did toward the non-Jew. When a crisis situation arose and a skillful demagogue appeared on the scene who played on this submerged feeling of againstness, organized scapegoating and massacre were easily achieved. In other words, this type of folkway prejudice, which ordinarily appears merely silly, rude, unfair, unkind, or anti-democratic, holds within it a submerged threat for the future as well as unpleasantness—or worse—for the present.

Interestingly enough, it is the nature of folkway prejudice to be almost universal. Members of primitive tribes manifest a general feeling of againstness toward the "outsiders," those who are not members of their own group. The Caribs believe that they "alone are people." The Lapps call themselves "human beings," implying that non-Lapps are not. Literally translated, Kiowa is "real, or principal, people." The Greenland Eskimo believes that Europeans have been sent to Greenland to learn virtue and good manners from him. The highest form of praise that the Eskimo can extend to a European is that he is, or soon will be, as good as a Greenlander.

This feeling of superiority that people feel toward their own way of life and their own group is not limited to the primitives. The ancient Greeks referred to the non-Greeks as barbarians. It is of course true that the Greek word *barbaros* did not have the identical meaning that our own word "barbarian" has, but it definitely attributed strangeness, rudeness, and inferiority to the non-Greeks.

Nor is this feeling of superiority, or "ethnocentrism," the term coined by Graham Sumner, limited to ancient peoples. To the query "Who was the first man?" a youthful patriot answered "Washington." "No," said the teacher, "the first man was Adam." "Oh, I suppose he was," conceded this small isolationist, "if you are going to include foreigners." Among Western "civilized" peoples we hear the constant use of such terms as "chocolate drop," "du-donk," "nigger," "Dutchy," "flip," "greaser," "dogs," "wop," "kike," "coon," "chee-chee," and others too distasteful to record.

Commenting on the story of Jesus, the Jew, told to a Sunday-School class of ten-year-olds, a pupil declared that she had never known that Jesus was a Jew. After further elucidation by the teacher, this youngster declared that she had always known "that God was a Presbyterian, but not that Jesus was a Jew."

Thus it goes, from one end of the world to the other, even filtering down to the jingles of children at play. A Dutch nursery rhyme tells us that:

The children in Holland take pleasure in making
What the children in England take pleasure in breaking.

This strongly rooted attachment to one's own way of life and one's own people has been observed through the ages by poet and scientist, the widely traveled diplomat and the secluded scholar. As Oliver Wendell Holmes so aptly remarked, "The people in every town feel that the axis of the earth passes through its Main Street."

I

It is this very group consciousness, or ethnocentrism, which lays the foundation of group prejudice. If there were no strong feeling for one's own group, there would not be strong consciousness of other groups. An awareness of one's own group as an in-group and of the others as out-groups is fundamental in group relationships. Group tensions are manifested by a sense of distrust or dislike, not to an individual as such, but to an individual as a symbol of an out-group. It does not always follow that a consciousness of one's own group leads to distrust and disharmony with all other groups. The friendly rivalry that exists between athletic teams, or the competition between rival garden clubs, for instance, points to the fact that not all group relations give rise to group tensions.

There appears to be present in all types of group prejudice some degree, whether real or imagined, of struggle or threat. So much of group prejudice, however, is linked up with imagined threat rather than real that a challenge is offered in attempting to understand it. The very universality of group antagonism has led many laymen to accept the inherent, innate, organic nature of these prejudices. So strong was this belief that up to thirty-five years ago it was a valid fact to the psychologists. Psychologically speaking, it was regarded as but one more instinct, along with mother love, pug-

nacity, curiosity, gregariousness, self-preservation, and fear, to name but a few.

The doctrine of the instinctivists has been pretty well discarded in the light of further and keener psychological research. No longer can the student of human nature accept the thesis that any human attitudes are inborn. No aspect of man's human social nature is inborn. His wishes, his beliefs, his knowledge, and his values all come as a result of his association with other men. They are developmental, not congenital. Attitudes are nurtured, not natured. Prejudice is fostered, not fathered.

We inherit biological traits such as the shape of the skull, the color of eyes and hair, the potentialities of tallness, all of which are carried through the germ plasm. Prejudices—indeed all our attitudes, habits, and emotions—are not carried in the germ plasm. They are developed by the culture that surrounds us: in early years, chiefly through the family groups; in later years, through other institutions—the church, the school, business.

The first few years are the most impressionable ones of our lives. The young child learns primarily from his parents. Children repeat what they find about them rather than invent their own forms of behavior. Thus the child acquires early in life those collective, specific attitudes of prejudice that exist in the family. These tend to remain with him and are often deep-rooted and unconscious.

When these prejudices in the family become further re-enforced by the community, society in general, and later associates, it is easy to see how ingrained and well established one's prejudices can become. And the tendency for prejudices to persist thus becomes comprehensible. For those with prejudices give them to those without them and, once acquired, they become further indelibly stamped. It becomes apparent, too, why it is that for generation after generation prejudices toward certain groups and persons are perpetuated.

This tendency toward perpetuation of group prejudices becomes understandable if we are aware that it is the nature of these prejudices, as well as of most attitudes, to be emotionally loaded. Prejudice consists of a pre-existing emotional tendency to act negatively toward a particular group, idea, or value. The very nature of prejudice presupposes feelings about things. Usually this emotional core is primary in these prejudice tendencies. Often reason supplies the rationalizations for existing prejudices. Emotions lead; reason apologizes and defends the prejudice.

Because attitudes of prejudice are full of emotion, it becomes clear not only why they persist, but also why it is difficult to be emancipated from them. Many instances exist that show how difficult it is to overcome prejudices, even though one is aware of the lack of basis in fact for his prejudice. For example, an intelligent and educated woman in the late fifties, who was born in the South, told the writer that although she was aware that the antiquated notions about the innate inferiority of the Negro are false rationalizations—that shape of nose, texture of hair, and pigmentation of the skin have nothing to do with mental ability, human morals, and interpersonal etiquette—she just could not help herself. By this she meant that given the stimulus, "Negro," she gets an emotional negative reaction, which is not reasonable or planned but nevertheless manifested and real. Her prejudice toward the Negro is an emotionalized phenomenon which appears automatically as a result of the previously established attitude toward the Negro. Usually the attitude is developed quite early in life. It often persists in spite of enlightenment and knowledge. People may "know better" but still are prejudiced.

Before we suggest what can be done about these emotionalized prejudices toward specific groups, a further word must be said about another type of attitude that man is capable of developing. We have spoken about specific prejudices, specific attitudes— attitudes toward Negroes, the next-door neighbor, the child on the other side of the tracks. These are specific attitudes directed toward specific objects. But there is another set of attitudes that tends to govern man's behavior and often determines the type of specific attitudes he will develop. A person has an attitude toward a particular individual, and he also has an attitude toward people in general. Besides having an attitude toward a particular member of the opposite sex, one has an attitude toward members of the opposite sex as a group, or toward sex in general.

Sometimes one's attitude toward a particular individual may be the same as the more generalized one. Very often it is not. One may be sincerely fond of, and favorably disposed toward, a particular Negro and still possess race prejudice and be anti-Negro. A person may be in love with a particular member of the opposite sex, but may have prejudicial attitudes toward members of the opposite sex as a group. He may elevate the particular member on a pedestal of social superiority, act obsequiously to her, sincerely feel inferior. Still, if he be an employer, he may be prone not to hire women. He could think and believe that a woman's place is in the home, that this is, and should be, "a man's world."

We can generalize these attitudes even further. Besides having a certain attitude toward a par-

ticular individual or a group, one also develops attitudes toward people in general; or even toward life in general. What do we mean when we say a person is not sociable? Or that he is an introvert? Or a cynic? We do not mean that he portrays an attitude toward a particular individual or group, but that he has a *general* tendency to act in a particular way toward all people and all groups. A cynic is not one who distrusts an individual as much as one who distrusts all people. The same holds true for an introvert. He is not so much one who "turns inwardly" in particular situations as he is one who practices introversion in most of life's situations. A "prejudiced individual" is one who is generally prejudiced. He may manifest his prejudices toward particulars only; on the other hand, one who is generally prejudiced will act in a prejudiced manner in many of, if not in all, life's social situations.

General attitudes therefore exist and are real, too. How else are we to understand the frequency with which prejudices often come in clusters? An individual who is *anti* a particular group is also anti-group B, C, D, and so on. We find that a tolerant person is not one who limits his tolerance to particulars. Rather, his tolerance usually pervades most of his relationships and experiences. General attitudes are chronic attitudes. The more numerous the situations that arouse a particular mode of behavior, the more general and basic the attitude behind the behavior. One who dislikes Negroes, Jews, Catholics, and children has a tendency to dislike people in general. One who dislikes people is a hostile being. And a hostile person possesses a basic general predisposition of prejudice. How his hostility will express itself depends on the definitions of his society and group. If a community is anti-Catholic, this hostile person will be prejudiced (overtly) toward the Catholic. If the community is anti-Negro, a basic attitude of hostility will arraign him against the Negro. People are what their dominant basic general attitudes are, and people do what the community dictates.

II

Specific attitudes are formed all through life, but basic general attitudes develop quite early in life and are mainly motivated by the family and other primary intimate face-to-face groups, such as the child's play group and the neighborhood. Unlike the specific attitudes, they are less capable of change and alteration, once formed. As long as an attitude is localized, it is capable of change, given a new experience. A person may change his attitude toward a particular individual when he "finds out more about him." A hostile person is not hostile because of a particular experience, but rather because of the fusion of many experiences in early life, until his tendency toward hostility becomes dominant. He has a general tendency to interpret all situations in a hostile manner, and his basic general attitude is not limited to particular objects.

Here again we are faced with an extremely difficult problem if we desire to effect a change, primarily because these attitudes are quite fully developed during the first years of life and, second, because they, too, are emotionally charged. In what other wise can we understand the high incidence of recidivism among criminals and juvenile delinquents? Although antisocial criminal behavior is not inborn, unless powerful reformistic programs are undertaken in our corrective institutions—and even then—one with a criminal bent tends to remain that way. It has often been shown that juvenile experts have ceased trying to alter the basic general attitudes of the bully. Once this aggression-domination tendency develops in an individual, it appears exceedingly difficult to reduce its degree. Without question, it appears even more dubious that a dominating, aggressive individual can ever change fundamentally into a nonaggressive, submissive type. In fact, juvenile experts do not seek basic attitudinal changes. Rather, they seek to constitutionalize the bully. Instead of allowing his bullying to express itself in the unsupervised street-corner gangs, the juvenile gang leader is made president of a club, or the captain of a baseball team. His aggressive tendencies still prevail, but now they are channelized into more acceptable social behavior.

If society could find a way of channelizing the hostile prejudicial tendencies of some people by furnishing outlets for their basic attitudinal drives, we might avoid the group tensions and group antagonisms that have not, of course, a place in democratic societies. If the bigot could be given the opportunity to fulminate his hatred against war, poverty, and disease—against objects of societal disapproval—the villain might even become a hero in the eyes of society. This is a task for education. Education of construction, that is—not education to reform the basic personality, but to open areas and outlets of satisfaction for already developed basic attitudes.

A more important task for education, of course—and specifically for the family as an educational unit—consists in integrating and normalizing the personality while it is being formed, in supplying the child with primary attitudes of tolerance rather than prejudice, with security, sympathy, and the

other human virtues of love, pity, concern, and sociability. This task for the family is the greatest challenge. It is easier to form attitudes *ab origine* than to change them *post maturus*. Baldur von Schirach, the leader of the Hitler youth movement, made an interesting and significant comment when he told an Army psychiatrist at the Nuremberg prison that he believed German children under ten could be directed toward democracy as they were toward Nazism, but that those older than twelve could be considered absolutely lost to democracy. He believed that just as Hitler "pulled young people away from their non-Nazi parents, so American authorities must remove today's children from their Nazi-poisoned parents."

Nazism was a general way of life in addition to the specific modes of reaction to particular objects. Nazis possessed basic general traits of social hostility to the outside world, group conceit (extreme ethnocentrism), personal and group aggressiveness, personal and group paranoia, group militance, and a tendency to blame others for their own and the world's ills.

Where do these general tendencies arise so early in life if not in the family? Studies of the development of basic general attitudes suggest that they are not a consequence of immanent mental development, but have their origin in the parents. Toward adolescence, children lose awareness of these origins and often devise rationalizations to support them. A young child, finding that his mother is not on speaking terms with her neighbor, refrains from associating with the neighbor's children. He will often accuse them with words repetitive of his mother's condemnatory remarks. And so he will take over his mother's attitudes toward other people and objects. If the mother portrays a general hostile attitude toward the outside world, her overt acts and expressions gradually fuse in the mind of the child until the imitation of his mother's specific negative habits of behavior becomes a general tendency to behave in a hostile manner. The many acts have become a basic predisposition which is fundamentally the same as the mother's predisposition. As the child matures, this basic dominant trait will force and compel specific behavior in specific situations. In extreme cases he will even offend those particular individuals he sincerely loves. A general tendency to behave colors the specific act itself. Man does not evaluate every stimulus that confronts him; rather, the stimulus becomes for him what he makes of it. And he makes of it what his dominant basic attitudes are. "Children," says George Brock Chisholm, Director General of the World Health Organization, "must be taught to live harmoniously together or mankind will follow the dinosaur into oblivion."

In order for children to develop the "correct" attitudes parents must be made aware of their inherent prejudices. How can this be done? Some self-analysis becomes necessary. It should be pointed out to adults—and here the task for adult education becomes most significant—that persons coming from particular strata and segments in society tend to possess the interests and biases characteristic of that segment. Only the few are the exceptions. A white, urban, Protestant, wealthy, middled-aged, Northern male will not possess the same tendencies to act toward given values, groups, individuals—will not have the same interests, attitudes, and predispositions—as a colored, rural, Catholic, young, poor, Southern tenant farmer. Can these two possibly feel the same toward the question of social security, toward equal educational opportunity for all groups, toward taxation, and an endless variety of other social issues? Residence in particular segments of society bring pre-existent reaction patterns.

White persons must be on their guard to avoid slips of the tongue about Negros—especially in front of children. Protestants can exert special effort in not expressing or showing through divers manners their dislikes of Catholics or Jews—again, especially in the presence of their children. On the other hand, the adults comprising minority groups can minimize their feelings and attitudes of insecurity, withdrawal, resignation, and unnatural aggression by becoming aware of these tendencies

Only in this way can we break into the perpetual cycle of those who have prejudices passing them on to those who have not, thus making an endless chain of hatred and antagonism among men. Adults can and should be made conscious of their predispositions so that their children need not duplicate their parents in this regard. If the adult avoids specific acts and expressions of prejudice, no groups will consequently become defined in the mind of the child as objects of antagonism. If no groups are thus ill-defined, the child will obtain nothing to integrate into a *general* attitude of prejudice.

People often ask whether there are not any *good prejudices,* such as prejudices against war, crime, and poverty. Prejudice means prejudgment. If one's beliefs and ideas result from prejudgments it is difficult to see that an attitude for or against any object, group, or value can be condoned. Absence of reason is a poor excuse for any judgment, even if it happens to further the good society. The good society can be and should be reasonable. It surely cannot succeed through capriciousness.

THE WILLOWS: HELPERS OF MAN

CARLETON R. BALL

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WILLOWS are native to all continents except Australia. Although they are nearly world-wide in distribution, most of the 500–700 species are found in the Northern Hemisphere. About 20 kinds occur in temperate Africa and one in South America.

Three recognizable but overlapping east-west belts of willows occur in the Northern Hemisphere. The relatively few tree willows of the warm-temperate and subtropical belt extend from the Philippines across southern Asia, the Mediterranean region, and southern North America. They are typified by our eastern black willow (*Salix nigra*) and western red willow (*S. laevigata*). The cool-temperate belt of some hundreds of shrubby willows extends across the central part of the three continents; these willows are typified by our Bebb willow (*S. Bebbiana*) throughout; sandbar willow (*S. interior*), central; and cordate willow (*S. cordate* Muhl.), eastern. The arctic belt of some 100 kinds of little prostate willows extends into the Arctic Ocean, and southward on mountain ranges into the United States. The circumpolar arctic willow (*S. arctica*) and net-veined willow (*S. recticulata*) are typical.

In relation to sea level, willows range from 200 feet below in California to 15,000 feet above on various mountains. Two of our species, the sandbar willow and the western shining willow (*S. lasiandra*), range from about latitude 30° (Louisiana and Mexico, respectively) to 67° on the Yukon River in Alaska, and withstand temperatures of 100°–130° above zero F. in the South to about 75° below zero in the North. Willows in general prefer moist to wet situations, but our dry-land and desert willows withstand long periods of drought.

The tree willows vary in height from 15 to 120 feet, with one to four trunks. Shrubs vary from a few inches to 15 or 20 feet in height, with few to scores of stems. The prostrate willows rise in height from a fraction of an inch to 1 or 2 inches, and form mats from 2 inches to 1 or 2 feet across.

Willows are widely used for cover, shelter, homes, and/or food by many kinds of animals, birds, insects, and even fishes. These associations supply many common names, such as willow ptarmigan and warbler, willow moth and sawfly, and even willow catfish. Many plants, having narrow leaves like the traditional willow leaf, are named from this resemblance (willow herb, willow oak, and desert willow). This traditional idea of the shape of willow leaves ("willowy") arose from the long slender leaves of the tree willows of the ancient world. In fact, however, the leaves of willows vary from literal toothpicks through linear, oblong, lanceolate, and oblanceolate to oval, ovate, obovate, suborbicular, and completely circular in shape, or even circular with both ends indented.

Willows produce enormous numbers of tiny parachuting seeds, which reproduce sparingly and die soon. Much reproduction is by twigs blown or broken off and washed or tramped into mud, sand, or gravel. They spread also by underground stems and roots, which may run out for 5–75 feet, sending up shoots as they go. Willows can produce buds or roots from any part of the stem or root.

The willows, genus *Salix,* with the closely related poplars (aspens, cottonwoods), genus *Populus,* comprise the willow family—Salicaceae. In English, the willows are called willows or sallows, and the basketmaking sorts often are called osiers. The word "sallow" occurs in all medieval languages. The name "willow" (*welogh, wyllo*) is derived from the Anglo-Saxon *welig, wilg,* etc. and the Low German *wilge* and *wichel* ("wicker").

USES OF WILLOWS

Willows have been useful to man, directly and through his animal friends, in a thousand and one different ways since before the dawn of history. These uses enter into every phase of his existence, material, physical, and mental, in activity and in relaxation. Willows have supplied man in all

stages of his development, from most primitive to most "civilized." Just a bare list of the things made or done with willows forms an amazing tableau of the upward climb of the human race.

Sentiment and superstition. Willows have entered into the ceremonial and religious rites of many primitive peoples (including Aryans). Of the six references to willows in our Old Testament Scriptures, four make them the symbols of fertility, vigor, and rejoicing; only one associates them with mourning (Psalms 137: 1–2, Children of Israel Captive in Babylon). From then on, our popular association of willows with mourning has been dominant.

Willows were important among the *arbores tristes* ("trees of sorrow, or mourning"), used in burial places from time immemorial, and our present use of the weeping willow in cemeteries is a custom survival. In twelve of Shakespeare's plays, the willow is the emblem of mourning, sorrow, and tragedy; in *Hamlet,* Ophelia drops to her death from a willow. Throughout medieval times, songs of sorrow were widely popular, and in many of them the willow was the symbol of grief, desertion, and despair. Garlands of willow were worn by forsaken lovers.

Superstitious beliefs about willows are many. In parts of Europe, it is believed that geese hatch from pussy willows; also that rheumatism will be cured if the victim walks thrice around a weeping willow as the sun sets. In the superstitions of the ancient and modern herbalists, willows were ascribed almost miraculous powers over disease, especially of the sexual organs. Man still uses withes as divining rods.

Art and ornament. Willows long have been subjects for paintings in Europe, especially those of Corot, and there are beautiful Chinese examples. Photographs, books, magazines, and photogravure supplements frequently portray them. The Oriental willow pattern in chinaware has been introduced around the world in past centuries and recently has been transferred to wallpapers.

The use of willows in ornamental plantings is universal. Some trees, like the white willow, are grown for the beauty of the silvery-silky leaves; others, like the weeping willow, for their pendulous, swaying branchlets. Many shrubby kinds are grown for their leaf effects in summer and the bright reds and yellows of the buds and twigs in winter. Pussy willows are widely used for the beauty of the furry yellow male catkins on leafless stems in early spring. Two European species, florist's willows, decorate our homes and churches at Eastertime.

Making of wooden products. Willow wood is used for many and varied products. Light in weight and usually light in color (caused by low mineral content), it does not warp, check, crack, or splinter if properly seasoned. These qualities give it high value for many purposes. The four chief classes of material used are timber (trunks and poles), lumber, withes (slender rods), and roots.

Willows long have been cultivated commercially abroad. In America, they are seldom used for wood products, except withes for basketry and wickerware. Timber and lumber here normally come from wild plants or those grown for windbreaks, soil-holding, or ornament. Native black-willow timber is cut largely along the lower Mississippi.

The making of containers for picking, carrying, weighing, storage, and shipment of various products is an enormous industry. It is ancient and modern, primitive and civilized, household and commercial. Willows are preferred because they are light, easily worked, and durable. Withes and roots and sawed lumber are used in four different types of construction: withes or roots woven; narrow strips of thin lumber woven or plaited into baskets or crates; the same nailed to heavier braces or ends for boxes and crates; and all cooperage (barrels, etc.).

Use of peeled willow withes, and roots, in making baskets, hampers, and panniers is very ancient, having been widely established in Greek and Roman times. Today, baskets, hampers, and crates are universally used for picking, carrying, measuring, weighing, storing, and/or shipping grains, seeds, vegetables, fruits, berries, melons, nuts, flowers, groceries, butter, eggs, cured meats, fish, bakery products, fertilizers, ammunition, lime, earth, sand, gravel, kindling, fireplace wood, laundry work, yarn, thread, buttons, sewing materials, and also cages and coops for birds, animals, and poultry. Many Indian tribes are experts in the making of beautiful baskets of withes, split withes, and roots.

The plaited and reinforced containers are used for many of these same purposes, especially in shipping. Cooperage includes barrels, half-barrels, casks, kegs, tubs, etc. Tight cooperage is used for liquids, products packed in liquids, and fine materials like sugar, salt, sulphur, etc. Slack cooperage may be used for firm fruits, cranberries, vegetables, and nuts.

The principal, long-time uses of willow wood in agriculture have been for fences, orchard props, vineyard trellises, tying materials, tool handles, and tools.

In Europe, willow poles and rods are used extensively as wattles or fillers in open fences and

FIG. 1. Crack Willows, Du Pont Powder Mills, Wilmington, Delaware, pollarded (trunk cut off several feet up) and each producing scores of slender canes for charcoal used in gunpowder manufacture. (Photo by U. S. Forest Service.)

also in constructing movable sheep fences, called hurdles. In the drier, unforested parts of America they have been much used for fence posts. Such fences often carry the rural telephone lines, and willow poles carry the line to the farmhouses. Early-day rail fences were interwoven with willow poles to restrain hogs and sheep, and later the few-wired slack barbed-wire fences were reinforced with willow poles or rods for the same reason.

Willow poles, props, and trellises have been used in gardens, orchards, and vineyards from ancient times. (The Romans wrote extensively on growing and using willows in their agriculture and horticulture.) All over America, they still are so used. California orchardists suffered loss when a willow weevil moved from the props to the trees and became a serious pest. Willow poles long have been used for home making of handles for hoes, rakes, brooms, shovels, etc. This use, once common in America, still goes on in Eurasia. In China, three-tined pitchforks are made from properly branched stems dried under pressure.

English beehives ("bee slips") still are made of willow. Willow wood often forms the blades, and sometimes the frames, of water wheels, because it is uninjured by water. Fans for winnowing grain were made from willow by the Romans, and probably wicker sieves also. Slender willow withes as tying material date back to antiquity and still are used by modern florists, gardeners, and vineyardists.

Willows furnish fuel in many unforested regions to explorers, travelers, and dwellers, the latter using both wild and planted. One brick manufacturer in Missouri fires his kilns with willow. Arctic willows furnish fire for outdoor summer cooking. Some Indians use willow for smoking fish, claiming that no taint is absorbed. Eskimos twist beaten cottony materials (willow seed hairs, moss, etc.) into wicks for oil lamps, and Siberian natives make candles of willow twigs shredded by stone beating, then braided together and soaked in blubber oil.

Wicker furniture, from woven willow withes, is of ancient origin. It now includes armchairs, rocking chairs, settees, tables, cribs, baby and doll carriages, perambulators, etc. Dressed willow poles usually form the legs, braces, and frames. Rustic furniture is made of willow poles, debarked or with bark, and includes chairs, benches, and settees. These are not smoothed, polished, or painted and are used mostly out of doors. Willow lumber does not take a high polish and is used generally for interiors in cabinetwork, such as drawers and shelves for bookcases, bureaus, cabinets, closets, desks, tables, and also in pianos and refrigerators.

Willow is widely used for some household

utensils because it is light, free from warping and cracking, and resistant to heat and water. Among kitchen and store utensils are bowls, ladles, scoops, spoons, and trays. Other articles include lap-, sleeve-, and ironing boards, and cutting boards and tables for leather workers. It is a favorite carving wood and is used in teaching that art. Siberian natives use leafy willow twigs for covering tent floors, and Amerinds make floor mats from willow twine. Willow twigs beaten and shredded at one end are used by Mohammedans and some primitive peoples in religious and ceremonial rites; these shredded twigs are probably the prototype of toothbrushes. Our Southern mountaineers used similar twigs as snuff sticks.

Willows provide important equipment for man when he walks, or rides on animals or in vehicles, or travels by water. Indian tribes make sandals from willow twine, and, in the Low Countries of Europe, all the widely worn wooden shoes are made from willow, because the wood is light and not warped by wetting. Canadian Indians use willow for snowshoe and hand-sledge braces. Aids to walking include wooden legs, crutches, and canes. Wooden legs have been made from willow since shaping them began in London some 125 years ago. Our "diamond-willow" canes are much prized as novelties.

The frames of Greek and Roman coracles (small, tub-shaped skin boats) were willow, and probably their oars or paddles also. Recent European coracles still are willow-framed. Eskimo kayaks, or skin canoes, have willow frames and spreaders. Small boats, and oars and paddles, still are made from willow by white men, as are also the keels for larger boats. River steamers usually burn bank-cut cordwood, and in Mark Twain's day some of this was willow—and perhaps it still is today.

For land travel and transport, by horse and vehicle, willows long have been useful. Perhaps the earliest transport of supplies (and children) was in woven willow panniers, balanced one on either side of an animal. When the Spanish ruled California, the "trees" of their famous and widely exported saddles were of willow. The *travois* poles of the Indians, French guides, and trappers were willow, the forward ends attached on either side of a pack pony, the rear dragging, with a load lashed to crossbars behind. The country boy astride Old Dobbin carried a willow switch for service and sentiment. Wicker baby carriages and perambulators transport our young. Willow is used to line carts, wagons, and wheelbarrows in Europe and also to line the brakes of wagons and tram- and railway cars.

In fishing and hunting equipment, the farmer boy and his willow fishing pole are well known. Sioux Indians snare the long-bodied pickerel with a running noose on a willow pole, and Northern Indians seine fish with willow-twine nets. Conical fish weirs, or traps of willow withes, are set in narrow channels to catch fish. Northern Indians attach rabbit snares to bent-over willows, and use willow-handled ice bags and strainers when fishing through ice. Willow withes tie the rails of the trap corrals and the poles of the caribou lookouts of Alaska-Yukon natives. Bear Lake Indians use a willow whistle to call loons, and they call moose by rubbing the shoulder-blade of a moose against willow stems, thus imitating the noise made when the animal rubs the velvet from his antlers in the spring.

Baseball and cricket bats, polo balls, fishing poles, doll carriages, snow-trail markers, whistles, rattles, and various toys are also made from willow, and willow bats and fishpoles are universal in juvenile America. All the cricket bats, standard and junior, of the British people are made from white willow. Withes mark trails accross blowing snowfields for mountain climbers. White and Indian children alike make willow whistles, and the latter also make willow rattles containing stones.

Both ancients and moderns have used weapons of willow. The rawhide-covered shields of the Roman legions were woven from four kinds of willow. Slave Lake Indians use long shields of willow withes as defense against arrows. German willow holts in World War I were overcut to supply ammunition crates. Willow charcoal was universally used to make gunpowder. Many Indian bows, and sometimes arrows also, were made of willow.

Excelsior, shredded wood, and wood-pulp are the basis for a large modern industry, of which willows probably furnish only a small part. Excelsior mills have used some willow from the beginning. In the Mississippi Valley, where black willow is abundant, it stands about fifth in rank of woods used. Willow is used as a mixture, and only in the coarse and medium grades. Candy makers have claimed that willow excelsior taints candies, and butter makers have claimed the same about wicker crates, but this is contrary to the beliefs of the Indians and many whites. Willow wood has but limited use in making paper pulp, partly because its short fibers must be mixed with longer-fibered woods. Shredded wood is a small industry and seems to use willows in the limited ways and proportions of the other two industries.

Chemical and medicinal uses. Chief willow uses in the chemical industry are for charcoal, dye-

FIG. 2. Crack willow windbreaks on muck soils (*left* and *right*). Ten years old and 22 feet high after being cut back to 12 feet two years before. (Photo by courtesy of Professor Paul M. Harmer, Michigan State College.)

stuffs, rust preventive, and tanning material. Soft light woods, like willow, were easily charred by primitive methods and, being relatively mineral-free, were suited to chemical uses. Charcoal was the basis of black gunpowder, invented by the Chinese and early brought to Europe. The crack willow (*S. fragilis*) has been a chief source of such charcoal from the beginning, and our colonists brought and planted it here. After smokeless powder was invented, willow charcoal was used in gas masks and in preventing rust in barbed wire and other much-exposed metals. It still makes black gunpowder (Fig. 1).

For home dyeing, willow bark, leaves, and roots have provided dyestuffs from earliest times, and still do. Tannin from willow bark long has served special industries; the famous gloves of Scania, Sweden, have been tanned with it for 3,000 years. It is often stated that our Indian tribes used willow for smoking, like tobacco, but their smoking was ceremonial, not a habit, until after the white man set the example. Many plants, including willow, were used, even under the name *kinnikinnik*, often said to be wholly willow. In the Arctic, the Eskimos collect the fluffy seed hairs in summer for use as tinder in winter. Dried willow leaves have been used to adulterate tea, both here and in the Orient.

Willow bark, charcoal, buds, leaves, twigs, and flowers have been used in medicine by both primitive and civilized man throughout historic time, mostly as decoctions and infusions. These have been used for every kind of ailment, disorder, and disease of every part of the human body, external and internal. Some uses had a basis in fact and from willow bark, has been in the U. S. Pharmacopoeia since 1820. First used as a quinine substitute, it now is employed as antiseptic, antiperiodic, febrifuge, antirheumatic, tonic, preservative, and skin healer. The ancients were not wholly wrong.

Cordage and clothing. Of willows, the bark, inner bark, or bast, and the silky seed hairs have been used for fiber, in both Old and New Worlds. Indians made twine, fishing lines, and fish nets from strips of bast fiber joined at the ends and rolled in pairs over the thigh. Stouter cord for backing fish nets was made from more and heavier strands. The Indians also made mats and sandals from this twine, and women of some Arizona tribes made skirts from strips of bark first buried in blue mud, then washed and dried. The soft fibrous strips were cut to the desired length and fastened thickly to the belt. In northern Europe, the abundant seed hairs have been used, in mixtures of one-third cotton and two-thirds willow hairs, for making clothing.

Food and feed for man and animals An amazing array of animals, including man, obtain some of or all their food from willows. The list includes many mammals, various birds, numerous kinds of insects (plus termites), and also parasitic fungi and mistletoe.

Willows are a staple food for man under arctic conditions. Scurvy, the dread dietary disease, kills when vegetables are lacking. Circumpolar arctic natives regularly prepare quantities of fresh vegetable matter for winter use, and willow buds, twigs, leaves, and catkins commonly are used; root bark and inner bark (cambium), occasionally.

plants may be included. The material may be eaten as salad or fermented to sauerkraut, supplies of both being preserved in oil for winter use. Masses of willow twigs and leaves from the paunches of slaughtered deer and caribou are eaten in emergencies by Yukon natives; lichens, willow twigs, and even evergreens may be gnawed by stranded natives and explorers in dire need.

The inner bark of tree willows was dried, powdered, and used for making bread in northern Europe and by American colonists in times of great scarcity, and willow leaves are eaten in China when the crops fail. Willows are listed as honey plants in forty-four states and many countries; early-flowering willows furnish honey for brood-rearing, and surplus for sale.

Willows furnish an immense amount of forage throughout the Northern Hemisphere, mostly as browse for livestock and cud-chewing game animals (antelope, bison, caribou, deer, elk, moose, musk oxen, and reindeer). Practically all kinds of willows are used for forage, because some are found in every environment and because different kinds of mammals, birds, and insects prefer different willows.

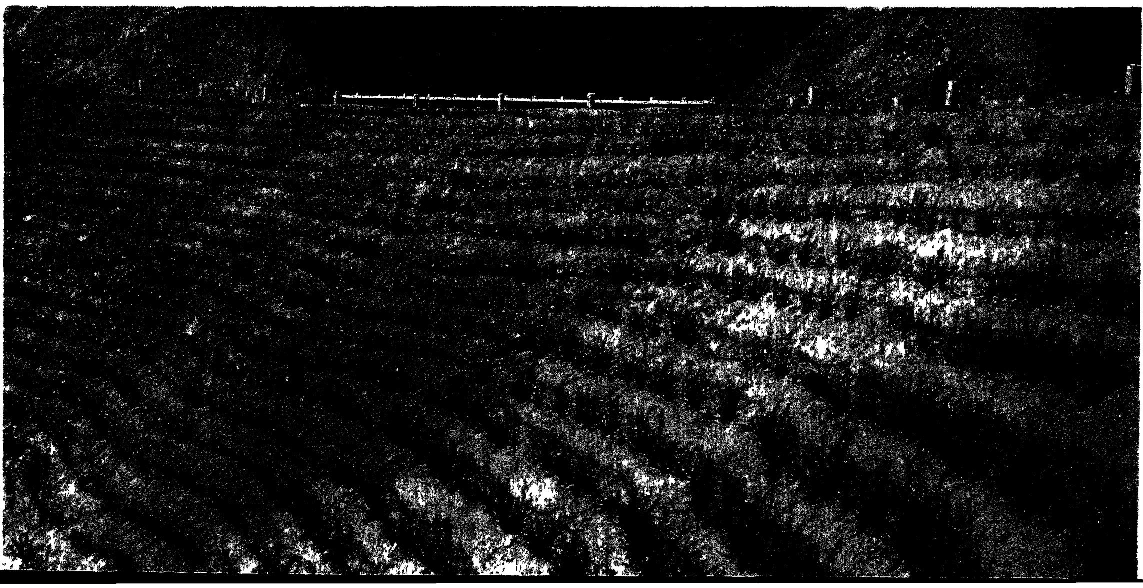
Much forage from all forested areas is browse from woody plants, willows being favorites. The U. S. Forest Service derives large annual revenues from grazing fees, and our National Parks and other public areas support abundant game animals. In the Arctic, both livestock and wild cud-chewers browse largely on willows in summer, and the latter do so in winter by pawing away the covering snow. Because the arctic summer is short, the leaves and catkins often do not fall in autumn, and so furnish extra browse in winter. Willows taken from the paunches of slaughtered reindeer have been identified as a guide to good winter grazing areas.

Willows frequently have saved livestock and game during emergency shortages caused by severe drought or cold in dry lands, and they have been introduced into Australia for this purpose. When cattle are marooned on high spots by river floods, they may be fed on willows gathered in boats. In Eurasia, willow shoots are dried or siloed for winter feeding.

Young bears will eat willow buds occasionally. Among rodents, or gnawers, beavers eat willow bark habitually and mountain beavers store willow twigs and branches in their burrows. Muskrats will gnaw willows sometimes. The rabbit family destructively debarks the stems of wild and cultivated willows, and the arctic snowshoe rabbit debarks above the snowline in winter, competing with the moose herds for food. The mountain conies cure little willows in their "haystacks" for winter use. Mice may girdle willow trunks or gnaw the roots until plants blow over, or debark branches to the very tops of trees.

Many birds of widely different families use willows more or less commonly for food: Different kinds of grouse all eat willow buds and twigs to some extent, as does the California quail; the willow ptarmigan uses the arctic willows for both shelter and food throughout the year; ducks scoop up floating buds, leaves, and capsules as they swim, and probably eat some from trees when roosting;

FIG. 3. Holding a new road fill on a California mountainside by thick plantings of red willow (*Salix laevigata*). (U. S. Forest Service photo.)



sapsuckers eat the inner bark and drink the sap; and many other birds occasionally feed on willows.

Numerous kinds of insects live on or in willows and obtain food from them during some of or all their four stages. Bees and wasps use willow pollen extensively in early spring for starting comb, and gather nectar for honey. Some butterflies and moths feed on nectar, but their worms feed on willow tissues. Many infesting insects create curious distorted growths, called galls, in twigs, buds, leaves, and flowers and live and feed inside them; others eat the inner bark, or tunnel in the wood. Termites eat out the wood of posts, poles, boards, and planks, often injuring buildings seriously.

For cover, shade, and homes. Willows furnish temporary cover or shelter to many kinds of insects, birds, and animals, including man. This is used, at times, to avoid winds, storms, or sun, to escape from enemies, or to rest. The dense growth of willows gives a maximum of protection. Even some water birds, such as ducks and herons, roost in willows near the water. An exploring party, threatened by Indians in Colorado, "retired to a small grove of willows" to make their defense.

Natural willow growth furnishes shade for animals coming for water and to rest after feeding. Tree willows with two to four leaning trunks afford maximum shade, and low-hanging branches keep biting insects off. Farmers often plant such trees near watering places and barns. Jack rabbits on the plains will sit in the shade of willow fence posts on hot days, moving with the sun. Kiowa women used leafy willow wreaths as sunshades while working. Willows, fast-growing, are used as nurse plants for new-planted, slow-growing trees

Many kinds of fungi live on or in willows. In Europe, one species of mistletoe grows on willow. One most peculiar "housing development" is provided by the huge old pollarded willows once grown for making gunpowder. These beheaded trunks, covered with cane stubs, collect leaves and dirt, in which grow many kinds of plants, including trees, whose seeds have dropped or blown in.

Willow timber is little used for houses, because it is not durable when exposed. Rafters and floors of such wood, however, kept dry, have been long-lived. Many Indian tribes have used willow poles for their conical tepees and as roofing poles on their flat-topped lodges, hogans, or jacals; these were then covered with thatch and earth.

For windbreaks and as soil-holders. Living windbreaks or shelter belts stop, slow down, or deflect wind sweep. This is needed on islands, ocean and lake shores, river valleys opening to the sea, and on treeless plains. Only thus can crops and livestock be protected from injury. Many plants are cut off by drifting sand and soil particles. Most shrubby willows form close thickets, and tree willows cut back produce the same effect. In wider shelter belts, several other kinds of shrubs and trees are used with willows, whose rapid growth and usual recovery from injury make them favorites.

FIG. 4. Close-up of red willow cutting used for road fills. Note the many vigorous shoots from the driven stem. (U. S. Forest Service photo.)

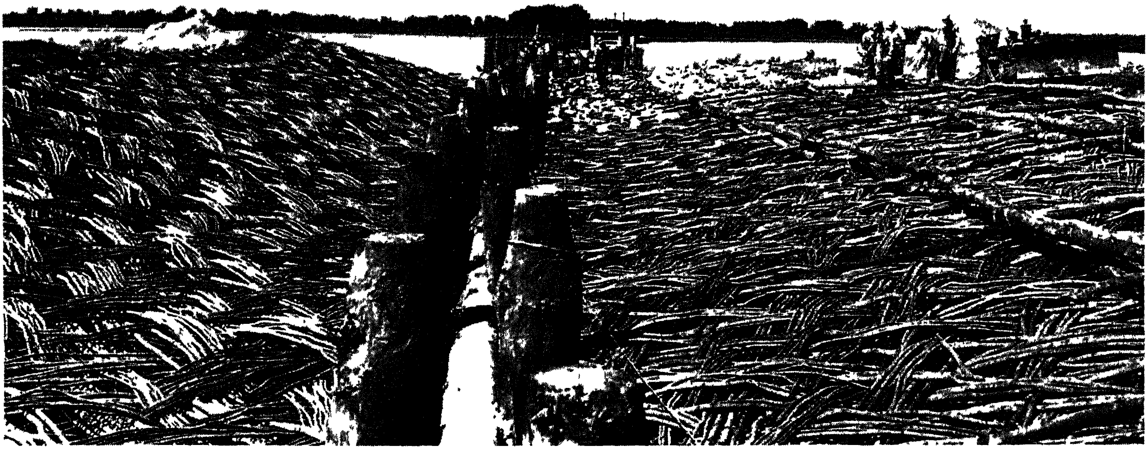


FIG. 5. Willow mattress in readiness for rock ballast, Sandy Point Bend, Missouri River. To be used for bank protection. (U. S. Army Engineers photo.)

On our muck soils, dry, blowing peat fragments cut off tender truck crops (Fig. 2). On treeless steppes and plains, Old World and American, shelter belts are used to break the force of the winds.

Throughout the world, two gigantic forces, wind and water, work eternally to move soil and sand and gravel to new locations. Removal of fertile topsoil, gullying of land surfaces, and eating away of stream banks may ruin agriculture where occurring. The deposit of these materials, good and bad, may produce ruinous effects elsewhere. Willow cuttings are widely used to protect stream banks, tie shifting sand and gravel bars, to stabilize gullies, hold lake shores, and reclothe mountain slopes denuded by lumbering and/or fires. Levees, dams, reservoir walls, and road fills (Figs. 3, 4) are protected from washing by willow plantings—60,000 on one California levee. Along the lower Mississippi River, Army Engineers hold crumbling banks by huge mattresses or aprons of interlaced living willow poles (Fig. 5) which, weighted down with earth, soon produce an impervious willow thicket. Injury to crops and painted buildings by blowing sand, and the merciless advance of sand dunes into towns or across farms, are stopped by plantings of willows and other sand-holders.

Willows as indicator plants. From earliest history, man has associated willows with water supplies. But willows grow also on dry plains and in deserts, and under these unfavorable conditions they often indicate seeps, springs, or underground flow. Knowing this has saved the lives of water-short desert travelers and prospectors.

The condition of willow clumps in range lands indicates the severity of grazing. If they are not spreading, are browsed to maximum reach, and portions are dying, overgrazing is severe. Willows also indicate the years elapsed since a forest burned, and the presence of excess alkali.

Willows as weeds. It is only fair to record that sometimes willows hinder and harm, instead of help. They hamper irrigation by invading ditches, slowing water flow, causing silt deposition, and using the water; they do the same in shallow streams. Sometimes they invade pastures or forest plantings, but poisoning or heavy grazing will hold them down. Sometimes they clog drain tiles with root growth. They furnish food and housing to many insects, some injurious to them, to man, or to animals. Finally, their abundant pollen causes hay fever in susceptible persons. But all these are trivial compared with the help they give.

ONE HUNDRED YEARS OF CALIFORNIA PLACER MINING

HERBERT A. SAWIN

Mr. Sawin is sales engineer for the Yuba Manufacturing Company, of San Francisco. His article is based on an address given in the symposium on "Mining Methods" at the Annual Meeting of the American Institute of Mining and Metallurgical Engineers at San Francisco, February 13–17, 1949.

VAST changes might have occurred in world history if some early explorer had learned more about the land he had found. Suppose, for instance, that Sir Francis Drake had known about the Sierra Nevada foothill placers only two or three hundred miles from where he left a brass plate claiming for Queen Elizabeth that immense territory now one of our country's greatest treasure stores. (Drake's brass calling card left in 1579 was found a few years ago near San Francisco Bay and can be seen in the University of California library in Berkeley.) The treasure he sought was lying in stream beds and practically on the surface of many square miles of California countryside, a relatively few miles from where he camped.

The course of our national history and development moved from the east coast westward because early European immigrants found land and, later, mineral wealth in New England, New York, Pennsylvania, and in Southern seaboard colonies. The trek West came much later. The fight for survival produced a hardy race and, fortunately, the adventurers who came to California in the days of '49. In 1848 and 1849 enough Americans to outnumber the foreigners heard the cry of "Gold in California." American Army and Navy detachments were already in California because of the war with Mexico and held the fort, so to speak, against other possible claimants.

Gold was not unknown to Californians prior to 1848. Mexicans used a wooden batea, forerunner of the gold pan. The Philadelphia Mint gold bullion ledger bears an entry under date of January 30, 1838, crediting Hussey and Mackay with three gold deposits "from Calafornia." One item (No. 21) is recorded as "309 ozs.—74 dwt. native grains." This probably came from Southern California. The Indians of California also knew of the existence of placer gold, but apparently thought it of little value.

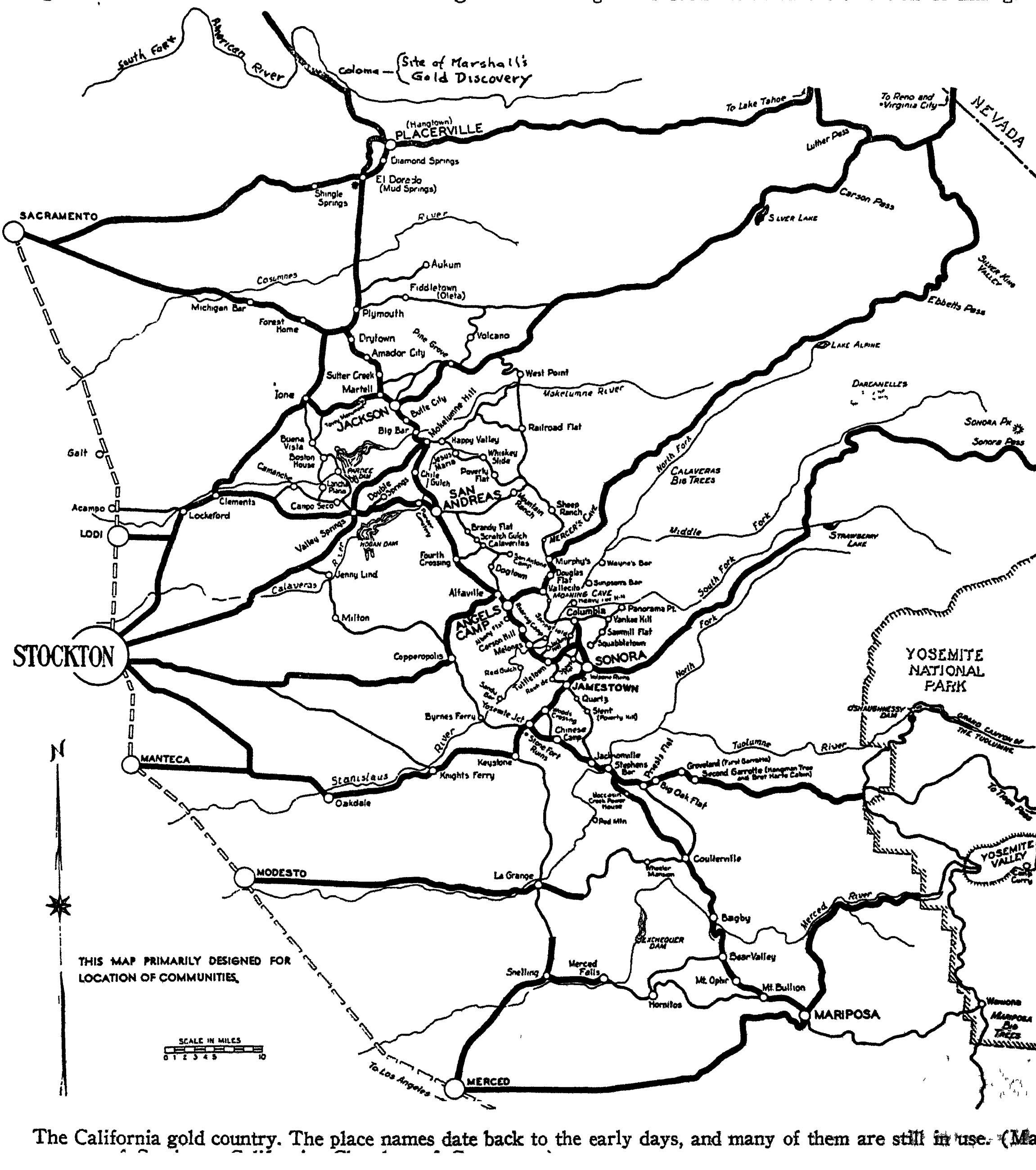
It was James Marshall's discovery of placer gold in John Sutter's millrace in Coloma in January 1848 that brought the hordes of wealth seekers in 1848 and 1849. Hard work and discomfort, disease and short rations—these meant little to the pioneers when gold was to be had for picking it up. Sutter owned many acres of land by legal grant from the Mexican government, but squatters overran his property, and the unfortunate Swiss, who had become a Mexican citizen, spent his last days vainly seeking redress from the Congress in Washington. By 1850 probably 100,000 persons had come to California, and by the end of 1852 the population, not including Indians, was about 223,000. The records are not complete or conclusive, but the 1850 census places the percentage of females in California at 7.5. The new country was a place for men, men who were interested only in the riches to be picked up and taken back home for investment. The thrilling story has been pieced together from many sources by Rodman W. Paul in his book *California Gold* (Harvard University Press). The flush days of placer gold production continued for twelve years or more. The annual value of gold from California, Paul states, reached a peak of over $81,000,000 in 1852. Authorities agree that until as late as 1855 most gold from the state came from placers. Earlier, lode mining had been started in Amador and Mariposa counties. Lode gold in quartz had been found by placer miners, and, after many attempts and failures, methods for mining and milling were developed. That, however, is another story.

Many early surface miners were drifters, always looking for a richer claim, listening to rumors and moving on at the whim of fortune. Our expression "It didn't pan out" can be traced to the experience of men who staked a claim for placer gold and found few or no colors in their pans. The early-day take has been estimated at $20 per day per man as an average, with some, of course, running much higher. In the first few years gold hunters spread quickly over an area extending from the American River drainage basin to the Bear, Yuba, Feather, Trinity, Klamath, Salmon, and Smith rivers to the north. South they mined placers of the Stanislaus, Mokelumne, Cosumnes, Merced, and Fresno rivers. A boom in 1855, built up, some say, by

deliberately false reports, led many miners to the Kern River far south. The boom soon collapsed.

Miners on the move soon found that the deep gravels in the elevated Tertiary channels carried gold. As early as 1850 these were worked by hand, but as knowledge of the formation was gained, new tools were devised, and these brought into being the great hydraulic mining days of the state. Inexperienced miners probably reinvented rockers, sluices, and other equipment because of the urgency and the need for tools. Miners with experience in Georgia or South America were held in great esteem because of their knowledge of gold-mining equipment. It is estimated that by 1876 more than $100,000,000 was invested in hydraulicking equipment, with an annual yield ranging from $11,000,000 to $13,000,000. In California Division of Mines Bulletin No. 92—*Gold Placers of California*—Charles Scott Haley has this to say:

> While the hydraulic mines were flourishing, the owners apparently overlooked the growth of another industry which was one day destined to drive them from their own field by the action of the courts. The broad valleys, where the tributaries of the Sacramento and San Joaquin unite with these streams, teemed with fertility and these rich farming lands soon attracted the attention of immigrant

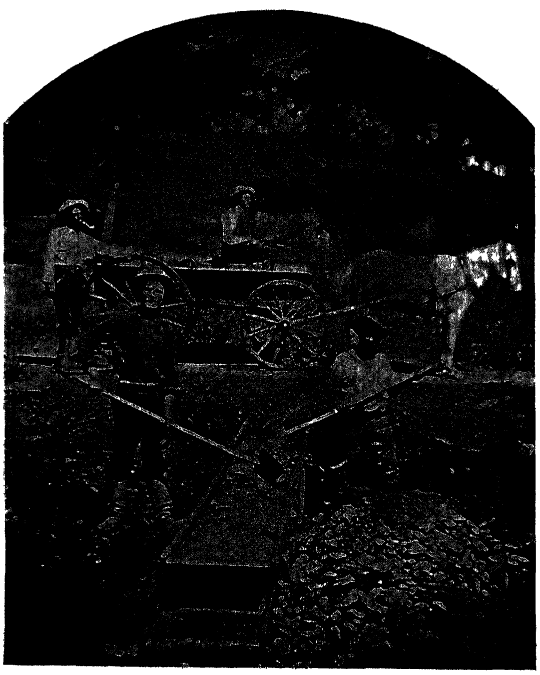


The California gold country. The place names date back to the early days, and many of them are still in use. (Map

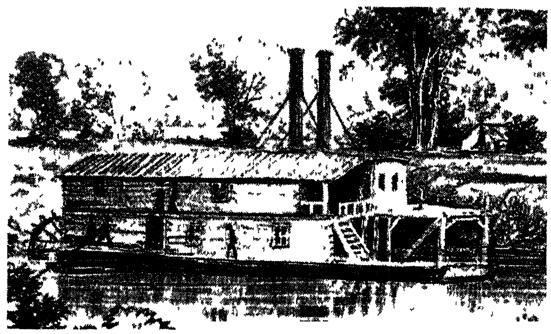
farmers. Land, which the miners could originally have bought for a song and held as a perpetual dumping ground, steadily mounted in value and its increasing productivity permitted the farming interests gradually to outgrow the mining in economic importance.

The accumulation of debris in the rivers was long a source of annoyance and considerable damage to the farmers. Even during the days of ground sluicing, the great flood in the winter of 1862 covered the richest bottom and orchard lands along the Bear and Yuba Rivers with tons of debris from the gold washing. At this time however, mining was regarded as the more legitimate and powerful industry, and but little protest was made. As the strength of the agricultural interests grew, protests against the overloading of the rivers became more and more frequent and more powerful until the struggle culminated in the famous Sawyer decision on January 23, 1884. This decision, in the form of an injunction, handed down by the United States Circuit Court, in the case of Edwards Woodruff *vs.* North Bloomfield Gravel Mining Company *et al.*, wiped out at one blow property values exceeding one hundred million dollars and indefinitely postponed the addition to the world's wealth of what has been roughly estimated at from five hundred million dollars upwards in the value of placer gold.

The struggle between miners and farmers resulted in the passage by Congress in 1893 of the Caminetti Act. This created the California Debris Commission, with jurisdiction extending over the drainage area of the Sacramento and San Joaquin rivers. Although the law does not make hydraulic mining illegal, it cannot be carried on if directly or indirectly it results in injury to the navigability of the two rivers, or injury to adjacent lands. Dams



Long Tom used for ground sluicing for gold. California about 1852



The "Phoenix," first dredge in California. Used on the Yuba River about 1850 (Photo by courtesy of the California Historical Society.)

have been built on the Yuba, but charges for impounding tailings are high. The intent of the Act, as miners hoped, was to return hydraulicking to its former important status. This has not been accomplished, although some small-scale hydraulic mining is done in Sierra Nevada rivers. Today, to see hydraulic mining in California, one must visit mines in the Trinity and Klamath river areas. These are outside the jurisdiction of the Caminetti law. There are those who claim that instead of harming farmlands, hydraulic slickings built up and increased the fertility of lands along the rivers. It is well known that many low spots, once breeding grounds for malaria-carrying mosquitoes, were filled in and the surface elevated many feet. The health of near-by residents must definitely have been improved by the elimination of such pestholes.

An important contribution to California gold history came through the operation of river mining companies. By the use of dams, ditches, and flumes, streams such as the Feather and the Yuba were turned from their natural courses and the bottom gravel washed for values. Potholes were rich sources of accumulated gold, deposited there by the stream action of thousands of years. Water could be turned aside and controlled only during the low-water season, say, from July 1 until the early rains started in November. The ventures faced two variables, the length of the working season and richness of the bottom gravel. Eastern and foreign capital was invested heavily in river mining, and before 1858 there were many such projects, involving dams and flumes, costing $100,000 or more each. Daily operating expense was high, but when luck held out large profits were realized. Conversely, great effort and cash outlays sometimes brought failure. Early high water sometimes washed out wing dams and destroyed a season's preparatory work before enough gold had been recovered to pay the costs, let alone any profits. Water is the principal tool of placer mining

but too much of it at the wrong time was bad news to river miners.

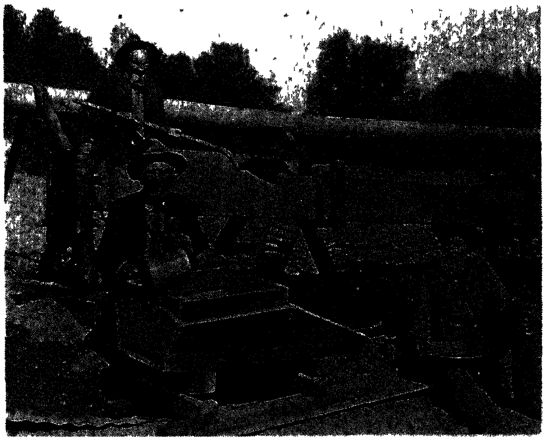
Drift mining for gold is peculiarly a California development, brought about by lava flows covering ancient gravel beds. Under table mountains, such as those in Butte and Tuolumne counties, streaks of gold, and often nuggets of good size, are found. This form of mining was an early development, and there are still mines of this type which it pays to operate. Many of the beautiful specimen nuggets to be seen today are from drift mines.

Space does not permit many statistical references, but it would seem desirable to trace briefly the production of gold in California. Most authorities agree that a high point was reached in the years 1853 and 1854, with most of the production in placer gold. Published figures range from about 62.5 to 69.5 millions of dollars annual value. From then until the new century the trend was downward. The value of placer gold reported in *Minerals Yearbook* for 1903 is $4,052,761. At that time dredging was beginning in California, and low-grade deposits, formerly too nearly worthless for even a Chinese miner to work, could be mined profitably. Large volumes of gravel could be dug and water in the dredge pond used over and over. With dredging methods improving, the placer gold industry of California contributed to greater production, and from 1903 to 1908 production about doubled in value. From 1909 to 1920 the annual value was in the 8–9 millions bracket. The average value per cubic yard of gravel for a twenty-year period—1920–40—was about 10 cents. Prior to 1920 the average value may have been much higher—say, 25–35 cents in certain rich areas.

Totals then fell off again until the new price for gold made marginal dredging properties worth mining and brought on a search in the foothills for small placers that the forty-niners, and later the Chinese and others, had overlooked. Placer production climbed again to a high in 1941 of $25,130,455, with dredges credited with over $22,500,000 of that total. High costs, because of war demands for labor, and shortages of material, forced the dredge operators to curtail or to shut down. Finally, in 1942 WPB Order L-208 fell like an axe, and California placer gold miners virtually were through. Since the war, of course, inflation has automatically lowered the purchasing power of gold and, combined with high operating costs, has made full recovery impossible. The year 1948 should show a total production worth about $15,000,000 in inflated currency, not comparable to the $15,000,000- to $20,000,000-years that were common during the 1870–90 period, when the whole family dined out in style at today's price of hamburger for four.

As one result of the new gold price, a certain form of placer mining was revived—dragline dredging. Steam-operated dragline outfits had been tried in earlier days, and one at least was used in Siberia prior to 1912. The advantages of using Diesel-powered draglines soon became apparent. In 1933 Harry England built near Oroville a floating washing plant, similar in operation to a small dredge, without a bucket line. He used a dragline on the bank ahead of the washing plant to excavate and feed gravel to a large hopper on the washing plant. Since then many similar plants have been built, with great improvements. Many small and relatively rich properties were mined in this manner during the succeeding ten years. More than 100 draglines were reported to have been used in California, with a total annual production going over $7,000,000 in 1940 and to nearly $8,000,000 in 1941. Production then tapered off as fast as it had climbed, and at last the industry felt the effect of L-208, plus high costs. Since the war a few such plants have started work again.

Bucket-line dredging in the state has been resumed on a greater scale than any other type of gold mining. Despite high costs, experienced operators have been able to keep going by taking advantage of every opportunity to improve methods. One department of the dredging industry, particularly, has received much attention. Jigs were used as early as 1914 on dredges, but only in recent years have they been given the thought and attention due them as gold savers; most modern California dredges now use jigs. Neill jigs were tried on California dredges in the early years, and at least one dredge today still uses them. Others are using Bendelari, Pan-American, and Yuba jigs.

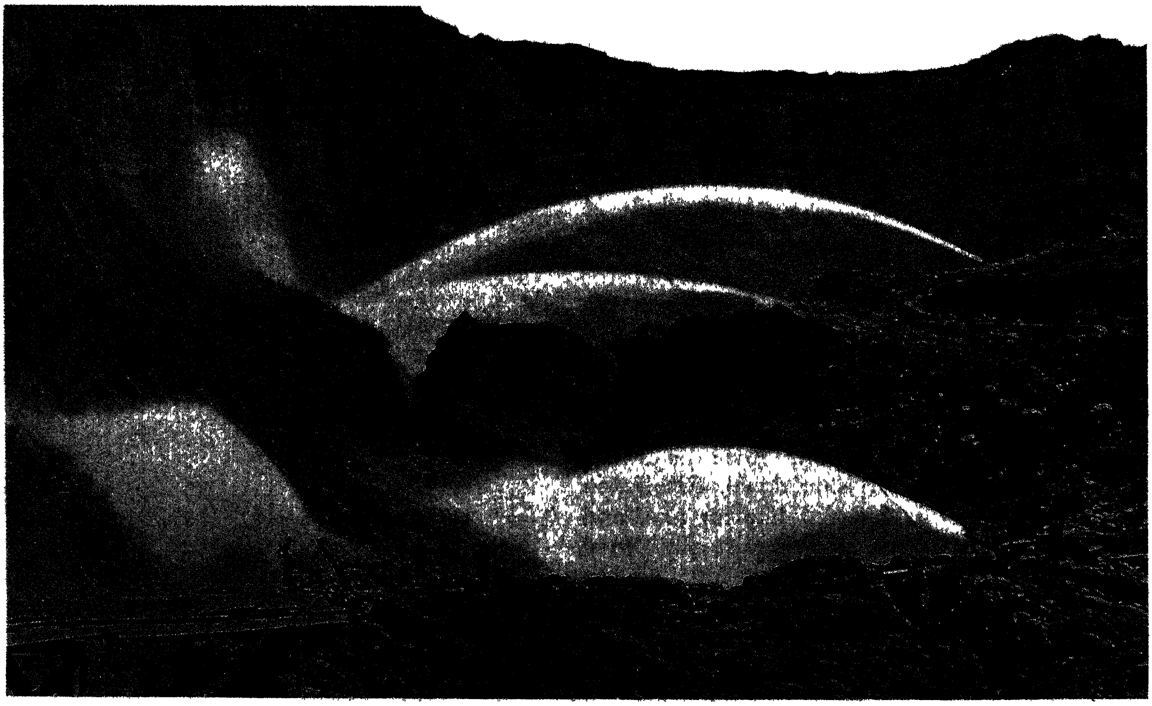


Typical rocker used by the forty-niners. (Photo by courtesy of V. Covert Martin.)

First successful dredge in California. Built in 1898 by the Risdon Iron Works, it was used near Oroville.

Amalgamators and other mechanical devices are required on jig-equipped dredges, and extra men to operate them.

Lessons learned in California have been useful in dredging fields in other parts of the world. Space will not permit a detailed account of the



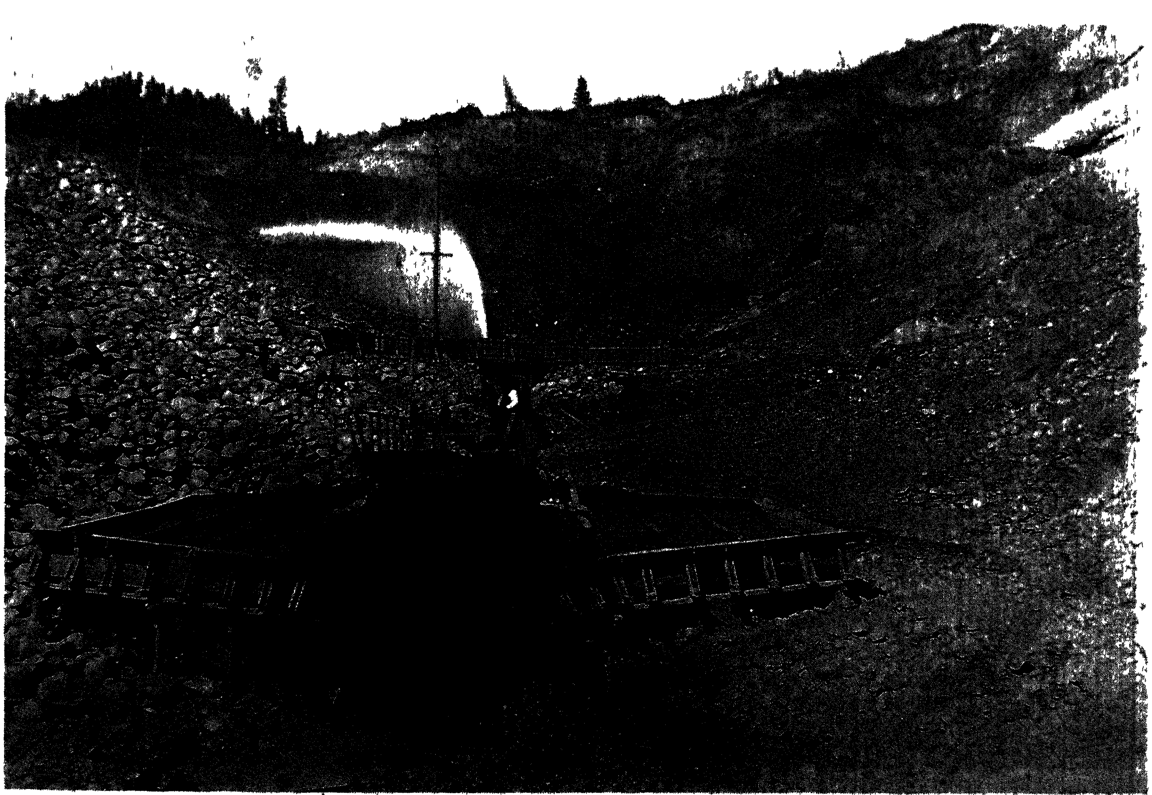
Malikoff diggings, Nevada County, California, a large hydraulic operation. (Photo by courtesy of V. Covert Martin.)

many early operations, but by 1913 bucket sizes had reached 13.5 cubic feet capacity on Yuba River dredges, operated by Yuba Consolidated Gold Fields. Later these were increased in capacity to 15 cubic feet and then to 18 cubic feet each. Digging depths of 60 feet were once thought to be maximum, but now two dredges in California dig 112 feet and 124 feet below water level and against banks 50 feet high if necessary. These dredges have monitors forward and high-pressure pumps to supply water needed for sluicing down high banks or old rock, in the latter case when working through old tailing piles.

Problems of a technical nature have been a common occurrence ever since gold dredging, as we know it in California, Alaska, and other Western areas, was first attempted. The first dredge in California, called the "Phoenix," appeared on the Yuba River in 1850. Newspaper files still available tell of it as a new venture and report it a failure for mechanical reasons One reporter stated that it was more successful in getting money from shareholders' pockets than in getting gold from the Yuba. The year 1948 marked the golden anniversary of successful dredging in California—fifty years of producing gold worth about $500,000,000 at today's price of $35. Half a billion dollars is small change to some of our economists, but half a billion in gold is a stable, nondestructible commodity.

A striking comparison between California gold placers of 1848 and the same area today can be found in a report entitled *Small-Scale Placer Mines as a Source of Gold, Employment and Livelihood in 1935*. This is a report by the Works Progress Administration covering the attempts made by some 28,000 or more persons to mine gold in many of the same California creeks and rivers where the pioneers worked. The forty-niners found gold from the grass roots down in earth that ran many thousands of dollars per acre. The 1935 miners, as unskilled at first as their predecessors, and obviously more in need than the pioneers, found hardly enough gold to buy beans. Gross yearly earnings are given in the preface of the report as $72 per man. California Division of Mines Bulletin No. 135 states that, although no actual count was made, 100,000 persons would be a conservative estimate of the number of amateur miners in 1933 and an equal number in 1934. Small-scale miners in 1937 sold about $44 worth of gold per man to the United States Mint.

The number of persons working the streams in depression years was comparable with the numbers

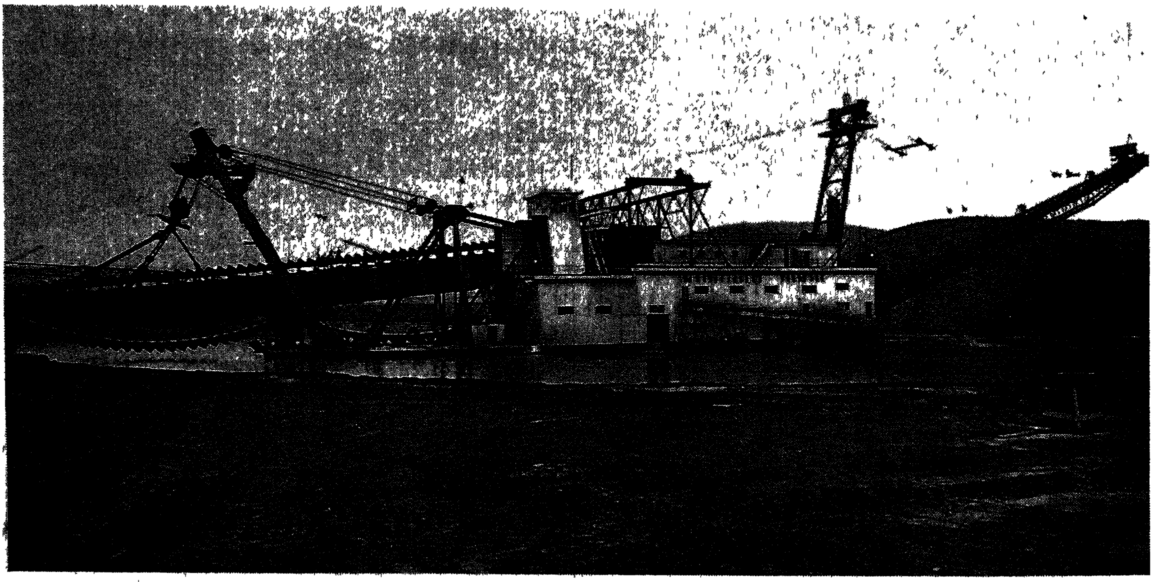


Hydraulic gold mine near Weaverville. Sluices in front show the gold being recovered from the muddy water.

Risdon dredge at Weaverville about 1901.

in 1848 and 1849. Their pitifully small returns, however, tell a story; the vast riches of California gold placers have been added during the years past to the country's store of wealth. It now is gold bullion and counted as an ever-ready gold reserve to back our currency and to inspire confidence in it.

Before building a sawmill at Coloma on the American River, John Sutter and his associates had surveyed prospects for such a mill at Cherokee Flat on the Feather River and at a spot on Butte Creek. A millrace in either place probably would have caught placer gold, since both the Feather River and Butte Creek have had a placer gold history for 100 years, as has the American River where Marshall first found a gold nugget. All are today sources of placer gold, and dredges are working in and near these streams as these words are being written.



Modern dredge used by Yuba Consolidated Gold Fields digs 124 feet below water level, against a bank 50 feet high if necessary, and uses buckets of 18 cubic feet capacity. Digging ladder is shown out of water for inspection.

SCIENCE ON THE MARCH

THE SIXTEENTH INTERNATIONAL GEOGRAPHICAL CONGRESS

THE XVI International Geographical Congress met in Lisbon, Portugal, April 8–15, 1949. In this first such meeting since that at Amsterdam in 1938, geographers from 29 countries presented many professional papers and discussed some of them The International Geographical Union revised its statutes, admitted 4 new countries, manifested an intention to participate in the solution of world problems, and accepted the invitation to hold the XVII International Geographical Congress in the United States in 1952.

Visitors were delighted with all that they saw of Portugal—a land with Mediterranean climate, not rich in an agricultural sense but intensively cultivated. Being dispersed among many Lisbon hotels, the delegates soon came to know the capital city, to admire its cleanliness and order, and to appreciate the universal sense of beauty revealed in the tiled walls of the houses and the profusion of flowers. Modest taxi rates facilitated movement everywhere.

Representation at the Congress and membership in the I.G.U. There were 330 registered individuals (the majority of whom were professional geographers) in actual attendance, including about 140 Portugese. There were 38 from the United States, including 7 with "official" credentials from the National Research Council. The total number registered was 706, of which 652 were in advance, including 43 subscribing institutions.

The composite list of 43 countries in Table 1 includes all represented in the published list of advance registrants (individuals and institutions) and the 30 countries that are now members of the International Geographical Union (in capital letters). Four of the latter (China, Hungary, India, and Turkey) were admitted at the closing session of the General Assembly.

Sixteen European countries were represented at the Congress. The only country behind the Iron Curtain actually represented was Poland—by a Polish woman geographer resident in Great Britain. From south of the equator the only countries represented were Argentina, Brazil, Peru, and Uruguay—all in South America. From the Americas, in addition to the 4 south of the equator, there were Canada, Colombia, Cuba, the United States, and Venezuela. From Asia, only Ceylon, China, and Turkey were represented; from Africa, only Egypt.

TABLE 1

un.	ARGENTINA	—	Lebanon
—	Australia	—	MOROCCO
un.	Austria	*of*	NETHERLANDS
of.	BELGIUM	—	NEW ZEALAND
of.	BRAZIL	*of*	NORWAY
—	BULGARIA	—	Palestine
of.	CANADA	*of.*	Peru
un.	Ceylon	*un*	POLAND
—	CHILE	*of.*	PORTUGAL
of.	CHINA	—	RUMANIA
un.	Colombia	*of.*	SPAIN
of.	CUBA	*of.*	SWEDEN
—	Czechoslovakia	*of.*	SWITZERLAND
of.	DENMARK	*un.*	TURKEY
of.	EGYPT	—	UNION OF SOUTH AFRICA
un.	Finland		
of.	FRANCE	*of.*	UNITED STATES OF AMERICA
of.	GREAT BRITAIN		
of.	GREECE	*un.*	Uruguay
—	HUNGARY	*of*	Vatican
—	INDIA	*of.*	Venezuela
—	Iran	—	YUGOSLAVIA
of.	ITALY		

of. = represented by a delegation accredited as "official" (20, including Portugal and the Vatican); *un.* = represented by an unofficial delegation (8 countries); — = not represented at the Congress (14 countries, of which 9 are members of the I.G.U.; 6 countries were represented in the published advance registration, viz., Australia, Czechoslovakia, Iran, Lebanon, Palestine, and the Union of South Africa—the latter being a member of the I.G U.).

Languages. German, English, Spanish, French, Italian, and Portuguese were recognized for use in the Congress. French and English were spoken almost exclusively, French predominately. Translation services were, on the whole, inadequate. Simultaneous translation, with earphones for the listeners, is clearly the most satisfactory for such meetings. Linguistic competence in both French and English on the part of delegates from many countries was marked, although linguistic laziness of delegates from the United States was unfortunate.

The clarity of the French lectures by Professor Ribeiro, on the excursions which he led during the Congress (to the Arrábida on Sunday, on the Tagus on Tuesday, and in the city of Lisbon on Wednesday), was remarked and appreciated by many.

I. THE CONGRESS

1. *Formal opening, closing, and plenary sessions.* The formal sessions, as usual, were well planned

and impressive. At the opening session, held in the Palace of the National Assembly on April 8, at 3:00 P.M., the President of the Republic of Portugal, General Carmona, presided in a scene of military display with appropriate music, and in the presence of part of the diplomatic corps. Professor Amorim Ferreira, chairman of the organizing committee of the Congress, gave the address of welcome, weaving Portuguese colonial history and geography into the background. Professor Emmanuel de Martonne, of France, president of the International Geographical Union, responded for the guests and recounted the history of keeping the union alive since the meeting in 1938.

Professor Boerman, of the Netherlands, and Professor George B. Cressey, of the United States, vice-presidents of the I.G.U., spoke briefly, the latter correlating the gathering of geographers with world problems and professional obligations.

At the first general assembly of the I.G.U., on the afternoon of April 9, President de Martonne outlined in general terms the problems with which the Executive Committee had been dealing, upon which action might be feasible by the time of the second general assembly on April 15. (The actions taken at the second general assembly of the I.G.U., on April 15, are separately dealt with in Part II of this report, relating to the International Geographical Union.)

The closing session of the Congress was fittingly held at the Lisbon Geographical Society, which was in the setting of a large museum reminiscent of the great days of Portuguese exploration and colonial expansion. Professor de Martonne presided and graciously expressed the thanks of all the delegates for the hospitality and cordiality of Portugal and its people. Professor Orlando, as secretary of the organizing committee, related some of the problems that had been faced and met in planning and arranging for the Congress.

Professor Cressey, as the new president-elect, expressed sincere appreciation of the delight of many in the charm of Lisbon and Portugal and of the arrangements for the Congress and the excursions. He remarked that the I.G.U. "belongs to the geographers of the world" and expressed the hope that applications for membership may be received from countries other than the present 30 members. He noted the expanding interest typified in the fact that the I.G.U. now has a vice-president in South America and another in Asia. He invited suggestions from geographers everywhere, and added that it is hoped to inaugurate a news letter which may be widely distributed to geographers throughout the world.

2. *Section meetings.* Except for the general sessions at the beginning and end of the Congress, already described, all the meetings were those of the following sections:

I. *Cartography.* (President, J. K Wright; secretary, B. W. Adkinson; both U S A)
II. *Physical Geography.* (Presidents, A. G. Ogilvie, Great Britain, and Niels Nielsen, Denmark, vice-presidents, F. Hernandez-Pacheco, Spain, and P. Birot, France; secretary, David L. Linton, Great Britain.)
III *Biogeography.* (President, H. Gaussen, France; vice-president, P. Dansereau, Canada; secretary, D. Duarte de Castro, Portugal)
IV. *Human and Economic Geography.* (Presidents, Dudley Stamp, Great Britain, and Max Sorre, France; vice-presidents, O. Tulippe, Belgium, and Amorim Girão, Portugal)
V. *Geography of Colonization.* (President, Ch. Robequain, France; secretary, Harrison Church, Great Britain.)
VI. *Historical Geography and the History of Geography.* (Presidents, R. Almagià, Italy, and Joaquim Bensaude, Portugal; vice-president, Damião Peres, Portugal; secretary, Angela Codazzi, Italy.)
VII. *Methodology, Teaching and Bibliography.* (Presidents, Christovam Leite de Castro, Brazil, and Ch. Burky, Switzerland; secretary, Alice Garnett, Great Britain)

From 3 to 6 questions in the field of each of the 7 projected sections, totaling 30 questions, were circulated in advance in order to stimulate and channelize the preparation of papers to be presented at the Congress. The responses were quite uneven. Sections I, II, and IV were especially well attended; some of the others had few participants.

The sections are not prescribed by the I.G.U. statutes but are planned by those organizing a congress. The experience of the Lisbon Congress in sectional organization will be useful in making plans for the Congress in 1952. Only a few comments on the section meetings can be included in this report.

In Section I, 37 communications were received, 4 not read; 23 were reports on cartographic work or projects in the countries (11) of those who read papers. The section voted to support the following propositions:

1) That a Commission be appointed to review the International Map of the World at 1:1,000,000 scale with a view to its use as a base map for all geographical purposes, and to give special attention to the production of an outline edition, or editions, to serve as a base map on which ecological and statistical data of geographic value may be overprinted.

2) That a Commission be appointed to study and encourage the production of a population map of the world at a scale of 1:1,000,000.

In Section II, 28 papers were read. There was

vigorous discussion of geomorphological questions by eminent specialists, with "models of clear thinking and presentation." Recommendations as to topics to be discussed at later congresses were formulated.

In Section V (colonization), 15 communications were received: 8 on black Africa, 1 on North Africa, 4 on South America, 2 on colonial territories in general. It was agreed that the subject of colonization is too large and vague for a section, and it was suggested that it be replaced at the next Congress by a section on the human geography of tropical (or desert) countries or areas.

3 *Field excursions*. The field trips or geographical excursions, one preceding the Congress and short ones during the Congress, were exceptionally well planned and managed—routes, timing, lectures, meals, and lodging. Those following the Congress lived up to the same high standards.

The excursions preceding and following the congress were:

A) Minho, Tras-os-Montes, and the Valley of the Douro (7 days). Leaders: A. Jorge Dias and Carlos Teixeira.
B) The central littoral and the calcareous massif of Estremadura (6 days). Leader: Fernandes Martins, of Coimbra.
C) Central Portugal (7 days). Leader: Orlando Ribeiro.
D) Estremadura and Ribatejo (6 days). Leaders: Mlle. Virginia Rau and Georges Zbyszewski.
E) Lower Alentajo and Algarve (7 days). Leader: Mariano Feio.
—) Madeira (April 23–May 8 or 9, following all other excursions). Leader: Orlando Ribeiro.

The 31 who went on Excursion *D*, 16 from the United States (the only excursion preceding the Congress), were enthusiastic about the glimpses they got into the life of the people and their economy—fishing, fruit growing, the wine industry, and ceramic production. Admiration of the charming leadership of Dr. Virginia Rau and of her linguistic versatility was universal.

The two all-day excursions of Sunday, April 10 (during the Congress), should be mentioned: These were the Arrábida; and Sintra, Cascais, and Estoril. Half-day trips during the Congress included a boat trip on the Tagus to its mouth and return, by courtesy of the administrative council of the port of Lisbon; and a visit to outstanding points in the city of Lisbon.

4. *Cartographic exhibits*. Several notable cartographic exhibits were on display in the Superior Technical Institute, in which all the section meetings were held. Canada, France, Italy, Portugal, Switzerland, Turkey, the U.S.A., Venezuela, and the Vatican were represented. That of the United States was the largest and was well organized; having been originally shown in South America, the explanatory notes were in both Spanish and English. Deterred by an anticipated exhibit fee that did not materialize, the British exhibit was not shipped to Lisbon

II. THE INTERNATIONAL GEOGRAPHICAL UNION

The revision of the statutes, the establishment of a new scale of contributions by member countries, the discontinuance of certain commissions and the creation of others, the decision to hold the next Congress in the United States in 1952, and the election of new officers of the Union, were among the most significant acts taken at the Lisbon meeting.

1. *Revision of the Statutes of the I.G.U.* At the closing general assembly, April 15, the statutes were revised, in general accord with recommendations of the Executive Committee. The principal changes made (the head of each official delegation casting one vote, in most instances) are summarized below:

1) The number of vice-presidents was increased from 6 to 7, elected by the General Assembly; one of them is named by the Executive Committee as first vice-president, available to replace the president if necessary.

2) The Executive Committee is to cooperate in the organization of international congresses.

3) Each Commission will be composed of a maximum of 6 members and a limited number of corresponding members (partly in order to enable regular commission members to meet, within budgetary limitations).

4) On scientific matters, each individual member registered and present at a congress will have the right to vote. On administrative questions the vote will be by countries, each country having one vote.

5) The basis of annual dues was changed. Countries adhering to the Union will be divided into eight categories. Each adhering country pays annually a number of units of subscription corresponding to its category, as follows:

Category	I	II	III	IV	V	VI	VII	VIII
No. contribution units	1	2	3	5	7	9	12	15

Each adhering country will specify the category it desires, which the Executive Committee may reject if judged manifestly inadequate. The value of the unit is to be fixed by each Congress subject to emergency alteration by the Executive Committee.

2. *Other actions of the closing General Assembly.*

1) The *unit of contributions* for the period 1950–52 was fixed at $100 U.S. currency. The

contribution of the United States will therefore be $1,500 per annum for the next three years

2) The following *Commissions* and their chairmen were agreed upon:

1. Study of Population. C. B. Fawcett (Great Britain).
2. Industrial Ports. W. E Boerman (Netherlands).
3. Bibliography of Ancient Maps. Roberto Almagià (Italy).
4. Geographic Utilization of Aerial Photography M. Barrere (France).
5. International Map of the World, 1/M. J. K. Wright (U.S.A.).
6. Agrarian Geography Daniel Faucher (France).
7. Medical Geography. Jacques May (U S.A.).
8. Inventory of Land Use. Samuel Van Valkenburg (U.S.A.).
9. Regional Planning. Jean Gottmann (France)
10. Periglacial Morphology. Hans Ahlman (Sweden).
11. Soil Erosion Edward Ackerman (U.S.A.).
12. Pliocene and Pleistocene Terraces. H. Baulig (France).
13. Tertiary Peneplains. E. de Martonne (France).

3) *Seat of the XVII International Geographical Congress, 1952.* The invitation to hold the next succeeding Congress in Vienna posed a delicate question. The chairman announced that, as Austria is not yet a member of the I.G.U., the invitation was out of order.

Brazil invited the next Congress to meet there; but after due consideration and receipt of further instructions the Brazilian delegation withdrew the invitation for 1952 and cordially supported the invitation of the United States, intimating that they may renew the invitation to hold the Congress next succeeding in Brazil.

Dr. Cressey presented the invitation of the United States in the name of the National Research Council, supported by letters from five geographical associations and societies. He added that the United States has not had the opportunity to serve as host to an international geographical congress since 1904, that 1952 will mark the centenary of the founding of the American Geographical Society of New York, and nearly a half century of the Association of American Geographers. His remark that we shall do our best to assist those who have dollar problems was greeted with applause. The invitation of the United States was accepted, and it is expected that the congress will be held in the early part of the summer of 1952, beginning in New York.

4) *Applications* of China, India, Hungary, and Turkey for membership in the I.G.U. were all acted upon affirmatively. Representatives of China and Turkey were present and spoke.

5) *Officers of the I G.U.* Professor de Martonne, retiring president, was elected honorary president for life—instead of becoming one of the vice-presidents—in gracious recognition of his service as president since 1938. Mlle. Lefèvre, who has been serving as secretary since 1938, was elected vice-president and was named by the Executive Committee as first vice-president.

The following members of the Executive Committee were elected, to hold office until the end of the XVII congress:

President ...	Prof. George B Cressey (U.S A)
1st Vice-President	Prof. M. A. Lefèvre (Belgium)
Vice-Presidents	Prof. R. Almagià (Italy)
	Prof. Hans Boesch (Switzerland)
	Prof. G. H. Kuriyan (India)
	Dr. Christovam Leite de Castro (Brazil)
	Prof. Orlando Ribeiro (Portugal)
	Prof. L. Dudley Stamp (Great Britain)
General Secretary	Prof. George H T. Kimble (Canada)

3. *Service to the United Nations and specialized agencies.* The following resolution was drafted during the Congress, and approved by the old and new Executive Committees in the desire to bring the I.G.U. into effective contact with the United Nations and the specialized agencies.

Whereas the United Nations and all of the specialized agencies are confronted by problems of unprecedented complexity in which the differences from region to region are of great significance and subject to change; and

Whereas geographers make intensive studies of regional distributions of physical, biological, and social phenomena and are constantly concerned with both the persistent and the changing patterns of these phenomena and are endeavoring to improve their techniques of analysis, interpretation, and cartographic presentation—all of which may be of very great value to the United Nations and the specialized agencies;

Now, therefore, be it resolved that the International Geographical Union tender its services to, and record its intention to cooperate with, the United Nations and the specialized agencies, in particular by its practice of creating commissions to investigate and report on such problems as are indicated above.

The general secretary was instructed to communicate the resolution to the Secretary General of the United Nations and to appropriate officers of all the specialized agencies (UNESCO, ICAO, FAO, WHO, etc).

S Whittemore Boggs

Department of State
Washington, D. C.

BOOK REVIEWS

THE MECHANISM OF ESCAPE

Flight from Reality. Norman Taylor. 237 pp. Illus. $3.50. Duell, Sloan & Pearce. New York.

An Outline of Psychoanalysis. Sigmund Freud. 127 pp. $2.00. Norton. New York.

TAYLOR'S *Flight from Reality* recounts what man has done to escape reality, and Freud's *An Outline of Psychoanalysis* (now available in an English translation) attempts to explain why he needs to escape. Taylor deals with what man does, Freud with why he does it, and both have a way of making their stories dramatic.

In the case of Taylor's material this does no harm, presents no distortions, leads to no unwarranted assumptions of overextended generalizations; in the case of Freud the story is based more on theory than on fact, and the inferences, according to the yardstick of scientific method, need further documentation. In Taylor, capital and dividend are in proper relation to each other; in Freud, dividends declared are in excess of available capital.

Taylor's book explores the properties of juices, flowers, seeds, fruits, barks, leaves, and roots in which men, from Cro-Magnon to John Doe, have sought momentary respite from the impact of reality—the reality of self, of other people, or of the inanimate world. As Taylor points out, this quest has "dominated more people than any religion, costs more than any food, and it yields in this country more than two billion dollars' yearly revenue to the United States Treasury." These substances are used by all strata of society, or cultures, from the civilized Man of Distinction—who obtains it from laboratory or distillery or tobacco warehouse—to the Malayan betel-chewer—who obtains his product from nature in the raw. From the exotic narcotics (some of which have their own gods), such as the ololiuqui and peyotl of Mexico, the pituri plant of Australia, the mandrake of Eurasia, on through Indian hemp, marijuana, heroin, morphine, to alcohol, tobacco, and coffee, the story reflects man's need for, and struggle with, these instruments of flight. The use and abuse of such products, as told by Taylor, makes fascinating reading.

And now the question of "Why?" To this difficult question many varied answers have been given—answers which lie, of course, in the realm of inference and involve a jump from what is observed to what is believed to be "behind" the observed. One such answer is that put forward by Freud in his system *An Outline of Psychoanalysis.* This system probes the Why? of human behavior from a broader point of view than the mere attempt of man to escape reality, but the Freudian emphasis on the Pleasure Principle and the Reality Principle are directly related to this question.

This last book of Freud's, written in London in 1938 and now translated by James Strachey, is quite literally an outline of the fundamental tenets of psychoanalysis. "It is the aim of this brief work," states Freud, "to bring together the doctrines of psychoanalysis and to state them, as it were, dogmatically—in the most concise form and in the most positive terms."

The question has been raised before, but it is worth raising again in the present context: Is it possible that the Freudian system as a system may one day be cited as another example of man's myth-making behavior through which he attempts a flight from reality? Only time, and more psychological evidence about man, can provide the answer.

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BROOD PARASITISM

The Parasitic Cuckoos of Africa. Herbert Friedmann. xii + 204 pp. Illus. $4.50. Washington Academy of Sciences. Washington, D. C.

BROOD parasitism, a species laying in another's nest and leaving the young to the care of the host, has evolved in a number of groups of birds. Much of the study on this has been done on the common cuckoo, *Cuculus canorus,* of Europe, but Dr. Friedmann has also given us a classical study on the parasitic cowbirds of the Americas. In 1924–25 he traveled in Africa to study the parasitic cuckoos, weaver birds, and honey guides there. Since then, from the literature and through correspondence, he has added to his data and analyzed and evaluated them in the light of his long-time interest in the subject. The time is not yet for a comparative monograph on the cuckoos; the Oriental and Australian species are little known. The African data, too, is incomplete, as Dr. Friedmann says, but the wonder is he was able to get so much. This fact-packed, and thought-provoking contribution will enlarge our concepts of the ramifications of brood parasitism.

In Africa certain species of cuckoos tend to select certain hosts—crows, shrikes, babblers, weaver birds, or sunbirds; apparently this reduces competition between species. Individuals of certain species may be host-specific, as when the territory of one cuckoo is a single tree in which is a nesting colony of weaver birds. In such a case the male seems most faithful to the territory and thus may influence the host-speci-

ficity. Resemblance of the parasitic egg to that of the host has, Friedmann thinks, had too much read into it as "perfection." Even if "perfected," is it for the good of the species? The cowbirds, more generalized in behavior, seem more successful. Some of the African cuckoos, laying several eggs a season, are more prolific than their hosts. Males of some species feed fledgling young. This is interpreted as an error in sex identfication, being in reality mistaken courtship feeding of a passive bird by a dominant one.

Geographical variation in color phases, and in non-morphological characters such as migration, breeding season, habitat, host species selected, and voice are recorded as probably the early stages of further evolution. One species, *Cuculus clamosus,* is said to have two ecological subspecies (p. 83). This would be unique in birds, and can be interpreted as two species.

Puzzling taxonomic points are discussed, and descriptions of the fifteen species and their races given.

A. L. RAND

Chicago Natural History Museum

YOUNG MAN WITH A DREAM

The Story of Television: The Life of Philo T. Farnsworth. George Everson. 266 pp. Illus. $3.75. Norton. New York.

GEORGE EVERSON may not know it, but this fine biography he has written on Philo T. Farnsworth qualifies him, I believe, as not only Farnsworth's Boswell but television's as well.

I think the chief virtue of the book is its tremendous worth to the layman, and by that I mean not only those who buy television receivers and bring the magic of video into their own homes, but the cameramen, technicians, actors, stagehands, and program managers all over the country. A review of this great young man's achievements cannot help but give them a new sense of the colossal effort that went into creating their jobs in this new medium of communication.

The book should be prescribed reading for the new owner of a television set, because many of our neophyte viewers are prone to be hypercritical of the programs being telecast in these early years of commercial television. They would thus be reminded of the years of painstaking trial and error that made possible this modern miracle. They would do well, also, to dwell upon the splendidly told story of how Farnsworth had to struggle against almost insurmountable odds to gain the financial backing he needed to develop his inventions. I think Everson did well to avoid romancing this phase of the story. As he tells it, the shrewd bankers who aided Farnsworth were influenced by the possibility of profits far more than the altruistic urge to foster the efforts of a young inventor. This is stark realism and, it seems to me, a valuable object lesson to any young man with an idea.

Since Farnsworth and Zworykin are undoubtedly the fathers of modern television, this fascinating biography—into which the story of television has been so well integrated—is MUST reading. Indeed, I have recommended it as such at ABC.

PAUL B. MOWREY

American Broadcasting Company
New York

MODERN PHYSICS FOR THE LAYMAN

The Universe and Dr. Einstein. Lincoln Barnett. 127 pp. Illus. $2.50. Sloane Associates. New York.

MY FIRST impression of this book was unfavorable. Its title seemed flippant. There is no table of contents, and the numerous short chapters have no titles; hence, to review it, I might have to read it through. But as I read along my dislike vanished.

The book is not flippant or facetious. It is characterized by a sustained effort to instruct the reader rather than to entertain him by outlandish paradoxes or dramatic assertions. Addressed to the thoughtful layman, it is a straightforward attempt to explain the meaning and the value of several of the great generalizations of modern physics. In this difficult task the author has had the help and advice of several top-flight American scientists. Emphasis throughout is on essentials, but good continuity and an admirable literary style make the book pleasant to read.

Modern theories of atomic structure and the dualism of light are discussed briefly. "The basic units of matter gradually shed their substance. The old-fashioned spherical electron was reduced to an undulating charge of electrical energy, the atom to a system of superimposed waves." Thus waves and particles coalesce in "a universe of wavicles."

The various kinds of relativity are treated at somewhat greater length. *Newtonian* relativity, according to which many laws of physics can be stated without assuming an absolute frame of reference, seemed adequate until the famous Michelson-Morley experiment to test the orbital motion of the earth showed where the merry-go-round broke down. Emergency repairs by Lorentz and Einstein are part of the *special* theory of relativity, which now includes the revolutionary concept of the interchangeability of mass and energy. The *general* theory of relativity substitutes a geometrical hypothesis of gravitation for action at a distance.

Dr. Einstein in a foreword says, "The main ideas of the theory of relativity are extremely well presented. Moreover the present state of our knowledge in physics is aptly characterized."

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HISTORY IS BUT BIOGRAPHY

Discoverers for Medicine. William H. Woglom. 229 pp. Illus. $3.75. Yale Univ. Press. New Haven.

UNDERSTANDING of the present is in part predicated upon comprehension of the past. The historical foundations of modern medical knowledge will be belittled only by those whose conceit demands that they be classed as truly original. Concepts evolve slowly and painfully, with many false steps and fruitless digressions into blind alleys. Much of what we "know" today will be discarded as useless, false wishful thinking tomorrow. Historical perspective makes us humble and proud simultaneously; it teaches us respect for the patient efforts of man and for that profoundly significant, though typically simian, characteristic of all discoverers: curiosity.

History may be approached and recorded as a series of events, as a development of ideas and concepts, or as a story of men. In *Discoverers for Medicine,* Dr. Woglom has combined two of these approaches in a series of fourteen delightful biographic sketches describing both the personalities involved and the genesis of concepts. Included are Stephen Hales, the first man to measure directly the arterial pressure; William Withering, great enough to listen to old wive's tales concerning the value of foxglove (digitalis) in cardiac dropsy; Eli Metchnikoff, whose studies of phagocytosis revolutionized our ideas of immunity and defense mechanisms; Benjamin Franklin exercising his burning curiosity upon the question of eyeglasses; Wilhelm Roentgen, discoverer of X-rays. Many of these discoverers for medicine were not physicians, but gifted and sincere amateurs. Hales was a clergyman, Mendel a monk. These personalities come alive once more through the facile pen and deep humanism of the author. The book is highly recommended for leisure reading and a quick bird's-eye view of some of the road over which medical science has traveled so laboriously.

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THE NAZI MEDICAL CRIMES

Doctors of Infamy. A. Mitscherlich and F. Mielke. xxxix + 172 pp. $3.00. Schuman. New York.

THIS thin volume recounts factually a small segment of the monstrous crime against humanity called Nazism. It was written by Germans for German consumption. The depths of depravity to which the proud profession of medicine sank under conditions nurtured in Germany, a country that prided itself on the development of civilization, are still unbelievable—not that there is the slightest doubt concerning the facts but because logic is inadequate to account for the motivations. The thoughtful general comments of Drs. Ivy and Alexander, appended to the translation, indicate their concern and, upon analysis, their inability to penetrate this crucial question of motivation. Dr. Ivy's explanation that "a true scientist must be a moral and an honest man" is an appealing one. Unfortunately, more precise definitions of truth, morality, and honesty at present do not seem to coincide among all peoples or their leaders.

Four years have elapsed since the horror of the Nazi concentration camps was laid bare. Thorough Germanic documentation has established their direct connection with governmental policy and their acceptance by the population. It is tragic that there is no indication that these horrors will not recur. The revision of the Hippocratic Oath by the World Medical Association to include utmost respect for human life, and the resolution of the United Nations condemning genocide are steps in the right direction.

The book is recommended as a footnote to the fuller records of the Nuremberg trials, and to the documentary film on Buchenwald.

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BRIEFLY REVIEWED

Fontes Historiae Botanicae Rossicae. Vladimir C. Asmous. 32 pp. Illus. $1.25. Chronica Botanica. Waltham, Mass. Strechert-Hafner. New York.

ALL botanists will be grateful to have at hand a list of publications which deal in some way with the history of botany in Russia. There are both subject and author indices; the latter gives the name of the author, date of publication, the original title, and a brief summary of the subject matter the article presents. There some full-page half tones showing the plans of several botanical gardens, and a few line drawings, largely decorative in nature. At least investigators are given an opportunity, through this rather comprehensive source book, to know of the existence of, the type of, and the content of many Russian contributions to botanical literature whether or not they are able to follow up and read the complete articles in their original form.

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CORRESPONDENCE

PSEUDO-SONNETS FOR SENESCENT SCIENTISTS

I

Why must the poet always tune his lyre
To sing of lovers' griefs, of passion's glow;
Why must his song be laden with desire
Or in a minor mode be filled with woe?
Must mossy banks and softly rippling streams
Or daring deeds or melancholy death
Run full the gamut of the poets' themes
And claim the voicings of the poets' breath?
Soft moonlight filtering through the whispering leaves,
Fair swaying daffodils on river's brim,
The vines that clamber over thatched eaves,
Ice crystals sparkling on the birch tree's limbs;
The smoke that, through the frosty twilight air,
Suggests the comforts of the toiler's cot—
All these the humble poet sings with care
These are the counters of the poet's thought.

II

This luring world of transient loveliness
Awakes the poet's soul to lyric song,
But Nature in her sphinxlike quietness
Serenely weaves enchantment yet more strong
And stirs the strivings of the questioning mind.
For he who seeks the wherefore and the why,
Not satisfied with Beauty, looks behind
Its surface where still deeper meanings lie.
On him fair Nature smiles or frowns in vain,
For, manlike, quite regardless of her mood,
He knows, alike in either joy or pain,
She's always eager to be understood.
So, not beguiled by her soft blandishments,
He would disclose what Nature would conceal.
With eager questions of experiment
He seeks her cherished secrets to reveal.

III

With oh what cunning skill and patient care
He fashions scientific traps designed
To catch elusive Nature unaware
And make her captive to his ruthless mind.
With meters, balances, and microscope
He tries to pierce the center of her thought.
"By measurement to know"—this is his hope
That Truth in nets of numbers may be caught.
In terms of matter, motion, time, and space,
Electrons, atoms, molecules, and force,
He bravely seeks her tortuous ways to trace,
And mathematically to plot her course
He catalogues her uniformities
And proudly nominates them "Nature's Laws,"
Neglecting her vexatious vagaries;
He'll not allow effect without a cause.

IV

Persistently he questions, till, forsooth,
Discouraged with the never-ending quest,
He wonders: "Does she always speak the truth?
Let Nature *take* her course, I'll take a rest"
Surprised, that for the time she's not pursued,
Dame Nature drops her mask of seeming guile,
Turns to her weary son in melting mood,
Bestows the favor of her rarest smile.
"You must not take me for a simpleton
And, fretful, charge that you have been misled
I answer truly but, my foolish one,
My answers still must be interpreted.
I'm but the outer aspect of your mind.
Consider well. Though I admit 'tis true
I give half answers, yet you still must find
The other half, my dauntless son, in you."

Renewed, still eager, but a whit less bold
Humbly he feels new truths within him stir.
His colleagues smile. "He's growing old.
Our scientist has turned philosopher"

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WORLD RESOURCES

Mr. Wilhelmy's reference to the views of the soil conservationists as "less well-founded opinions" ("Correspondence," May 1949) is unfortunate. The critics of Vogt and Osborn generally overlook the fact that the much-discussed books of these authors are very well documented. Mr. Bennett, in his article on "Population and Food Supply" (January 1949), seems also to have overlooked a number of other facts.

Soil erosion, soil fertility, and efficient land utilization are primarily problems of the physical and natural sciences. With few exceptions, economists are neither well trained in these fields nor well acquainted with recent studies of the physical and biological basis of land utilization. Their judgments of the broader aspects of population and food potentials, etc., are therefore limited by economic horizons. Mr. Bennett's paper is an illustration of this fact; its economic implications are illuminating, but geographers and geologists will find some of its statements naive.

One may well question the assumption that Vogt's wide field experience and his numerous specific examples lead to "less well-founded opinions" on soil erosion than a library study of food habits. Short-run economic conditions are not necessarily a reflection of major physical trends, and it is dangerous to use purely economic data as a basis for conclusions that depend on physical facts. Mr. Wilhelmy will find ample confirmation of Vogt's and Osborn's general thesis in the reports of scientific surveys made in all parts of the world. The leading geographical journals will tell him the same story. But a few weeks in the field in, say, our own arid Southwest, might prove more enlightening than a barrel of economic statistics.

MALCOLM H. BISSELL

Calistoga, California

THE

SCIENTIFIC MONTHLY

AUGUST 1949

EVOLUTIONARY GROWTH RATES IN THE DINOSAURS*

EDWIN H. COLBERT

Dr. Colbert (Ph.D., Columbia, 1935), who is curator of fossil reptiles, amphibians, and fishes at The American Museum of Natural History, of New York City, where he has been an associate since 1930, is spending the summer doing reconnaissance in the Triassic sediments of Southwestern United States.

MOST people may not know much about dinosaurs, but in this day of widely disseminated popular publications, of moving pictures, and of radio, the public is fully aware of the fact that such animals once lived on the earth. Moreover, because of publicity given to the large dinosaurs of upper Mesozoic times, there exists a nearly universal concept that all the dinosaurs were huge beasts; that they were veritable giants in the ancient world. Indeed, the word *dinosaur* has become almost synonymous in many minds with the word *giant*.

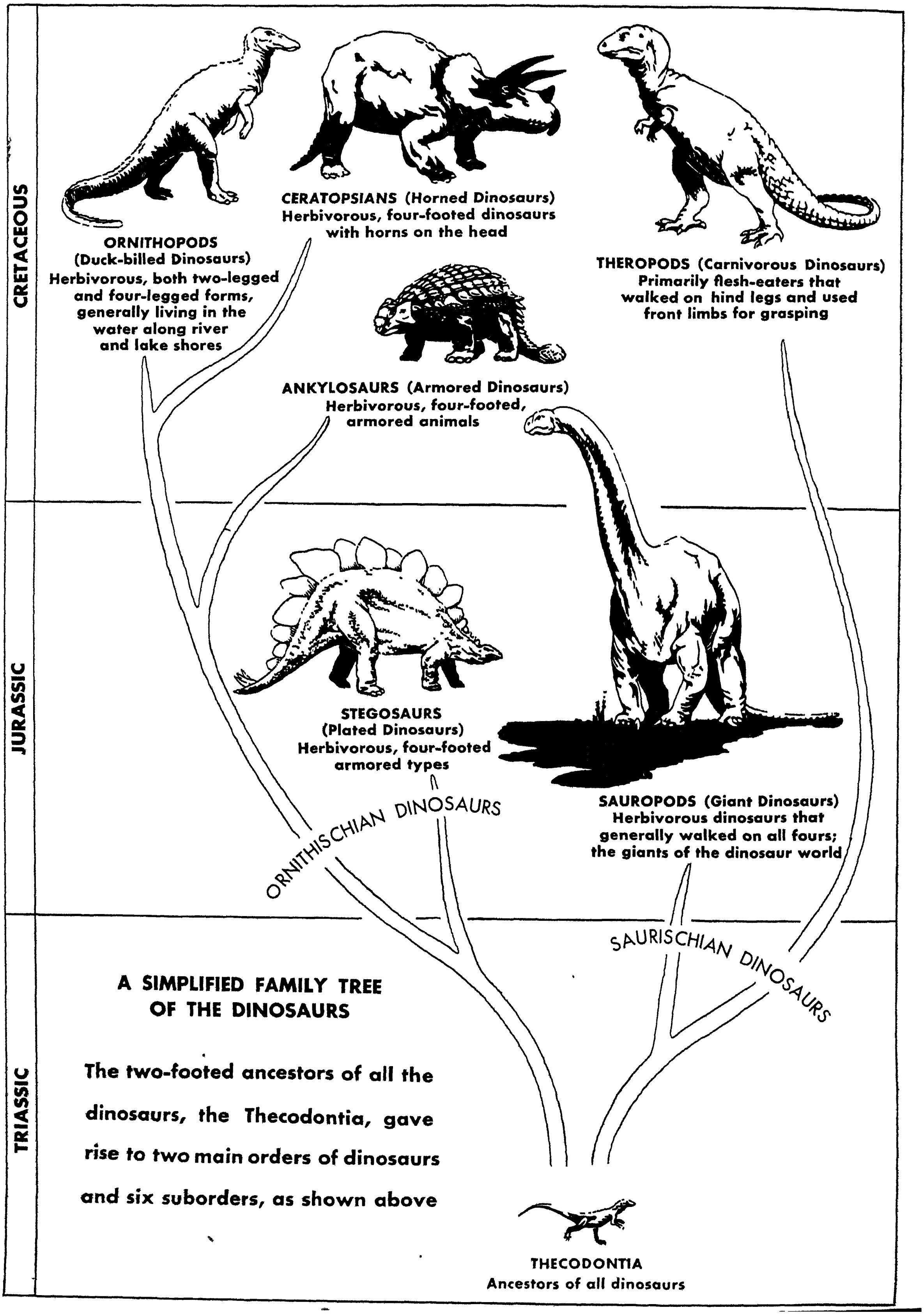
It is true that many—in fact a majority—of the dinosaurs were what we might call giants, animals 15 feet or more in length, with probable weights of several to many tons. Not all of them were large, however; some dinosaurs were of very moderate size, and some of them were quite small Yet, even though there were small and medium-sized dinosaurs, these reptiles were on the whole giant animals, because out of about 230 genera of dinosaurs known at the present time, at least 150 genera were giants. Giantism was a dominant trend in dinosaurian evolution.

Evidently large size was generally advantageous to the dinosaurs during the long course of their history, which began in the Triassic period of the Mesozoic era and extended through the Jurassic and Cretaceous periods. Many lines of dinosaurs evolved during the 100 million years or more of Mesozoic history in which they lived, and in most of these lines of ascent there was an early trend toward giantism. In those days the earth had a tropical or subtropical climate over much of its land surface, and in the widespread tropical lands there was an abundance of lush vegetation. The land was low, and there were no high mountains forming physical or climatic barriers. Conditions were therefore favorable for the evolution of large plant-eating, or herbivorous, reptiles, and of course the development of large herbivores led to the simultaneous evolution of large meat-eating, or carnivorous, reptiles.

Such trends toward large size frequently appear in certain groups of animals. We are familiar with this evolutionary phenomenon today as it is seen in the elephants. The elephants are giants, and through most of their evolutionary history they were giants. To a lesser degree the same can be said for such animals as the rhinoceros or the hippopotamus.

It is interesting to note that giantism was achieved independently by various separate lines of dinosaurian evolution. Time and again in the collective history of these reptiles a phylogenetic line had its beginnings with small animals and very quickly progressed to animals of large or even huge size. This pattern of size growth was repeated over and over at different times during middle and

*All photographs of the restorations, by Charles R. Knight, are reproduced by permission of The American Museum of Natural History. The family tree of the dinosaurs is from Dr. Colbert's *The Dinosaur Book*, published by The American Museum of Natural History.



A SIMPLIFIED FAMILY TREE OF THE DINOSAURS

The two-footed ancestors of all the dinosaurs, the Thecodontia, gave rise to two main orders of dinosaurs and six suborders, as shown above

upper Mesozoic earth history, various small dinosaurs evolved into giants during the Jurassic period, and again during the Cretaceous period. Wherever and whenever the different dinosaurs evolved, they were more likely than not to grow into giants.

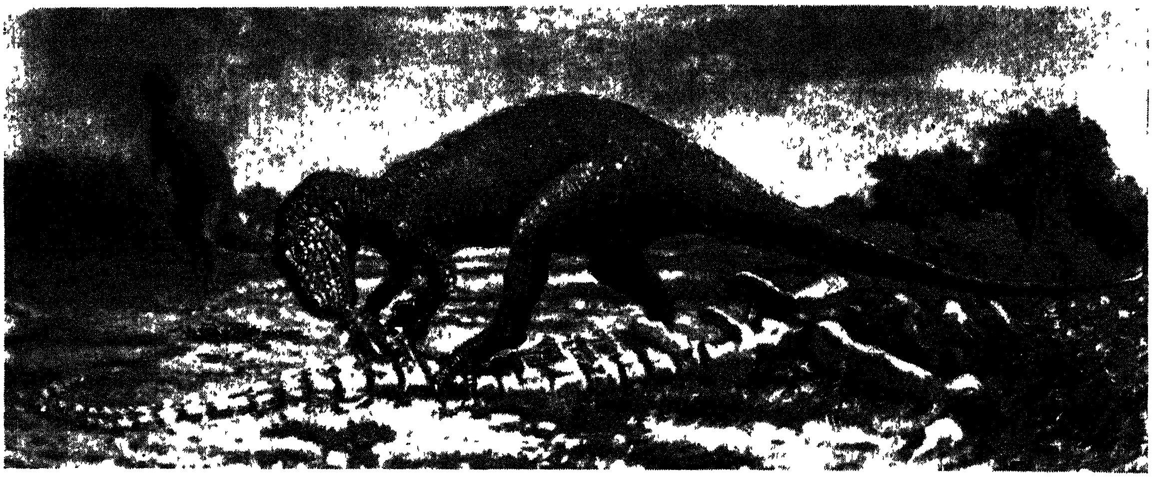
The independent evolution of giants among the different lines of dinosaurs at different times during the Mesozoic era leads to the question with which we are particularly concerned at this place—namely, how fast was giantism attained in the several groups of dinosaurs? Was the progression from small ancestors to giant culminating descendants roughly the same in all the lines, or were there differing rates of evolutionary growth? In any particular evolutionary line was the phylogenetic growth from ancestor to end form a constant progression in size through the ages, or were there differing rates within the single developing line? What does the fossil record show us?

The fossil record shows us quite a lot, but not as much as we would like to know. Fossils have a way of occurring sporadically, both geologically and geographically, rather than being evenly distributed through the sequence of sediments as they are exposed in all parts of the world At some stratigraphic levels and in some localities fossils can be very abundant indeed; at other levels and localities the rocks can be completely barren of fossils. Consequently, when we undertake to study any particular group of animals in the fossil record we find that our data consist of various suites of materials with geologic and geographic gaps between them. These limitations must be accepted and allowed for in any attempt to study fossil animals, particularly when the study involves such a subject as evolutionary rates, where, perforce, a complete record is desirable. It is necessary to go ahead in the best way possible with the materials available

When we look at the fossil record we see that the dinosaurs evolved as two distinct orders of reptiles, known as the Saurischia and the Ornithischia. It is quite evident that these two orders arose from a common ancestry, because they have many morphological characters held in common that show their relationships to each other. In spite of certain resemblances and common characters, however, the two orders were separate from the very beginnings of their phylogenetic histories

The first dinosaurs, appearing in beds of upper Triassic age, were, for the most part, small, primitive saurischians belonging to a suborder known

The small theropod dinosaur, *Ornitholestes,* of upper Jurassic age, shown catching the earliest known bird, *Archaeopteryx*.



Allosaurus was a giant meat-eating theropod of upper Jurassic age, commonly 30 feet or more in length. Shown here feeding upon the carcass of *Brontosaurus*.

as the Theropoda. They were lightly constructed little dinosaurs with hollow, fragile bones, and they were completely bipedal. They walked around on strong hind limbs, and the fore limbs, which were small, were used to aid in feeding. They were meat-eaters, and their jaws were armed with sharp teeth. One might say that these primitive theropods set a basic pattern from which many later and more highly specialized lines evolved.

One such line is exemplified by the progression from the genus *Coelophysis* of the upper Triassic period to *Ornitholestes* of the upper Jurassic period. *Coelophysis* was a primitive theropod, built along the lines described above as characteristic of the early dinosaurs. It must have been a very agile little dinosaur than ran through the jungle undergrowth in search of small reptiles and insects.

Ornitholestes, living many millions of years later than *Coelophysis,* was remarkably like its Triassic predecessor in size and structure, because it too was a small, quickly moving carnivore that pursued and caught lesser animals on which it fed. The Jurassic dinosaur maintained, 40 million years later, the same adaptations for the same mode of life that had been established by the Triassic dinosaur. From *Coelophysis* to *Ornitholestes* there was very little evolution, either as to structure or size. This was a slow line of evolutionary development.

From this slow evolutionary line there evolved a branch, however, that showed a considerable amount of evolution in both structure and size. This line culminated in the genus *Struthiomimus* of upper Cretaceous age. *Struthiomimus* lived about 40 million years later than did *Ornitholestes* and about 80 million years later than did *Coelophysis,* and during the lapse of time after this particular branch of dinosaurian evolution appeared as an offshoot from the primitive theropod heritage there was a considerable trend to increase in size. Whereas *Coelophysis* and *Ornitholestes* were animals that measured 6 or 7 feet in length, with probable weights in life of about 40 or 50 pounds, *Struthiomimus* had a length of 16 feet and a probable weight in life of several hundred pounds. Thus the later dinosaur increased in size many times over its earlier and more primitive relatives.

Though this size increase of *Struthiomimus* over its ancestors was striking indeed, the fact is that *Struthiomimus* at the most attained only a moderate degree of giantism It was a big reptile as compared with most reptiles with which we are acquainted at the present time, but it was rather small as compared with many dinosaurs with which it was contemporaneous; in fact, it was small as compared with many other theropods.

At an early stage in theropod history, there was a definite trend toward giantism, with the result that many genera of these dinosaurs became very large. These were the theropods collectively known as the carnosaurs, which probably arose in upper Triassic times as an offshoot from the small primitive theropods that already have been described. The trend toward giantism was apparent even in the ancestral Triassic carnosaurs, exemplified by such genera as *Zanclodon* or *Palaeosaurus*. It continued through the Jurassic period where, in late Jurassic times, we find the meat-eater *Allosaurus,* a dinosaur 30 feet or more in length with a weight in life of several tons. It continued through the Cretaceous period in such forms as *Gorgosaurus,* comparable to *Allosaurus* in size, and culminated in the truly great giant, *Tyrannosaurus,* of upper

Cretaceous age, an animal that measured almost 50 feet in length. It is interesting to see that throughout the evolution of these giant meat-eaters, the pattern of structure established by the small, primitive carnivores of the Triassic period was followed. In spite of their great size, the giant carnosaurs retained the bipedal pose of their ultimate ancestors; for active, predaceous dinosaurs this was a very efficient method of locomotion.

In this review of evolution in the theropod dinosaurs there is an answer to the questions that were raised at the beginning of the discussion The theropods do show that there were differing evolutionary growth rates during the Mesozoic era The line from *Coelophysis* to *Ornitholestes* was a slow-rate line (so far as increase in size is concerned), and that culminating in *Struthiomimus* was a moderate-rate line. As contrasted with these lines, the line of carnosaurian evolution, beginning in the Triassic and culminating in *Allosaurus* of the upper Jurassic, was a fast-rate line. In the slow-rate line there was little if any size increase during a lapse of some 40 million years, whereas in the fast-rate line the size was increased a great many times over in the same amount of time.

Moreover, the theropods illustrate the fact that evolutionary rates vary within a single developing phylogenetic line. In the evolution of the carnosaurs the greatest relative increase occurred during lower and middle Jurassic times, so that by the time *Allosaurus* made its appearance in the upper Jurassic period most of the growth to giantism had taken place. During the 60 million years of time after *Allosaurus* the increase of size among the carnosaurs was relatively minor. Many of the later meat-eaters were no larger than *Allosaurus,* and in only a few, such as the giant *Tyrannosaurus,* was there an increase of significant proportions. Consequently, the evolutionary growth rate in these dinosaurs can be pictured as an asymmetrical curve on a graph, which quickly approaches the limits of its greatest height and then levels off in a comparatively gentle slope.

The other lines of dinosaurian evolution give additional information as to differential rates of size increase, not only with regard to rates between separate phylogenetic lines but also as regards the rates within single lines. It may be of interest to consider some of them here.

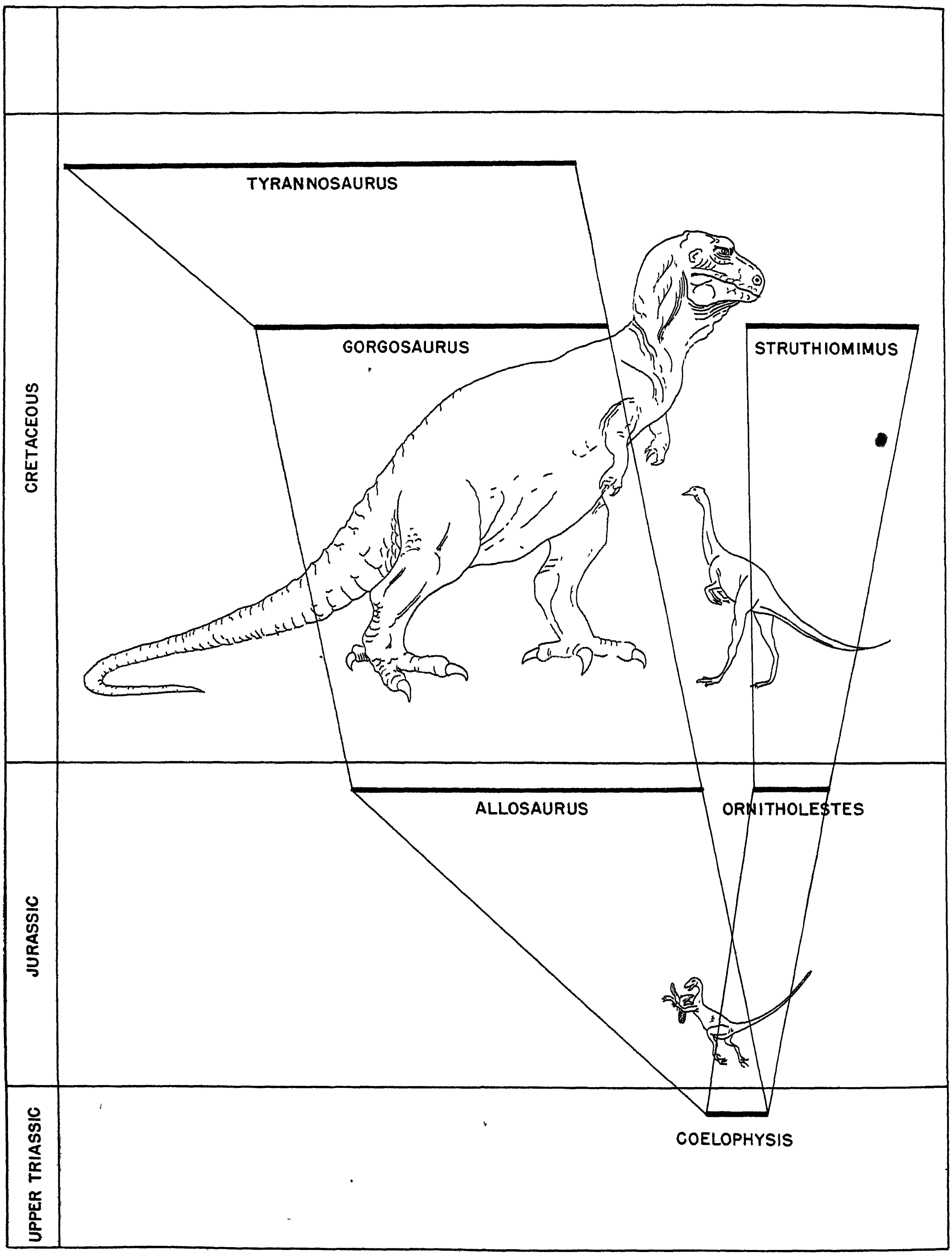
There was another suborder of the Saurischia known as the Sauropoda. These were giants among giants, the greatest of all the dinosaurs. They were great herbivores, with tremendously long necks and tails, massive bodies, small heads, and heavy, postlike legs. No longer were they bipedal, like their ancestors but, rather, because of their great size, were completely quadrupedal in posture. They lived for the most part in swamps, where they fed upon the abundant vegetation that grew in these places

The ancestral sauropods, exemplified by the genus *Plateosaurus,* were dinosaurs of moderately large size that lived in upper Triassic times Even at this early stage of their history they developed the trend toward giantism that was to be so characteristic of the sauropods, and they were many times larger than most of the contemporary meat-eaters Evolution toward great size continued at a comparatively rapid rate in this line of dinosaurs, so that by the end of the Jurassic period the sauropods had attained, in such animals as *Brontosaurus* or *Brachiosaurus,* the ultimate in size, not only among all the dinosaurs, but also among all land-living animals. There was no increase in size among the sauropods after the Jurassic period, which can be attributed to the probable fact that the Jurassic sauropods had become about as large as land-living animals can be.

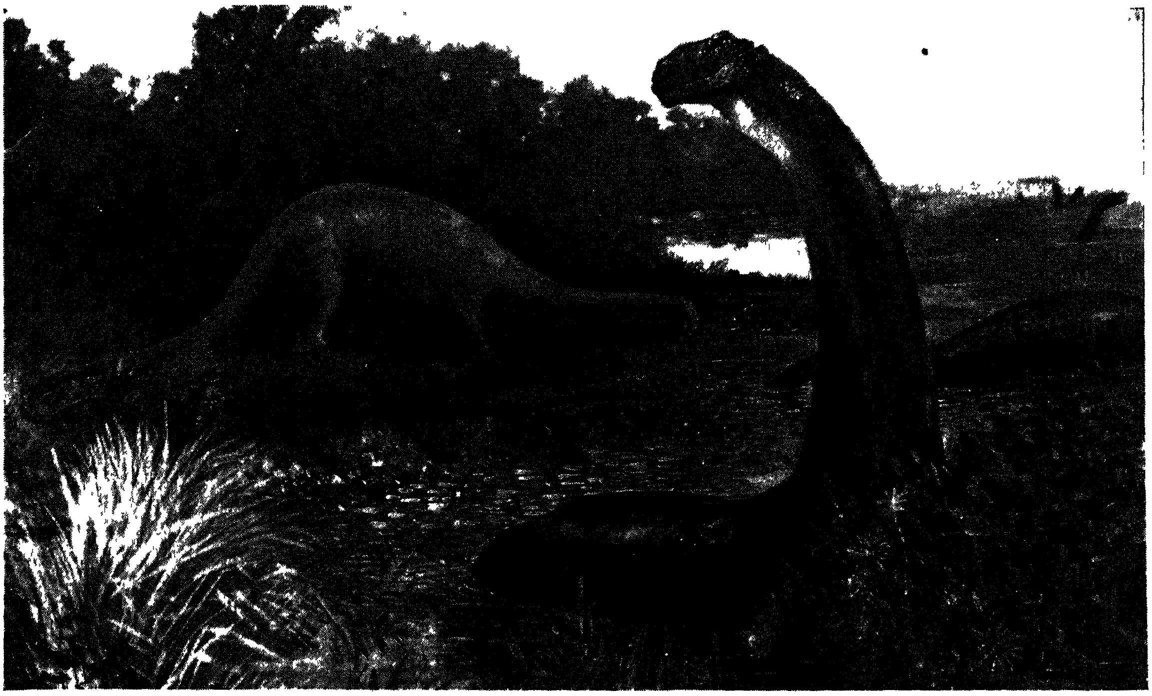
The ornithischian dinosaurs were, on the whole, a later and more highly evolved order of reptiles than were the saurischians. Their early representatives appeared later than did the early saurischians, and their various evolutionary branches developed at later stages in Mesozoic time than did the branches of saurischian evolution. Because of the late appearance of many of the ornithischians, their evolutionary histories were relatively short as compared with the histories of the saurischians. The contrast is interesting and instructive.

There were four suborders of the ornithischians: the Ornithopoda, or duck-billed dinosaurs; the Stegosauria, or plated dinosaurs; the Ankylosauria, or armored dinosaurs; and the Ceratopsia, or

Tyrannosaurus, the largest of the meat-eating dinosaurs. This was among the last of the dinosaurs to live during the final phases of Cretaceous times.



Size increase in two lines of theropod dinosaurs. One line, culminating in *Struthiomimus,* shows a moderate increase in size from the small ancestral types; in the other, culminating in *Tyrannosaurus,* giantism was attained during the Jurassic period, and from then on this was a line of giants.



Largest of the dinosaurs were the marsh-dwelling sauropods such as the upper Jurassic form *Brontosaurus*. Seventy or more feet in length, in life they must have weighed as much as 40 tons.

horned dinosaurs. Of these, the ornithopods retained to a considerable degree the bipedal pose of the primitive dinosaurs, but the other members of the order became quadrupedal. Giantism was prevalent, but none of the ornithischians became as large as the largest meat-eaters or, of course, as the giant sauropods. Without exception the ornithischians were herbivores.

Perhaps the most generalized of the ornithischians were the camptosaurs of upper Jurassic age. *Camptosaurus* was a small, bipedal dinosaur, comparable in size but not in structure or adaptations with the primitive meat-eating dinosaurs, described above. From a camptosaur ancestry the giant duck-bills evolved during the Cretaceous period. The evolutionary rate here was rapid, because by the beginning of upper Cretaceous times the duck-bills, such as *Corythosaurus* or *Saurolophus,* were beasts 30 or 40 feet in length, with body weights of several tons.

As contrasted with this evolutionary rate, the known rates of increase in the stegosaurs, or plated dinosaurs, and in the ankylosaurs, or armored dinosaurs, were comparatively slow. For instance, in the progression from *Scelidosaurus,* the earliest known stegosaur, to *Stegosaurus* itself there was a limited increase in body size occurring through a lapse of about 30 million years. The same was true in the sequence from *Polacanthus,* the earliest known ankylosaur, to *Ankylosaurus,* a late member of the group. It should be pointed out, however, that in these two phylogenetic lines of dinosaurs we do not have the earliest stages of evolution; the first known representatives in each line were already animals of considerable size. Without much doubt there must have been a comparatively short stretch of geologic time during which the earliest known genera of these dinosaurs evolved from small ancestors, following the same essential pattern of size increase that was developed in the evolution of the giant meat-eaters.

This pattern of evolution to giantism is very well exemplified in the history of the ceratopsians, or horned dinosaurs, which had their origin and went through their entire phylogenetic history within the compass of upper Cretaceous times The first true ceratopsians appeared at the beginning of upper Cretaceous times in Mongolia. Here we find *Protoceratops,* a small ornithischian dinosaur about 6 or 7 feet in length with a probable weight in life of perhaps 80 or 100 pounds. Within the course of the next few million years most of the increase in size took place. *Monoclonius,* a large horned dinosaur from sediments not much later in age than those in which *Protoceratops* was found, was a dinosaur about 18 feet long, with a weight in life of several tons. From this point to the end of Cretaceous times, a matter of 20 million years or so, size increase in the horned dinosaurs was relatively not very pronounced. The last of the

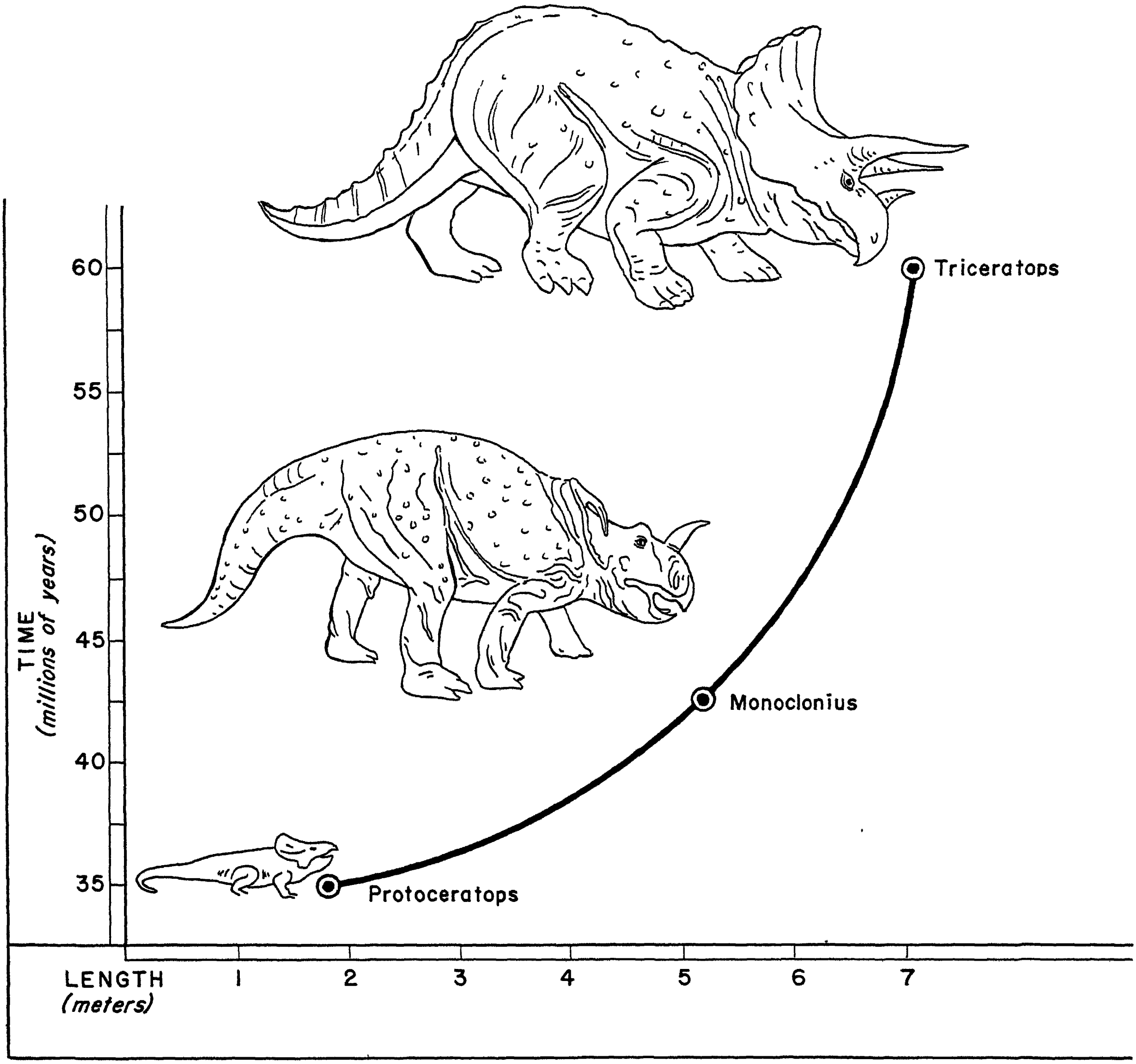
horned dinosaurs, such as *Triceratops*, were bulky animals a little more than 20 feet in length. Thus, in the history of the ceratopsians, which is well documented, there is seen the asymmetrical curve once again, rising rapidly and then tapering off in a long, gentle slope.

From the accumulated evidence there emerge certain facts regarding the evolutionary growth rates in the dinosaurs, and these can be briefly stated and reviewed. In the first place, it is evident that giantism was attained independently and at different times during the Mesozoic era by various lines of dinosaurian evolution. Certain meat-eating dinosaurs grew to great size during the interval between upper Triassic and upper Jurassic times, and the same was true of the herbivorous sauropods and the plated stegosaurs. The duck-billed dinosaurs followed the path to giantism in the interval between upper Jurassic and middle or upper Cretaceous times, and the armored dinosaurs became moderate giants during the same interval. Finally, the horned dinosaurs evolved as giants entirely within the limits of upper Cretaceous times.

Again, rates of size increase varied greatly in the different lines of dinosaurian evolution Some were slow-rate lines, and others were fast-rate lines. Certain meat-eating dinosaurs were almost static in the rate of size increase, whereas other theropods showed a rapid rate whereby they evolved from small ancestors to giant end forms within the confines of a single geologic period. The giant sauropods, the ultimate in size among all land-living animals, attained their maximum dimensions at a comparatively rapid rate during Jurassic times. In the Cretaceous period the armored dinosaurs showed relatively little size increase, but the duck-billed dinosaurs evolved rapidly during the same interval The horned dinosaurs, the latest dinosaurs to appear, also showed a rapid rate of evolution to giantism during the upper part of the Cretaceous period.

Finally, it is evident that the evolutionary rate,

Aquatic duck-billed dinosaurs were numerous and varied during upper Cretaceous times. Here is *Trachodon* along the shore of a Wyoming lake, perhaps 70 or 80 million years ago.



Increase in size through time of the horned dinosaurs. The increase from the small ancestral form, *Protoceratops*, to the giant, *Monoclonius*, took place at a relatively rapid rate. After that the increase to *Triceratops* was comparatively slow. (Illustrator's Corps, The American Museum of Natural History.)

so far as size growth is concerned, was not in the least constant within a single phylogenetic line. Rather, there was a marked variability in the rate of evolution, and this commonly followed a definite pattern. According to this pattern, there was an initial period of very rapid evolution, during which the developing line went through the greater portion of its size increase. After this there was a comparatively long stretch of phylogenetic history in which there was a limited size increase. To put it in another way, the evolving dinosaurs had a brief period of "growing up" followed by a "long life" as giants.

It is this long phylogenetic life of the dinosaurs as giant reptiles that is so impressive to the student of Mesozoic earth history. Most of the dinosaurs soon became giants, and as giants they dominated the earth for more than 100 million years. Evolution in the dinosaurs was largely a story of increase to large size, and for these ancient reptiles it proved to be a very successful pattern for long survival.

RECURRENT THEMES IN MEDICAL THOUGHT

ERWIN H. ACKERKNECHT

After receiving his M D. at Leipzig in 1931, Dr Ackerknecht held various medical positions in Germany and France Coming to this country in 1941, he was a Fellow in Medical History at Johns Hopkins until 1944 He spent a year as curator of anthropology at the American Museum of Natural History before going to his present post as professor of the History of Medicine at the University of Wisconsin.

MEDICAL theories and techniques have changed tremendously during past millennia, and particularly during the past 150 years. Medicine's goal, however, has always remained the same · to recognize diseases and cure the sick. Medicine has thus never been a pure science like, for example, physics, chemistry, or physiology. As much as medicine has profited from the progressive incorporation of scientific elements, it remains essentially an art using the findings of a great number of sciences, and proceeding empirically in those cases where no sufficient scientific knowledge is yet available.

In applying the traditional term "art" to medicine I should like to make clear that it has nothing to do with the inspirational activities or commercialized neuroses that are now associated with this term. If we had to translate again today medical "art's" linguistic predecessor and equivalent, the Greek *techne,* we would be on much safer ground with the term "craft," a less poetical but eminently respectable notion, by no means excluding a sense of beauty and harmony. At the time of the original translation the arts and crafts still belonged together, and medicine was called an "art" just as were agriculture and architecture, two other utilitarian activities, at that time equally poor in scientific foundations, but acting successfully on the basis of solid empirical knowledge.

It is due to its peculiar character that medicine has always been more a field of action than reflection; nevertheless, at certain turning points of their history doctors have been compelled to extend their thoughts beyond the more immediate problems of their field, and to ask and answer a few basic questions, such as "How is medical knowledge best acquired?" and "What are the limits of medical knowledge?" A few of the significant answers given to these two questions in the course of medical history are here described and briefly discussed.

I

In the fifth century B.C. more and more Greek medical craftsmen began to base their practice on the notions of "hot, cold, moist, and dry" that had been singled out by the philosophers, the speculative scientists of that period, as the fundamental qualities of all matter. In the famous treatise *On Ancient Medicine,* one of the writers of the collection of medical books that has been attributed to Hippocrates arose against this theory and developed his own ideas on medical method. Medicine, according to him, has so far been based exclusively on observation. It has to remain this way, and has no need of any hypothesis:

> Wherefore I have not thought that it stood in need of an empty hypothesis, like those subjects which are occult and dubious, in attempting to handle which it is necessary to use some hypothesis; as, for example, with regard to things above us and things below the earth.

Whereupon the writer relates in a very elaborate hypothetical story how in his opinion medicine developed out of observations on diet. Although this rationalistic tale sounds very unlikely to us today, it is incidentally a good description of the inductive method.

No less hypothetical, in spite of his many fine observations, are this author's ideas that faulty diet causes all disease or that disease is the predominance of one of *his* basic qualities in the humors (bitter, salt, sweet, acid, sour, insipid). He adheres to one of the oldest and most tenacious hypotheses in biology, the teleological one, in regarding disease as an effort of nature to throw off the morbific matter by coction, etc., etc.

Practically, our Hippocratic antihypothesist seems thus rather more opposed to hypotheses made by nonmedical men ("scientists") than to all hypotheses. Among the former he also seems to count many aspects of anatomy:

> Certain sophists and physicians say that it is not possible for any one to know medicine who does not know what man is (and how he was made and constructed), and that whoever would cure men properly, must learn this in the first place. But this saying rather appertains to philosophy, as Empedocles and certain others have described what man is in his origin, and how he first was made and constructed. But I think whatever such has been said or written by sophist or physician concerning nature has

less connection with the art of medicine than the art of painting.

All the medical man needs to know—"what man is in relation to the articles of food and drink, and to his other occupations"—he can fortunately learn best and learn only through medical observation.

Our somewhat one-sided selections might make the Hippocratic writer look a less acute thinker than he actually was. His keen realistic insight into the practical limitations of medicine in his period when faced by certain ideal postulates is evident in his remarks on "quantitating":

For one must aim at attaining a certain measure, and yet this measure admits neither weight nor calculation of any kind, by which it may be accurately determined, unless it be the sensation of the body. We ought not to reject the ancient art [of medicine], as if it were not, and had not been properly founded, because it did not attain accuracy in all things, but rather since it is capable of reaching to the greatest exactitude by reasoning, to receive it and admire its discoveries, made from a state of great ignorance, and as having been well and properly made, and not from chance.[1]

The author of *Ancient Medicine* was a conservative, fighting a losing battle. Hypotheses like the "hot, cold, dry, moist" won, even in his own time, and for the next 2,000 years the field was most of the time dominated by some brand of dogmatist that placed theories, hypotheses, and real and fictitious results of other sciences above mere clinical observation. It is of the essence of dogmatists that they need not theorize why they are dogmatists. But those who revolted against dogmatism had to show why observations should prevail over theories and hypotheses. Their writings form, with a few exceptions, the bulk of medicophilosophical literature. Almost invariably these medical rebels and philosophical empiricists would now march under the awe-inspiring banner of "Hippocrates." Whatever the viewpoint of "Hippocrates," or even of the author of *Ancient Medicine,* may have been, they would take up more or less completely and consistently the points, abstracted above from *Ancient Medicine,* and show that the only healthy development of medicine would lie along the road of this "Hippocratism."

They thus became involved also in the same self-delusions and contradictions. While they fought hypotheses, they had all their own hypotheses and theories, for it is impossible to handle observational old or new facts for any length of time without these aids. Any approach toward solving medical problems—which are only specialized problems of general science—by nothing but unconnected medical observations, would sooner or later come to a dead end. And yet paradoxically enough, these philosophically and logically deficient movements are *landmarks of real medical progress.*

The first to rebel in this vein were apparently the "Empiricists" of Alexandria in the third century B.C. Unfortunately, our knowledge of them is scanty and indirect, stemming mostly from quotations in later writers like the Roman Encyclopedist Celsus (who lived around the beginning of our era) or Galen. But what little we know of them bears the unmistakable earmarks of our particular brand of "Hippocratism": their opposition to theory and hypothesis, their contempt for anatomy and dissections, their defeatism as to the possibility of knowing "hidden causes," and so on. They were soon outnumbered by dogmatic sects, and after Galen medicine was enveloped for 1,300 years in the rarely disturbed night of sterile Galenicist dogmatism.

II

In the Renaissance medical men not only produced a great mass of new observations in all fields of medicine—clinical, psychological, anatomical, and surgical—in this period also one of the most radical, most boisterous, most confused, and most gifted "Hippocratic" rebels entered the scene in the person of the strange Dr. Philippus Theophrastus Bombastus Aureolus von Hohenheim, called Paracelsus (1493–1541),[2] a contemporary of the Reformation and the German Peasant's War.

Hippocrates, the wayfarer, the man of observation, was the only medical author of the past that the revolutionary Paracelsus respected. Otherwise he despised ancient medical books and what they stood for to an extent that he could easily be credited with the legend of burning them publicly. Like other Hippocratists, he had no use for anatomy. He was somewhat more tolerant as far as theory goes, asking only that "not out of speculative theory should practice flow, but out of practice theory." (Nobody acted less according to this golden rule than Paracelsus himself.) Paracelsus the chemist—it is in this field that his immediate influence on medicine has been greatest—would allow chemical and astrological observation to be added to clinical observation. Paracelsus the mystic, still full of the all-pervading religiosity of the Middle Ages, is the only medical philosopher to have made God his fourth immediate source of medical knowledge. He was thus the father of what Marshall Clagett so aptly calls "iatrotheology."

The men of the sixteenth century had dealt heavy blows to ancient dogmatism, but the very discoveries of the young experimental sciences, chemistry and physics, became prematurely applied to medicine in the seventeenth century by the

iatrophysicists and iatrochemists, and thus formed the basis for new dogmatisms, no less deductive, stifling, and fact-distorting than the old. It was Thomas Sydenham (1624–89)—not incidentally an ex-captain in Cromwell's cavalry and a friend of John Locke—who raised the banner of the fourth "Hipprocratic" rebellion in the seventeenth century. His scorn for all medical literature except the works of Hippocrates is clearly expressed in his recommendation to a medical student who had asked him what books to study: "Read *Don Quixote*." He had no use for physics and chemistry, not even for the new anatomy and physiology. Clinical observation again is the sole and only foundation. Hypotheses are worthless. It is, as he repeatedly states in the prefaces to his *Medical Observations of Acute Diseases,* impossible to find the "final causes" of disease. Etiology is essentially inexplicable. Thus treatment has to be derived from the observation of symptoms.[3]

Actually Sydenham abounds, of course, not only with fine clinical observations, but also with doubtful theories. He maintains not only the old Hippocratic teleological "coction" of humors as an effort of nature to overcome disease, or develops Hippocratic clues into his vague theory of "epidemic constitutions," but he creates such un-Hippocratic notions as the classification of diseases into species like plants and animals, or the striving for an automatic *methodus medendi.* Actually, also, Sydenham is another case of split personality, theorist and antitheorist in one. In this respect some historical injustice has been done to many of his iatrochemical and iatrophysical contemporaries such as Willis, Sylvius, or Baglivi. Their dogmatic utterances and Sydenham's antitheoretical declamations have, especially in more recent times, both been taken too much at face value. These men, too, were split personalities, in their case hiding first-rate clinical qualities under the cloak of dogmatism. Although Sydenham undoubtedly was a great clinician, he was only one of many great clinicians in the seventeenth century. True Hippocratism never dies out among really good doctors, fanciful as may be the theoretical notions that they embrace.

III

The common man's hopes for freedom during the Reformation had come to nought. Absolutism in its crudest forms followed the great awakening. A similar situation prevailed in medicine, where the "systems" ruled sovereign during the seventeenth and eighteenth centuries. The same great French Revolution of 1789 that wiped out absolutism was followed by a medical revolution that wiped out the "systems." This revolution gave France world supremacy in medicine for half a century,[4] bringing into being the great French clinical school, also called the pathologico-anatomical school, or the "school of observation." This French school represents the fifth of our "Hippocratic revolts," and is, for that matter, the most consistent and most successful of all.

Like the political revolution that had been preceded by a revolutionary philosophy, the "medicine of observation" was preceded by a medical "philosophy of observation" which is usually connected with the name of the ideologue P. J. G. Cabanis, a follower of the sensualist Condillac.[5] Among those who put this philosophy of observation into practice, at least the leading group of the Paris school (G. L. Bayle, Laennec, Chomel, P. Louis) shows the well-known fundamental tenets of earlier "Hippocratic revolts."[6] Observation is the exclusive foundation of medical science. Therefore, among the authors of the past only the observer Hippocrates (and possibly Sydenham) deserve consideration. The following quotation from Laennec, the inventor of the stethoscope and auscultation, shows the typical aversion to hypothesis and theory, and the usual defeatism as to the possible elucidation of causes:

> I shall not here attempt with the nosologists to divide the diseases of which I propose to treat into genera and species. . . . I shall still less endeavor to ascertain the primary, or as they are called, proximate causes of diseases. The vanity of researches of this kind is sufficiently proved by the profound oblivion into which all theories of this nature have successively fallen. I shall content myself with describing the disease of the thoracic organs. . . .[7]

This attitude toward theories is also reflected in Laennec's definitions of medical science: "Only the facts constitute science." "Theories in science must be regarded as aids to memory. They do not constitute science." The logical outcome of this attitude was P. Louis' "Numerical Method" (clinical statistics). If you can't reason about facts, you can at least add them up and divide. Neither chemistry, nor the animal experiment, nor the microscope was used by this school.

In order to understand why it nevertheless was so conspicuously successful—far more successful than all its predecessors—we must realize that the "observation" it practiced was no longer merely the passive observation of symptoms that constituted almost the total observation of the past. Using the new methods of auscultation and percussion, the Paris doctor of the 1820s no longer only "observed" the patient, he "examined" him.[8] He furthermore checked his observation with the

findings at the autopsy table. There he found the anatomical lesion, which now became his point of reference, instead of vague symptoms. And all this he did, not on the small scale of a rural practice, but on the "industrial" scale of the large hospitals of the big city.

Still, even this school, which opened a new world to medicine, suffered from the same contradictions as its empiricist predecessors, and it ended in a blind alley when its technical and philosophical limitations caught up with it. It was the last of the great empirical movements in medicine. The scientific and philosophical foundations on which its successor, appearing under the label of "physiological" or "experimental" medicine, built have now lasted for more than a hundred years. Not that the older ideas are dead; but our fundamental concepts have not been seriously shaken by the "neohippocratic," "neosydenhamian," "neoparacelsian," and "neolouisian" attempts of the past three decades.[9]

The finest philosophical exposition of modern medicine is still Claude Bernard's *Introduction to the Study of Experimental Medicine* (Paris, 1865). Bernard gave us a proper attitude toward hypothesis that is controlled through the experiment. He gave a real philosophy of the experimental method, which is more than just experimenting. He relieved us of the old sterile dichotomies between empirical and scientific medicine; induction and deduction; observation and experiment. His notion of the internal environment was an important attempt to bridge the contradictions between mechanism and vitalism, determinism and free will.

The solutions provided by Bernard and his contemporaries were possible only on the basis of the great amount of newly acquired actual knowledge. A more correct philosophy was found only when a reality had been created that corresponded to it. Before, occasional guesses as to such solutions were possible, but they could be consistently maintained only to the extent that they could be applied. Now that we know at least *something,* we have also for the first time become able to face the whole magnitude of our ignorance. The confidence of former generations in the mere figments and fragments of knowledge they possessed is touching, psychologically understandable, and was probably necessary; yet there is no doubt that such delusions would also play a retarding role in the progress of knowledge.

Such discussions of the medical and scientific thought of the past as we have indulged in will probably seem superfluous to many "practical" scientists. Yet the problems that provoked our forefathers do still exist, although perhaps on a higher level. And it cannot be hoped that even Bernard's philosophical solutions will have permanent validity. The best proof for the necessity of such discussions lies in the amazingly unscientific attitude shown by just such critics (often great scientists) if they are faced by any problem lying outside the limits of their often very narrow specialty. The inability of many scientists to approach problems outside their specialties in a scientific way (unemotionally, objectively, critically, quantitatively) is the consequence of the fact that they have never become conscious of the philosophical premises of their own work by comparing them to other possible premises. This failure of scientists is a tragedy. Science could contribute more to humanity than refrigerators, or vitamin pills, or atomic bombs if scientists would not only make science, but, in the footsteps of so many of their greatest, also think of it. It might even help them in making science.

NOTES

1. The Hippocrates quotations are taken from *The Genuine Works of Hippocrates,* translated by Francis Adams. New York: 1886, Vol. I, 132, 143, 136, 138.
2. The only available English Paracelsus translation is the *Four Treatises of Theophrastus von Hohenheim called Paracelsus* (H. E. SIGERIST, Ed), containing the "Seven Defensiones," translated by C. LILIAN TEMKIN, which are particularly relevant to our subject. Baltimore: 1941.
3. See especially *The Works of Thomas Sydenham,* translated by R. G LATHAM London. 1848, 16, 4, 14, 18, 20, 72, 16
4. This French supremacy is not limited to clinical medicine. It extends to numerous other sciences—and hygiene. See ACKERKNECHT, E. H. Hygiene in France 1815–1848. *Bull Hist. Med.,* 1948, **22**, 117 ff.
5. Concerning the philosophy of Cabanis see the two following excellent papers: TEMKIN, O. The Philosophical Background of Magendie's Physiology. *Bull. Hist. Med.,* 1946, **20**, 10 ff.; ROSEN, G. The Philosophy of Ideology and the Emergence of Modern Medicine in France *Bull. Hist. Med.,* 1946, **20**, 328 ff.
6. For a detailed analysis of the philosophy of the Paris school see the forthcoming article of E. H. ACKERKNECHT, Elisha Bartlett and the Philosophy of the Paris Clinical School. Elisha Bartlett's *Essay on the Philosophy of Medical Science* (Philadelphia, 1844) is the only contemporary systematic exposition of this French medical philosophy.
7. LAENNEC, R. T H. *A Treatise on the Diseases of the Chest* New York: 1835, 60. (First edition, Paris, 1819).
8. FABER, K. *Nosography in Modern Internal Medicine.* New York: 1923, 36.
9. The following interesting article uses also analyses of the five movements described above, though from a different point of view: GUTTENTAG, O. E Trends Toward Homeopathy, Present and Past. *Bull. Hist. Med.,* 1940, **8**, 1, 1,172 ff.

THE SEVENTH PACIFIC SCIENCE CONGRESS

ROBERT CUSHMAN MURPHY

Dr. Murphy sits in the world's only endowed chair of ornithology. He is Lamont Curator of Birds and chairman of the Department in the American Museum of Natural History. Dr and Mrs. Murphy have spent the past two winters in New Zealand, engaged in part in excavating the remains of moas and other extinct birds. In 1949 they were both accredited delegates to the Seventh Pacific Science Congress.

FOR members of the delegation of the National Research Council an unforgettable aspect of our New Zealand experience was the flight from Washington in a plane of the United States Military Air Transport Service. The principal object of Captain Gratton C. Miller, in command, seemed to be to gratify the curiosity of his scientific contingent, even to the extent of minor departures from course. Most thrilling of several such opportunities was an ascent to 17,000 feet above the then active crater of Mauna Loa on the island of Hawaii. Making loops or figures of eight, the craft passed three times directly over the *caldera* so that we could watch the fountain of molten lava welling up at least two hundred feet above the orifice, spilling over as a "waterfall" into the smaller South Crater, and thence flowing down the slope in three great fingers, each with a glowing axis.

On the opposite, or southern, side of the *caldera* we saw the cold black lava of a former flow which had threatened Hilo, but which had been thwarted by Army bombardiers who smashed the hardened tube through which fiery fluid was pouring, thus diverting the stream into an uninhabited valley.

All this private view of the world's greatest insular volcano was accompanied by a running commentary by Dr. Donald E. White, the official volcanologist of our group, and by the botanist Dr. F. R. Fosberg, who knows the island of Hawaii with particular intimacy.

A second bit of unscheduled scouting came in Fiji, where our plane hedgehopped over the emerald wilderness of Vanua Levu, giving us glimpses of hidden valleys and lakes, unmapped peaks, and scattered Melanesian villages.

Previous congresses in a series unduly interrupted by the war had been held in Hawaii (1920), Australia (1923), Japan (1926), Java (1929), British Columbia (1933), and California (1939). The New Zealanders, taking over a responsibility that the war-battered Filipinos had been obliged to defer, extended invitations for February 1949 to forty-eight countries and to several international agencies, such as appropriate commissions of the United Nations. The Seventh Congress was launched at Auckland on February 1, the date of our arrival, with an evening reception given by the Mayor of the City—"His Worship" rather than His Honor.

AUCKLAND SESSION

The opening of the scientific sessions in Auckland University College on February 2 saw an impressive gathering of nearly five hundred delegates from New Zealand and countries bordering the Pacific, as well as from Great Britain, France, and the Netherlands, which have historical and national ties with extensive Pacific areas. The three Scandinavian nations, all of which have conducted scientific work of high significance in the Pacific Ocean, were also represented. The chairman of the Norwegian delegation somewhat whimsically remarked that his country actually has a Pacific possession; he thereupon invited the audience to locate Peter First Island without scrutinizing a map.

The chairman of the Swedish group, Dr. Carl Skottsberg, an authority on island plant relationships, noted that his own country is about as far from the Pacific as it is possible to travel. "On the west," remarked Dr. Skottsberg, "Sweden is separated from the Pacific by another ocean and a continent. On the east it is blocked off by a vast territory in which the present climate is so unfavorable that international highways have been closed."

Incidentally, the USSR had not replied to the invitation of the Royal Society of New Zealand to participate in the congress. Hawaii, on the other hand, was present as formerly in the full stature of a "nation." Dr. Peter H. Buck (now Sir Peter Buck, known in New Zealand as Te Rangi Hiroa because of his Maori origin), director of the Bishop Museum in Honolulu, and chairman of the Hawaiian delegation, remarked somewhat wryly

that he failed to understand why Americans did not universally view Hawaii as one of the states of the Union when the rest of the world, at least as exemplified by Pacific Science Congresses since 1920, obviously granted it the status of a sovereign country!

Such comments from the chairmen of the various national delegations were characteristic of the humor that accented the completely serious tone of replies to the gracious New Zealand welcome extended by Dr. Robert A. Falla, president of the congress. A state of enthusiasm was reached when the Philippine delegation, appearing for the first time as spokesmen for a new republic, requested the members and participants to make the Philippines the site of the Eighth Pacific Science Congress. This invitation of Dean Antonio G. Sison, of the Medical College at Manila, was subsequently accepted, though without the designation of a date, which probably will be set within the next two years. The ravages of war had compelled the Republic of the Philippines to yield its place to New Zealand; cheering therefore greeted Dr. Sison's description of the valiant work of reconstruction that will fit the ancient capital to serve as host for the Eighth Congress.

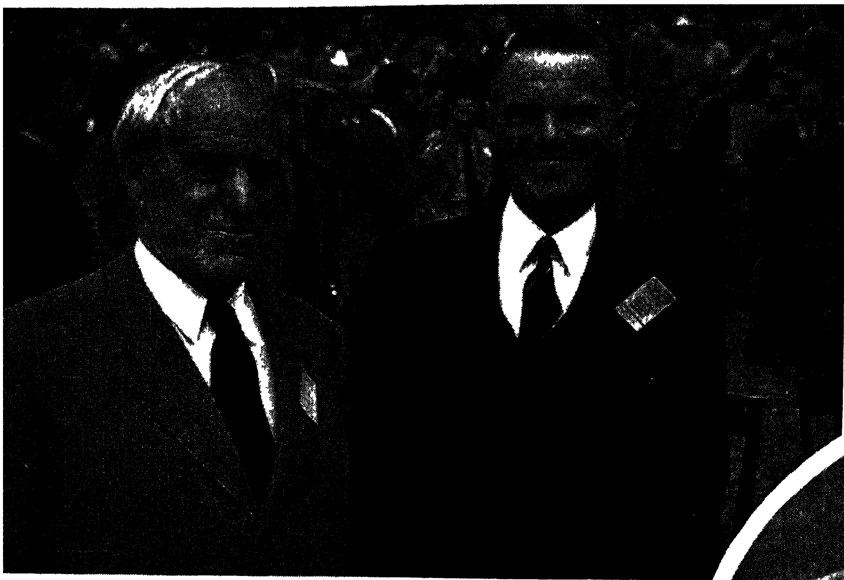
It was interesting to note that all delegates, including those from the Philippines, the East Indies, and Scandinavia, spoke in English, with the exception of the French representatives from Europe, Indo-China, or the Pacific possessions, all of whom used their own tongue.

The inaugural assembly was followed by meetings of the ten separate sections of the congress, each of which proceeded into its scientific agenda as soon as the organization details had been attended to. These sections were: Geology and Geophysics; Meteorology; Oceanography; Zoology; Botany; Soil Resources, Forestry, and Agriculture; Anthropology; Public Health and Nutrition; Social Sciences; Organization of Research. A glance through the eighty-six printed pages of the general program showed a strong coordinating tendency among the various scientific disciplines. Zoology and Botany, for example, combined part of their sessions with Oceanography, part with Agriculture, and there were many other liaisons exemplified by anthropological, medical, and social sciences, etc. In the admirable organization of the congress the energy, skill, and tact of Dr. Gilbert Archey, who had accepted the arduous post of secretary-general, have been evident from the beginning of negotiations in 1947 to the completion of responsibilities still under way.

It scarcely needs to be pointed out that problems requiring international scientific cooperation are especially suitable for assemblies marked by a geographic limitation. Much of the New Zealand program dealt with fundamental science, as might be expected from the stated aims of the Pacific Science Congresses—as well as from representatives of research in the native land of Lord Rutherford. At the same time, lay followers of the proceedings occasionally needed reminding that basic subjects, such as mathematics, were in large measure excluded by the geographic scope; and that even physics and chemistry entered chiefly as the handmaidens of seismology, meteorology, biology, etc., as these related to Pacific areas. Such facts always create difficulties for a program committee. It is even to be suspected that here and there the phrase "with special reference to the Pacific" was tacked onto the end of a title with the object of dragging it into the regional paddock!

Both morning and afternoon sessions were regularly interrupted for general gatherings at tea, and in this respect all the visitors seemed rapidly to become inveterate New Zealanders. The evenings were devoted to public lectures, of which the first, entitled "Leaves from a Plant Collector's Diary," was given by Professor Knowles A. Ryerson, of the University of California, chairman of the American delegation. A lecture on the evening of February 3 by Te Rangi Hiroa burst the seams of the largest available hall in Auckland, so that the doors had to be shut against hundreds still seeking admission. This was a tribute not only to Dr. Buck's intellectual vigor and sprightly speech, but also to the fact that, as anticipated, he lifted the roof with an ancestral Maori chant before he left the platform. His lecture, which was illustrated by a documentary color film, related to Kapingamarangi, an atoll south of Truk in Micronesia which, surprisingly, is inhabited by purely Polynesian people whose language differs only dialectically from Dr. Buck's native tongue. The lecturer was officially thanked by the elderly statesman Sir Apirana Ngata. Although a rock-ribbed conservative, who has not espoused the Labor cause along with most of his Maori people, Sir Apirana is universally admired for his character and his pungent oratory.

Other interruptions in what might have become a too-tense experience in scientific concentration came in the form of afternoon parties in the open air among well-nigh peerless New Zealand gardens, which were at this date at the height of their luxuriance. Furthermore, we were all looking for-



Presidents, veteran and new. Prof. Herbert E. Gregory, president of the original Pacific Science Congress at Honolulu, in 1920; and Dr. Robert A. Falla, president of the Seventh Congress.

Delegates from overseas. Athelstan F. Spilhaus, University of Minnesota; Wang Ging-Hsi, UNESCO; G. E. R. Deacon, Admiralty Research Laboratory, England; Patrocinio Valenzuela, University of the Philippines.

Informal conference between Professor G. W. Robinson, agricultural chemist and chairman of the British delegation, and Mr. Charles W. Fleming, New Zealand geologist.

Visiting the camp of the New Zealand-American Fiordland Expedition, where long-term investigations on the effect of introduced deer upon native "bush" are in progress. Col. J. K. Howard, of Boston, organizer, is third from the right; at the left end is Dr. Olaus J. Murie, of Wyoming, scientific director.

ward to the intersessional and postsessional tours that subsequently transported visitors, as guests of a generous government, to many of the famous regions of natural charm in New Zealand, including the thermal districts, the mountains, glaciers, and national parks.

By the middle of the first week, the number of delegates from overseas had passed two hundred, the largest representation in the history of the Pacific Science Congresses. The total attendance was next in size to that of the Sixth Congress in San Francisco, ten years ago.

Important as were the scientific meetings at which contributions were presented and discussed, the contacts and informal chats with colleagues from other lands offered perhaps the most important opportunity of all. During the first day or two, visitors from the United States and Canada made many literal "contacts" with their New Zealand confreres in some of the narrow corridors of Auckland University College. The overseas men instinctively turned to the right, the New Zealanders, of course, to the left. As one of our own group remarked, the American expression "I'm glad I ran into you" acquired a new significance!

For lack of a sufficient cooling-off period after the bitterness of war, the Japanese had not been permitted to send their nationals to this congress, a fact regretted by many. A few of those present remembered happily the Third Congress, at Tokio, in October and November, 1926. All who flew from Japan to New Zealand were Americans who came by authority of General MacArthur, the Supreme Commander for the Allied Powers. Most of these delegates, however, brought with them manuscripts by Japanese scientists and presented them on behalf of their authors. There were, indeed, about one hundred fifty contributions by Japanese, and it is noteworthy that they were as well received and as freely discussed as any of the others.

USE AND MISUSE OF NATURAL RESOURCES

New Zealand, as a prevailingly agricultural and pastoral country, lives and exports mainly from the soil, and its citizens have a keen eye for applications of science that will directly enrich their resources or accomplish the same end indirectly by slowing down the rate of exploitation. Conservation was, indeed, the keynote of many papers in practically every section of the congress program, indicating a realization of the fact that all the modern world is living largely upon the capital rather than the interest of natural wealth. A Dominion that aims to send a thousand tons of food a day to Great Britain in addition to maintaining a high level of subsistence for its own population was, of course, interested in learning of food supplies not yet significantly tapped. Deep interest was expressed in a report to the congress by Dr. J. L. Kask, of the Food and Agriculture Division of the United Nations Organization, to the effect that the Pacific, the largest of the world's oceans, yields only a third of the total marine fish catch of the world, and that of this small proportion less than 5 percent is taken from waters south of the equator.

Resolutions adopted at the final plenary session reveal the deep concern of visitors and native sons alike in the status of New Zealand soils, water tables, and plant cover, and in the role of introduced organisms. Many of the last-named have long been acknowledged pests; and yet the feeling was expressed that New Zealanders seem to have difficulty in making up their minds—permanently—as to when vermin is vermin and when it is not. Rabbits, for example, suffer the enmity of agriculturalists while enjoying the favor of dealers in meat and pelts. (They thrive on both!) Few of the foreign delegates, despite the briefness of their experience, had any doubt about the curse of alien animals. They at least had the backing of an able New Zealand forester, Dr. Holloway, who stated forthrightly that it would be impossible for the Dominion to keep both introduced deer and native beech (*Nothofagus*) forests, and that the decision as to which of the two is wanted forever is strictly a home problem.

Although there was a certain diversity of opinion regarding the implications of soil erosion, all visitors emphasized its seriousness. Professor G. W. Robinson, chairman of the British delegation and a renowned expert, held a relatively hopeful outlook, whereas others, such as Mr. Ernest G. Holt, of the American Soil Conservation Service, regarded the present damage in certain areas as the worst they had ever witnessed in temperate lands, and amenable to repair only through prolonged and unflagging labor involving the re-establishment of dense plant cover on hilltops and upper slopes.

In science as applied to farm life, New Zealanders have long been pioneers and leaders. For example, the "progeny test," originally advocated in Denmark as a technique of judging and breeding livestock, has been carried to its logical conclusion in the sheep runs and cattle pastures of New Zealand. The method implies that in com-

petition a domestic animal, such as a ram or a milch cow, is judged not merely on its conformation, ancestry, and "blue-ribbon look," but even more upon the character of its offspring. This means that at New Zealand fairs the best-looking animal is by no means sure of first prize. It may, and often does, lose out to another that has produced a superior line of descendants. In short, "handsome is as handsome does." The real criterion of excellence is not what the creature appears to be, but what it begets. In the United States, there is still opposition to this point of view among breeders because many owners and exhibitors incline toward prejudice in favor of the most sleek, becurled, and beribboned entry in the stock show, and are ready to assume that such will likewise produce posterity of equal worth. New Zealanders are more hardheaded and empirical. American delegates taking part in the section of Soil Resources, Forestry, and Agriculture were vitally interested in the progeny test and in evaluating the results as they found them at the agricultural institutes of both North and South Islands.

SCIENCE AND SOCIAL RESPONSIBILITY

Among participants in the congress, the anthropologists were perhaps represented in largest force. They also seem to have had a particularly lively time. The content of many papers in this comprehensive field indicates a new spirit in the world with respect to what used to be called "subject peoples." The general theme of the earlier anthropological sessions was announced as "Administration and welfare, including contemporary cultural changes among Pacific island peoples."

No longer, it appears, are the bigwigs of administration wont to assume that the fewest suggestions and the least interference from any outside source will enable them to accomplish the most acceptable job. On the contrary, most of those charged with the responsibility of governing have grown humble; they crave, and even pay for, the wisdom that may issue from specialized research. It is surely symbolic that, when Professor Ernest Beaglehole criticized aspects of his own government's regime in the Cook Islands and Western Samoa, the Prime Minister of New Zea-

Evergreens two hundred feet tall. New Zealand has all too few remaining stands of its half a dozen kinds of huge podocarps, relatives of the yew. These trees, at Inangahua, are *kahikateas,* or "white pines" (*Podocarpus dacrydioides*).

land merely turned the other cheek, stating to reporters that Beaglehole's paper would be studied by the Island Territories office "with a view to translating such criticism into beneficial administrative action."

Beaglehole said that the contrast between self-government and good government is much more obvious in the capitals of world powers than it is among Pacific dependent peoples. Good intentions, together with self-interest, frequently delay recognition of the fact that a colonial people may be fitted to govern itself, even though the level of government thus attained may not be as efficient as imposed government. But inherent progress is more important than mere orderliness. We must remember, he stated, that in homogeneous island societies the residents' wants are often easily met, provided they obtain them in their own way and place and time. Self-government for such societies is likely to be more satisfactory in the long run than any type of imposed government Administrators should be expected to draw more than in the past upon the insights and techniques to be derived from contemporary psychology and anthropology.

Americans listening to the opinions of this New Zealand scholar could not help but rejoice that the Philippines had been turned over to their own people on the due date, regardless of the temptation to delay because of the international turmoil in the Pacific.

The outmoded missionary notion of completely supplanting original customs by provincial *mores* of the white man is, of course, anathema to current points of view, religious as well as scientific. Sir Peter Buck, always a wag, remarked during one of the discussions that the former insistence upon covering nudity had more or less faded out of the Pacific island picture because present-day whites are "outstripping the natives."

NORTH ISLAND AND SOUTH

Between the citizens of the two large islands of New Zealand an old and healthy rivalry leads to bantering similar to that of the Floridians and Californians. The way to make a North Islander froth at the mouth is to tell him that you have just crossed Cook Strait from the "mainland."

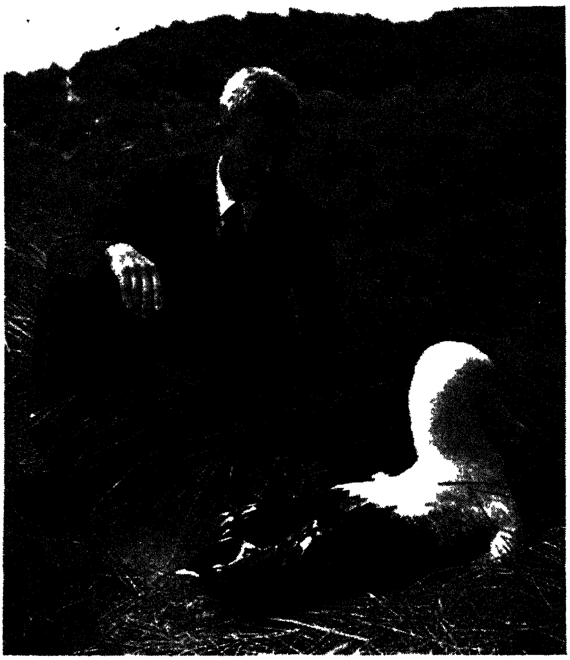
Before the delegates of the Seventh Pacific Science Congress left Auckland for the concluding session at Christchurch, the North Island itself seemed to take a hand in the striving for popularity. The last sunset at Auckland, for example, was marked by a magnificent rainbow over the Gulf of Hauraki. The first evening at Wellington, the capital put on a welcoming display of the aurora australis. Over the ocean to the south the sky was a red and orange glow, against which the Kaikoura Mountains, across Cook Strait, stood out in livid silhouette, and a ghostly gleam tinged the landscape about us, suffusing even the grass at our feet.

These phenomena, however, were only a small sample of the North Island's bag of tricks. During the southward series of tours, in which all overseas members of the congress participated, the routes of the buses carrying botanists, meteorologists, anthropologists, etc., converged at Rotorua, in the thermal district, where the geysers and fumaroles put on the best show known for months past. Not to be outdone, Mount Ngauruhoe, one of the triple volcanoes in the Tongariro National Park, west of Lake Taupo, went into violent eruption after a long period of quiescence. The visiting volcanologists, of course, climbed the mountain even beyond the zone of safety. Several larger parties were driven to points of vantage as close as six miles from the cone, where for two or more days and nights they saw the fiery halation around the rim and watched house-sized rocks being shot into the air to heights of 1,500 or 2,000 feet above the crater, all to the accompaniment of earth rumblings and gargantuan machine-gun fire.

The larger neighbor of Ngauruhoe, Mount Ruapehu, discreetly remained seen but not heard, no doubt because it had put on its own exhibition with considerable fury only two years ago. A lasting impression for many of the visitors was that of the pink-flushed snowfields of Ruapehu, looming up above 9,000 feet and seen at evening of the last day in North Island across fifty or sixty miles of lowland.

Finally, at Wellington we were served with a very comfortable little earthquake, which made our beds quiver thrice as a prelude to the arrival of early-morning tea.

A notable experience for all members of the anthropological party and many of the other visitors was the welcome arranged by the communities of Maoris for the distinguished representative of their own blood, Sir Peter Buck, who had not previously returned to his native land for many years. The bulk of the Maori people still resides in the North Island, as in primitive times, and at each center ancestral tribal celebrations were staged for the homecoming, at which ancient costumes, ritual dances, and Polynesian chants and feasts all had their part.



Where else but in New Zealand could delegates to a science congress reach the nests of albatrosses by motor? The author at Taiaroa Head, the tip of Otago Peninsula.

CHRISTCHURCH, AND THE ZOOLOGICAL PROCEEDINGS

From Wellington the visitors traveled by steamer to Christchurch, most English and Anglican of all New Zealand cities. The time of arrival was before breakfast of February 16, and yet by mid-morning everybody was thoroughly absorbed, in halls around the Gothic quadrangles of Canterbury College, in the smooth continuation of the scientific program that had been broken off more than a week earlier at Auckland.

A new and invigorating feeling was in the air. Summer days at Christchurch can be hot, but the city and its environs rarely or never experience the subtropical weather that is characteristic of low-altitude areas in North Island. At any rate, we were particularly favored, for the days were both sunny and crisp, and at night the temperature even fell slightly below frost. The sparkling air and fresh breezes of morning were well calculated to arouse the time-honored Anglo-Saxon sentiment, "It's a fine day; let's go out and kill something." Suffice it to say that this urge was sublimated by scientific debate!

Of the Zoology Section of the congress, faithfully attended by the writer at both Auckland and Christchurch, space will permit reference to only a few aspects that have special bearing upon the New Zealand region.

Dr. G. E. R. Deacon, of the British *Discovery* Committee, set the stage with a paper on "Surface Boundaries in the South Pacific Ocean." This treated of the more or less fixed lines known as "convergences," where circumpolar bands of antarctic, subantarctic, subtropical, and tropical water come into contact, delimiting zones comparable with the better-known climatic belts on the continents. Deacon elucidated the hydrostatic origin of the convergences, based upon the prolonged investigations of the *Discovery* and other recent oceanographic expeditions.

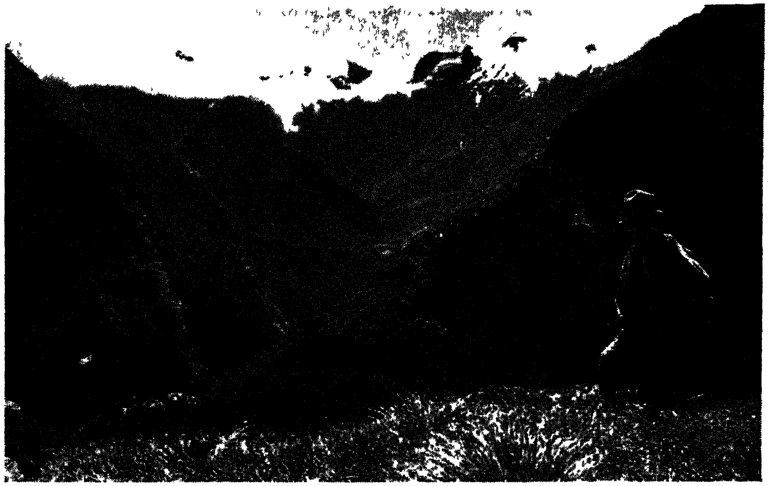
The term "boundless deep" is a purely poetic concept. We now know that sea water is not homogeneous, like water shaken in a bottle; on the contrary, it is stratified and stable. Masses of differing temperature and salinity do not mix readily, even at the wind-blown surface. They retain their separate physical and chemical characteristics, which limit in turn the distribution of marine organisms. The control is, indeed, as thoroughgoing as that imposed on the land by barriers such as mountain range, river, "bush," or desert.

It so happens that the convergence separating subantarctic from subtropical ocean water in the Pacific cuts across New Zealand near the latitude of Cook Strait. The effect of the two zones upon animals and plants living within the sea, and also upon aerial creatures such as the marine birds that feed upon them, has long been familiar to Dominion naturalists. Dr. Deacon's report was therefore followed by lively commentaries, in the course of which new information was brought to light.

If there is science in the modern exploitation of whales—sometimes incorrectly called an "industry"—it surely belongs in the applied category. Nevertheless, the companies engaged in whaling operations can render priceless service to scientific investigators. In the long run, this is also likely to redound to their practical benefit. A good illustration was offered in the report by Mr. W. H. Dawbin and Dr. R. A. Falla on the life history of the humpback whale, based upon studies made at the commercial station in Tory Channel, New Zealand. Inquiry into the migrations, probable routes, population, ratio of the sexes, time and rate of reproduction, growth, and the alternating periods of feeding and abstinence yielded enlightening data for integration with findings in other areas of the world ocean.

A related though more general study by Dr.

N. A. Mackintosh, of the *Discovery* Committee, revealed that the humpbacks, unlike other whales, tend to segregate themselves in several restricted antarctic areas. There is little doubt, Mackintosh believes, that the humpbacks captured in New Zealand waters come from Area V in the far south, that is, from the waters between longitudes 130° E and 170° W. The pelagic whaling factories have thus far scarcely touched this Pacific sector, a fact which may presage a satisfactory picture for the New Zealand catch if it is conservatively handled.



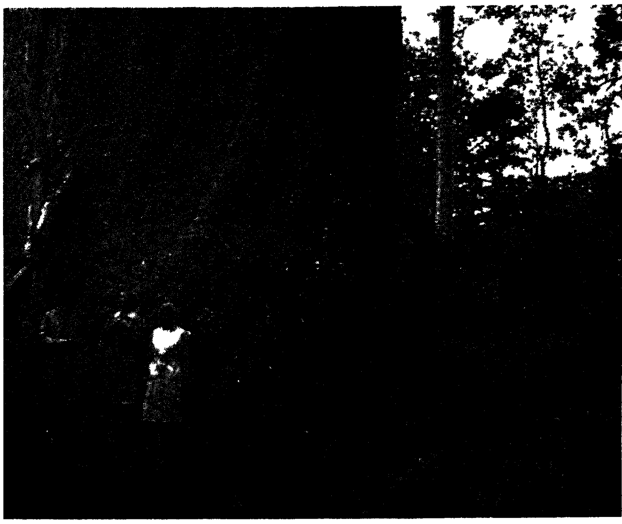
The author on a three-day hike from Milford Sound through the alpine pass to Lake Te Anau, known to New Zealanders as "the finest walk in the world."

Overseas migration of birds was the subject of several contributions of a more maritime than terrestrial nature. It was discussed in papers read on behalf of two Americans, Dr. Rollin H. Baker and Dr. Ernst Mayr, neither of whom was able to attend the congress, and in another report by Dr. D. L. Serventy, of Australia.

Large numbers of northern Asiatic birds winter each year among the islands of the Pacific. Every New Zealander is acquainted with the godwit, for instance, or at least with the stories and legends applied to it. The more distant an island is from the coast of Asia, the fewer wintering species it possesses. A less spectacular annual migration, but one that still involves more than twenty-five different kinds of birds, proceeds from New Zealand and Australia northward. Both these types of migration can be explained on the basis of orthodox theories in combination with the familiar phenomena of route abbreviation and prolongation. There is no advantage to be derived, in Mayr's opinion, from dubious geological hypotheses, such as that of continental drift. Migration is necessarily one of the products of evolution, and flights to tiny and isolated atolls of the central Pacific, such as are made by the New Zealand long-tailed cuckoo, can be readily comprehended if it is assumed that such islands are remnants of once more extensive volcanic archipelagos, now submerged. Such a "stepping-stone" theory is far more credible than one based upon a Pacific continent that has subsided beneath the waters.

A notable place in the program of the congress was taken by women. The greater proportion of these, naturally, were New Zealanders, but Australia, the East Indies, Hawaii, the continental United States, and several European countries were likewise represented. Since yet unutilized oceanic food resources received so much attention, a large assembly heard the presentation of a paper on "Problems in Fisheries Management for Pelagic Schooling Fish," by Dr. Frances N. Clark, of the California Bureau of Marine Fisheries. Another American woman who took effective part in the discussions was Professor Carey D. Miller,

Hunting the long-departed. A glimpse of excavations for moa skeletons at the famous Pyramid Valley Swamp in North Canterbury.



Te Matua Ngahere, "the father of the forest." This tree, fifty-three feet in circumference, was believed to be the largest surviving *kauri* in the Waipoua State Forest. Recently, however, a still bigger one has been discovered.

In a latitude corresponding with that of Montreal, the New Zealand "bush" resembles a tropical rain forest, with abundant lianas, orchids, and cryptogams, including an amazing wealth of ferns

of the Department of Nutrition, in the University of Hawaii.

AFTERTHOUGHTS

It was a source of surprise and regret that the Latin-American republics bordering upon the Pacific sent no scientific delegates, but confined their representation to the nominal appointment of diplomatic officers. This is hard to understand in view of the ready participation of such countries as Chile, Peru, and Mexico in similar scientific conferences within the Pan-American region and in Europe. The feeling was expressed that Peru, which has developed the largest undertaking in the world based wholly upon the conservation of wild animals—i.e., the guano industry—would have had much to add to deliberations on natural resources and agriculture. It is hoped that means may be found for persuading our southern neighbors of the mutual advantages to be derived from sending delegates to the Eighth Congress in the Philippines.

There was something peculiarly warming and altruistic about our experience in New Zealand. As Dr. Falla remarked, monographs, textbooks. and names suddenly became faces and voices and handclasps. An international political meeting, however statesmanlike and well-intentioned, is likely to be bedeviled by nationalism and undue watchfulness. Here among the scientific men and women there was no reigning idea other than the free exchange of data and the pursuit of truth, all in relation to the betterment of the world in which we must somehow contrive to get along together.

The congress, by the way, was perhaps the first general scientific meeting at which problems of human population in relation to the ultimate possible productivity of soil and sea were squarely faced. In the memorable symposium on "The Social Implications of Science," as well as in various sectional meetings, present and impending population trends of man—by far the most abundant, and the only rapidly increasing, species of large vertebrate animal—were stimulatingly discussed.

Press coverage in New Zealand was accurate, dignified, and sympathetic, in both the news and the editorial columns. If the sideshows of journalism, such as cartoons, quips, and verse, are sometimes a better gauge of genuine popular interest than even the best of reporting, the congress fared well on that score also. We were all the happier for the compliment of a little good-natured lampooning. We had a glorious time; we shall not forget what New Zealand has given us of both information and inspiration, and of bountiful hospitality.*

* Reports on the congress already published include the following:

Sir Norman Haworth. Science and Development in the Pacific Area. *Nature,* March 26, 1949, 163, 469–72.

R. C. Miller. Flight into Tomorrow. *Pacific Discovery,* March–April 1949, 2, 18–21.

Grace E. B. Murphy. Scientists Scan the Unknown Pacific. *New York Herald Tribune* (editorial page), January 31, 1949.

Grace E. B. Murphy. The Pacific Science Congress, *idem,* March 13, 1949.

K. A. Ryerson. The Seventh Pacific Science Congress. *Science,* May 27, 1949, 109, 529–33.

MAN'S MOMENT

Leslie A. White

Midnight,
Vast vapors spread through space,
Infinite, thin, nebulous.
Dawn approaches,
The Cosmos stirs in her sleep.
The vapors sigh, coagulate.
Clouds gather, shift, and part;
Galaxies begin slowly to revolve,
Their spiral arms closing
In rotational embrace.
Heat and light and rays unknown
Pass like whispers from star to star.

Morning comes,
And on a tiny speck,
An infant of a modest star,
A ferment starts.
Swimming about, then crawling;
Climbing and chattering in the trees;
Through a bright morning hour
Man struts about,
Wondering, building, fighting,
Viewing the Cosmos through himself:
"All this for me!" he cries.

Morning wanes,
Civilizations grow old and die.
Man comes of age at last,
But as his grasp extends
He clutches ever less.
His conquest of the Earth
Robs him of his mastery
And leaves him but a tenant
For a few moments longer.

At noon the stars are old.
Some have died, their lights gone out;
Sidereal convulsions, galactic tremors,
Tell of time and change.
No one noticed when Earth ceased to be,
Disintegrating into cosmic dust,
For long ago its Little Tenant
Had laid himself down in sleep
Deep and never ending.

Dusk and long waves
Spread throughout space and time,
Changing here, arranging there,
A never-ending flow
On and on through cold and darkness
Toward a rebirth and a new dawn
Or to eternal death.

STRIP MINING AND LAND UTILIZATION IN WESTERN PENNSYLVANIA

E. WILLARD MILLER

Dr. Miller (Ph.D., Ohio State, 1942) taught geography and geology at Western Reserve University in the Army Air Corps program in 1943. Later he did map intelligence work in Washington for the Office of Strategic Services. Since 1945 he has been chief of the Division of Geography at the Pennsylvania State College.

STRIP mining, the process of removing an overburden of rock from an underlying mineral resource, is one of the oldest mining methods. Open-pit mining for such minerals as iron ore, limestone, and slate has been practiced for hundreds of years, but the mining of coal by stripping is a relatively modern phenomenon. The development of this type of mining, which requires the movement of tremendous quantities of earth as a result of the relative thinness of coal seams, became important only after the development of the large power shovel. With this advancement coal strip mining is a comparatively simple operation.

Bulldozers first remove the weathered surface materials, and then large power shovels or draglines remove the bedrock. The largest of these draglines in western Pennsylvania has a capacity of 40 cubic yards and can move overburden to a thickness of 70 feet. Strippers indicate they can remove 15 feet of overburden for each foot of coal, the amount varying somewhat with the type of bedrock encountered. After the overburden has been removed, the coal is mined by smaller shovels and loaded into trucks, which normally haul it to the nearest railroad.

The advantages of strip mining over subsurface operations are many. One acre of coal, 3 feet thick, will produce by stripping operations approximately 5,000 tons, whereas in underground mining the average recovery will be about 3,300 tons. Eighty to nearly 100 percent of the coal can be recovered by surface methods, whereas underground mines recover only 40–60 percent of the seam being mined. Strip miners use larger units of machinery than underground miners, so that in 1948 the average surface miner produced nearly 15 tons of coal per day compared to a little over 6 tons for the subsurface worker. Investment in stripping machinery is high, but the salvage value is much greater than that of underground equipment, and in strip mining the interval between initial investment and full production is comparatively short. As a result of minimum danger in open-pit operations, insurance for the surface miner is 2.1 percent per hundred dollars of pay roll; for subsurface workers it is 4.3–4.8 percent. The strip mine operators are unhampered by traditional methods and techniques.

There are also several disadvantages to surface mining. The outcrop of the coal seam suitable for mining is frequently irregular, and if the overburden becomes too thick mining operations must be abandoned. In contrast, coal which is exposed on the surface is weathered and consequently has a low B.T.U. value and usually a high ash content. The greatest problem of strip mining, however, is the restoration of land after the scrappers and power shovels have worked to uncover the coal. Although the spoil banks offer opportunity for reclamation for a variety of uses such as forestry, pasture, nut and fruit crops, wildlife, and recreation, they have commonly remained areas of complete abandonment.

TRENDS OF STRIP MINING IN WESTERN PENNSYLVANIA

Strip mining of bituminous coal became a significant part of the industry about thirty years ago in the Midwest, but coal stripping operations in the Eastern fields were small until the beginning of World War II (Fig. 1). The Pennsylvania bituminous strip mining production averaged less than 750,000 tons annually until 1938. A number of factors retarded the development of surface mining in the Eastern fields. Pennsylvania is one of the oldest centers of coal mining in the United States, and underground mining facilities are highly developed. Also, from 1920 to 1939 coal production of the United States did not exceed that of the World War I period, so that there was small need to develop the near-surface coal seams. During this period the stripped coals, frequently mined by the small producer with little or no equipment to clean and size the coal, were usually

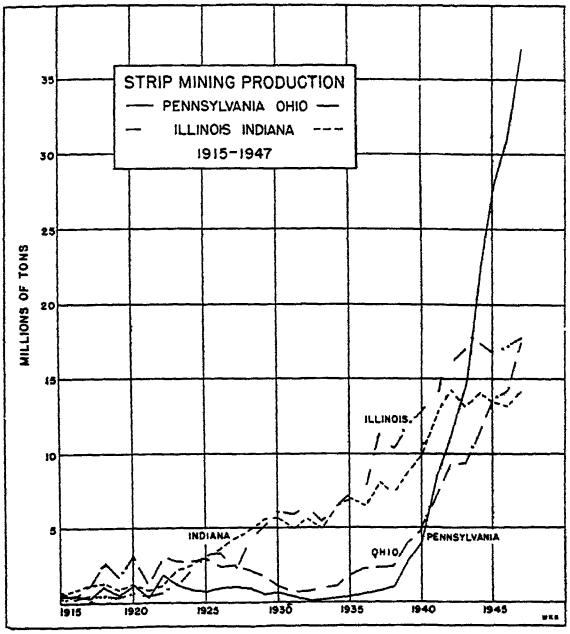


FIG. 1. Strip mine production of bituminous coal in four leading states. Note the significant rise in output in Pennsylvania since 1940.

of low quality and not accepted by the general public.

As a result of the gradual depletion of some of the readily accessible underground coals, the growing demands for fuels in World War II, and the decreased number of underground miners, the Pennsylvania coal producers began to exploit the near-surface coals. Production increased steadily from an output of 2,792,000 tons in 1939 to a peak of 37,075,145 tons in 1947. As a result of the declining domestic and foreign market in 1948, strip mine production decreased to 33,483,000 tons. In 1944 Pennsylvania surpassed Illinois as the principal producer of surface coals and is now far in the lead. At the present time Pennsylvania produces 32 percent of the nation's stripped coal and has 44 percent of the total number of strip pits. The state also has 37 percent of the workers and 40 percent of the power shovels engaged in stripping operations. One out of eight coal miners in Pennsylvania is now working in strip mines.

The percentage of coal mined by stripping in the state has consequently risen remarkably. Until 1938 open-pit output was less than 1 percent of the total. Stripped coal increased to 8 percent of the output in 1942, and only two years later 16 percent was from strip pits. The 1948 stripped coal production was nearly 26 percent of the total bituminous coal output of the state. It has only been through surface mining that Pennsylvania has maintained its high coal position in the nation in recent years.

Strip mining did not develop uniformly in western Pennsylvania (Fig. 2). On a volume basis Washington County leads with a production of 5 million tons annually, followed by Allegheny, Clearfield, Clarion, and Cambria counties, each with between 2 and 4 million tons. Seven counties have over a million tons production annually, and 16 counties have an output varying from 1,000 to 500,000 tons.

The growth and relative importance of strip mining in each county is shown in Figure 3, which compares the percentage of bituminous coal produced by strip mining in 1941 and 1947. Although strip mine production was greatest in both years in Washington and Allegheny counties, the percentage mined by surface operations was small because of large underground output. Strip mining gained its greatest relative importance in the northern counties at an early date and has strengthened its position up to the present. Surface mining, because of its lower operational costs, can mine coals which would not be economically feasible for underground mining. The average thickness of stripped coal decreases northward in Pennsylvania (Fig. 4). The average thickness of surface

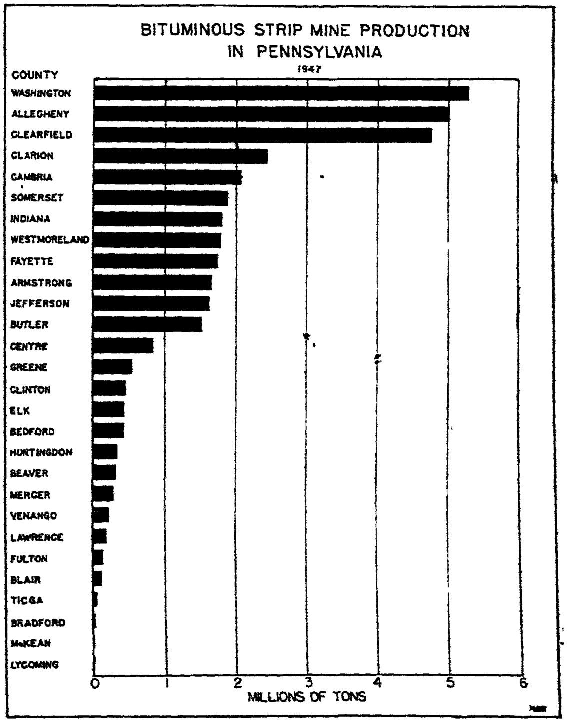


FIG 2. The bituminous strip mine production by counties in Pennsylvania in 1947.

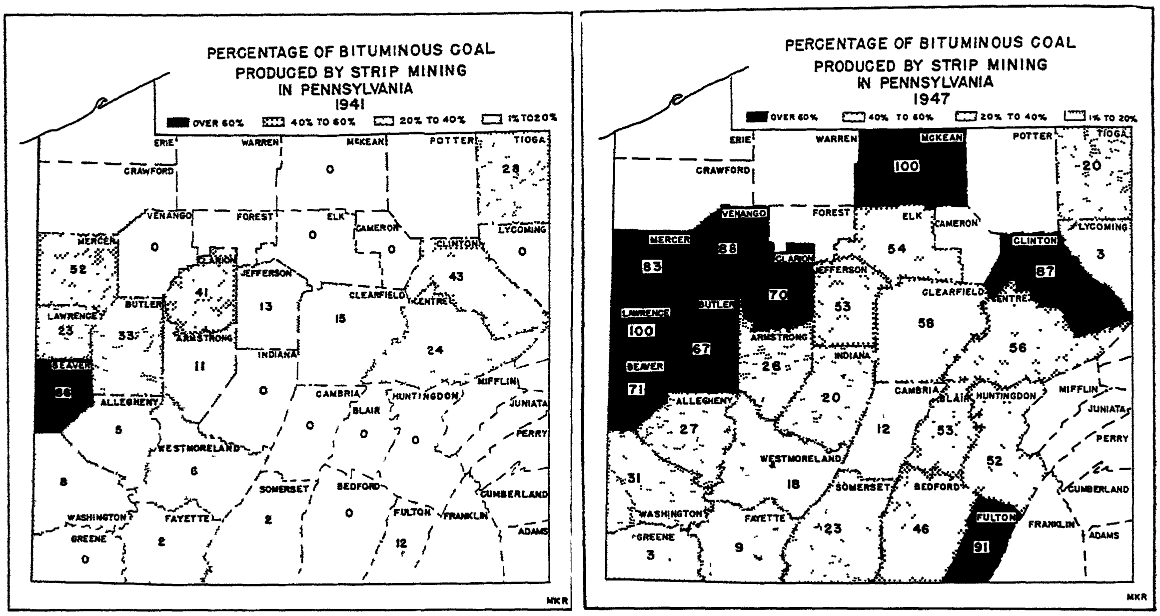


FIG. 3. A comparison of the relative importance of strip mining on a county basis in 1941 and 1947.

coals in the southwestern counties varies from 60 to 73 inches, whereas the average thickness is but 35–50 inches in the northern counties. Coal seams as thin as 12 inches are being stripped in several sections. As a result of the development of efficient stripping technology, coals are being mined which were previously submarginal. Since 1941 underground mining has declined relatively and strip mining has expanded in essentially all coal-producing counties in western Pennsylvania.

PROBLEMS RESULTING FROM STRIP MINING

To the average citizen the most striking feature of strip mining is the destruction of the scenic beauty by the development of unsightly spoil banks (Fig. 5). Because there are usually numerous small operations in an area, possibly affecting not more than 5–8 percent of the land, the scenic beauty of the entire landscape will be marred. It is a common experience to drive along roads for miles in western Pennsylvania and never be out of sight of abandoned spoil banks Many areas, mined as long as thirty years ago, are today only partially covered with low thorn brush, which is not only unsightly but has no economic value. The continued expansion of the spoil banks affects the tourist trade of the state directly, for this trade is based primarily on maintaining an unimpaired natural environment. In recent years the tourist trade has added more than $500,000,000 to the gross income of the state. If the natural beauty of large sections is marred, one of the largest industries of the state will gradually decline.

Since stripping operators select, whenever possible, the least rugged areas and those which have been cleared of trees, the destruction of productive farmland is a major consideration in the loss of future productivity to both the individual and the

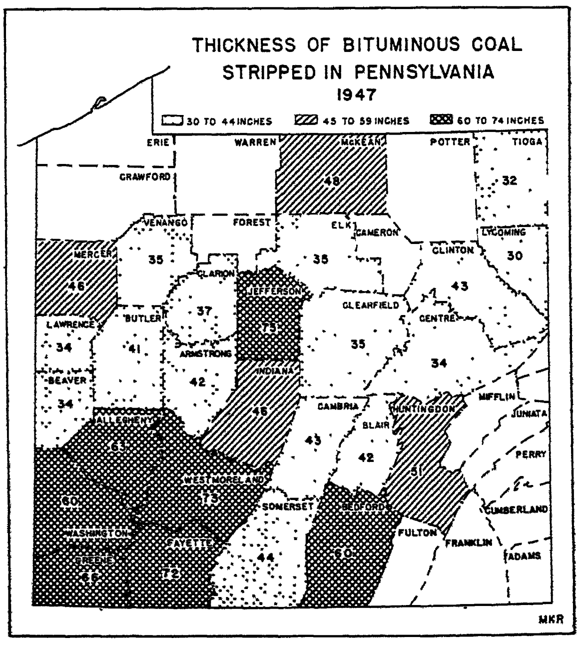


FIG. 4. The average thickness of bituminous coal stripped in each county in western Pennsylvania in 1947.

state (Fig 6). It is common practice for the coal stripper to justify his operations by the statement that 85 percent of the land mined has been classified as agriculturally submarginal and that consequently he has no responsibility for its reclamation. There is, however, essentially no land in western Pennsylvania that will not produce timber. It is also mistakenly thought that turning the soil over may add to its productivity, and this might be true if stripping were limited to 2 or 3 feet of the surface. With modern stripping extending to depths of 30–70 feet there is little but bedrock on most spoil banks. However, as long as profits obtained per acre by the farmer are 100–300 times the agricultural income of a single year, farmland will continue to be sacrificed with little consideration for its reclamation.

Erosion of the exposed raw earth in spoil banks and, particularly, of leveled areas that are on a slope with no vegetation to hold the ground is another grave problem (Fig. 7). It is common to find gullying 6–12 inches deep on newly leveled areas. Since many of the spoil banks are on the higher slopes, the raw earth has been carried over lower agricultural lands, thus destroying their productivity. Many of the small streams have become highly silt laden, with the consequent killing of fish.

Another problem has resulted when the trench made by the last cut has not been filled. If the ditch between the last spoil bank and the high wall is allowed to remain open it normally fills with water. The water which seeps out of the coal seam usually has a high iron and sulphur content and is consequently acidic. When this water runs into streams, as it often does, it adds to the destruction of fish. The excessive seepage of water out of the horizon lowers the general water table of the mining region, and as a result many shallow springs have dried up in recent years. The high wall, which may be as great as 70 feet, is also a hazard in the area, for there is usually no guardrail to prevent animals or individuals from falling over the cliff. It is particularly dangerous to cattle. Several people have received fractured limbs as a result of falls.

Many of the secondary and some of the primary roads of western Pennsylvania were not constructed to carry heavy truck traffic. The weight of the load carried by coal trucks varies from 3 to more than 20 tons. Because of the extraordinary wear imposed by such loads the surface of many of the roads is broken and pitted with holes. Although many of the coal operators keep road crews and graders continually at work maintaining haulage roads from the mine to the public highways, the state roads have deteriorated in some sections so that they can hardly be used. The Department of Highways is now limiting the load on most roads to less than 10 tons, and state police are rigorously enforcing this regulation.

PATTERN AND EXTENT OF STRIP MINING

What is the pattern of strip mining, and how large an area is mined each year are foremost questions which must be answered before an effective reclamation program can be inaugurated (Fig. 8). As a result of stream erosion in the Appalachian plateau, the surface coals usually outcrop near the top of the slope. The mining operation begins at the outcrop and extends into the hillside until the overburden becomes too thick. Thus, strip mining normally follows a given contour around the slope. It is common for a stripped area to be less than a hundred feet wide and possibly a mile long. A single strip pit will cover from as little as one acre to—rarely—more than 50 acres. In recent years there have been nearly 1,000 power shovels operating between 600 and 700 strip pits. Since the beginning of intensive operations in 1939, and extending through 1948, there have been approximately 38,000 acres strip mined in the bituminous coal fields of Pennsylvania. At the present rate of production and technological advancement, it is estimated that strip mining of coal in Pennsylvania will continue for at least another twenty-five years.

RESTORATION OF STRIPPED LAND

Until 1945 reclamation of strip mined areas in western Pennsylvania was accomplished by individuals, companies, or by local government regulation. The program was entirely unorganized, and the total amount of land restored was very small. For the most part private individuals did little to restore stripped areas, although in isolated instances the better farmers stipulated in their contracts with the miners that the topsoil be removed by bulldozers and then replaced after the mining operation was completed. This method preserves the good agricultural lands, but it lowers the individual's profit and has not developed as a standard reclamation method in strip mining. The common practice was for farmers to plant a row of trees along the edge of the stripped areas which face public highways or the farmhouse in order to hide the devastated land, and the remainder of the land was left for nature to restore.

A few of the larger coal companies, which have long-range mining programs, restored most of the

land they stripped. One company, operating near Burgettstown in Washington County, leveled spoil banks to less than a 20 percent slope. On about 100 acres the topsoil was removed and, after mining, replaced and the area seeded with a mixture of clover, alfalfa, and red top grass. On another 120 acres coniferous trees were planted. In this same section a lake 0.5 mile long by 0.25 mile wide and 25 feet deep at the brest was constructed in 1943 and is now stocked with fish. The tree growth has been good, so that it has become a district recreational center. On another stripped area near Pittsburgh a dairy farm was established on reclaimed land to supply dairy products to miners in the area; still another company has constructed a private airfield, with runways 1,800 and 2,200 feet long. A number of the companies plan to utilize the timber produced on the stripped areas in their underground mines. Unfortunately, strip mining has been largely developed by small producers, who are primarily interested in securing a maximum profit. It is estimated that not more than 300 acres were restored by larger corporations prior to 1945. These early reclamation projects do prove, however, that a profit can be secured when complete recovery of the land is a part of the program.

Under powers granted by the state, local governments may establish zoning laws to control stripping operations. Penn Township in Allegheny County is the only area that has taken advantage of this means of regulation. In 1940 the township was zoned into residential, business, and industrial districts. Most of the strippable coal is located in restricted residential sections. A zoning Board of Adjustment, appointed by the Township Commissioners, grants a permit to remove coal as a variance from the zoning regulations after application from the owner of the land is approved. The permit is granted to the owner of the land and the coal stripper that he designates. In order to guarantee effective reclamation, the township requires a bond of $1,000–$2,500 for every acre to be stripped. The township solicitor has ruled that in the industrial zone a stripper may mine without a permit, but bond must be posted to assure restoration of the land. Since 1940 all stripped land has been leveled to its original contour and reseeded in grass. Five companies have strip mined coal in Penn Township and have reported a fair profit on their operations, and none have forfeited their bond. Penn Township is the sole example in Pennsylvania where a large area of stripped land has been completely reclaimed.

A unique example of reclamation was developed by the community of Grove City, which in 1932 took 225 acres that had been stripped in 1918 and developed a community park (Fig. 9). Trees were planted and drives were laid out. On four acres picnic sheds, a swimming pool, a bath house, a children's playground, and tennis courts have been constructed. This work was done largely by the

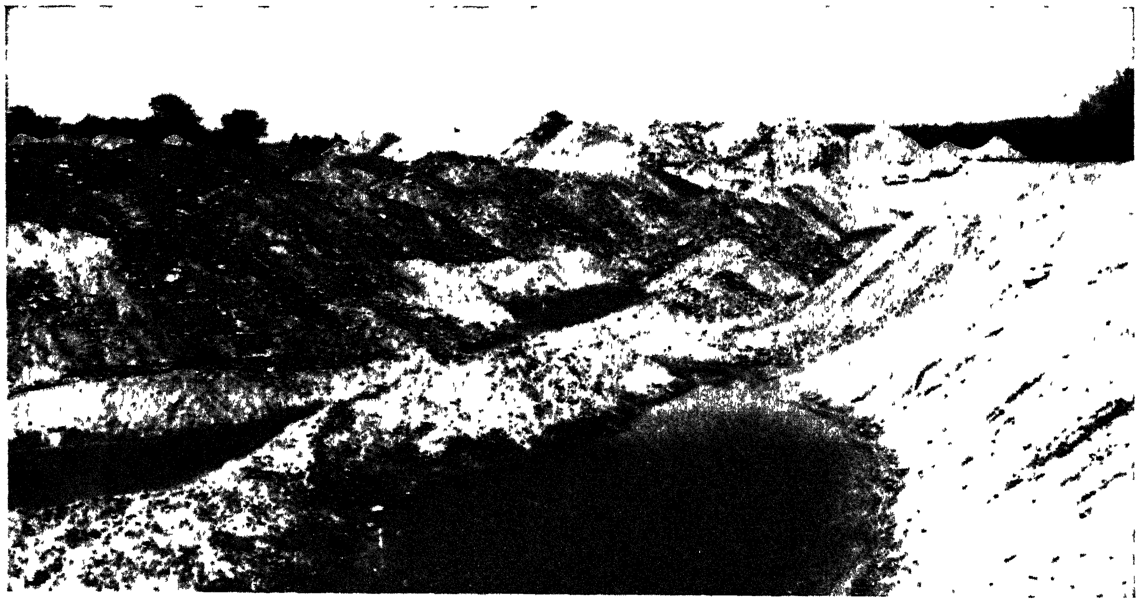


FIG. 5. An abandoned spoil bank in Mercer County, Pennsylvania. This bank is three years old, and no vegetation has yet appeared.

FIG. 6. Spoil banks around a farmhouse in Clarion County. Productive land in foreground.

Civil Works Relief Administration and cost approximately $40,000. It is now the most attractive community park in western Pennsylvania. Other cities could well follow this example, for many of the spoil banks are on the outskirts of towns.

When reclamation of stripped land was left to the discretion of the individual operator or local regulations, the restoration program was highly ineffective. It is estimated that not more than 3–5 percent of the total area stripped by 1945 was restored and is now productive.

As the area of spoil banks increased rapidly during World War II, demands were made by conservationists, certain newspapers, farmers, and a few progressive citizens to develop state control over reclamation of stripped land This led to the passage of the Bituminous Coal Open Pit Mining Conservation Law in 1945. The law requires that each miner planning bituminous stripping operations must register with the Department of Mines, deposit a filing fee of $100 for each stripping operation, and post a bond of $300 per acre to be stripped, with a minimum bond of $3,000. Liability under the bond is for the duration of open-pit mining at each operation, and for a period of five years thereafter. Within thirty days after starting the removal of overburden the miner must file an operation report giving the location, description of the tract, and the name of the landowner.

The act also requires each strip mine operator to cover the exposed face of the unmined coal within one year after completion of mining, and to level and round off the spoil banks sufficiently to permit the planting of trees, shrubs, or grasses. The slope of the leveled areas is not to exceed 45 degrees. After the leveling is completed, the miner must plant the stripped area to the satisfaction of the Department of Forests and Waters. If the operator fails to comply with the regulations of the law, he forfeits all or part of the posted bond. The

FIG. 7. Erosion of a newly leveled spoil bank when there has been no attempt to plant grass or rye or cover with a layer of mulch until trees hold the ground in place.

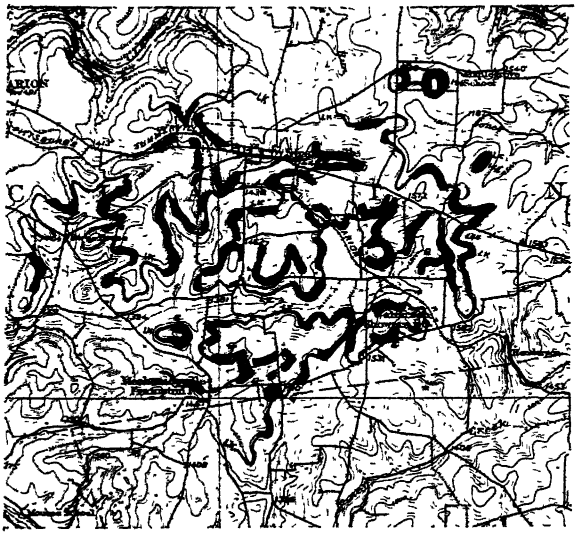


FIG. 8. The pattern of strip mining in a section of the Clarion Quadrangle in Clarion County, Pennsylvania. The blacked-in portions are stripped areas. The map represents an area approximately five miles square.

first bonds will be forfeited in May 1950, which will end the initial five-year period since the passage of the law. It now appears that a large percentage of the bonds issued since 1945 will be forfeited, and then it will be the responsibility of the Department of Forests and Waters to restore the productivity of the land.

The conservation law was strongly attacked by many of the bituminous strip miners. The Central Pennsylvania Strippers Association was organized to protest the passage of the conservation law, and in 1946 contested its constitutionality in the Dauphin County Court. The strip miners based their attack on the leveling of the spoil banks and the payment of a registration fee. The court decision was against the miners, and the validity of the law was upheld.

The reclamation of stripped land presents difficulties not previously encountered in the revegetation of an area. The average coal miner has no real interest in the recovery program, so if it is to be successful a simple, direct technique must be evolved. Numerous questions, such as the best method of leveling the spoil banks, type of trees, shrubs, or grass to be planted, erosional control until a cover crop develops, and length of weathering period in order that the raw earth will support a satisfactory vegetation growth, have to be answered to assure satisfactory reclamation. Since 1945 a number of field research programs have been initiated by the state. Preliminary studies by foresters, geologists, and geographers indicate that stripped areas differ widely in their ability to support vegetation, owing to such factors as variations in the physical and chemical composition of the rock debris, the differences in slope, and the quality and quantity of available water supply.

In the coal regions of western Pennsylvania the greater portion of the overburden of the surface coals consists of shale, a compacted clayey rock which does not become plastic when wet (Fig. 10). It is weak mechanically and slowly weathers into a clay, thus gradually providing soil material. In many districts sandstone layers are encountered. Sandstones are mechanically much stronger than shales and in spoil banks may form a jumble of boulders that resist weathering for very long periods. When limestone or limey shales occur in the overburden, these tend to neutralize the acidic waters and thus aid vegetational growth.

The spoil banks usually contain a number of minor materials that retard plant growth. Pyrite, an iron sulphide, is commonly found near the top of the coal seams and in the "boney" parts of the bed. Consequently, in the stripping operations, when the earth is overturned the pyrite is normally deposited near the surface of the spoil banks. When exposed to air and moisture these particles decompose to iron sulphates and on further oxidation and hydration form rusty iron oxides and sulphuric acid. The acid, though hastening the weathering process, hinders plant growth Soluble alumina formed by the reaction of acid on shales likewise is harmful to plants. Where pyrite is abundant a much longer period of weathering is needed before planting is desirable.

The quantity, quality, and distribution of the spoil bank waters are also important factors in revegetation. The irregular surface of the leveled areas may have numerous clay depressions where water collects for long periods after rains. In these places trees may be completely covered for several weeks and are usually drowned. In dry periods these clay depressions usually become hard pans; in other areas the material may be exceptionally porous and the surface will become dessicated quickly. The acidity of the water will depend on the rapidity of weathering and will usually vary from a *p*H three to six.

The slopes of the raw spoil banks and leveled areas vary considerably. It is common to have slopes as great as 40 percent; if the slope exceeds 50 percent the survival of trees is low. The Conservation Law requires no preparation of the spoil bank beyond leveling, so that fertilizers are very seldom applied before planting.

Under such physical conditions some type of coniferous trees has usually been found to be most

practical (Fig. 11). The red, Banks', shortleaf, and pitch pine, Norway and white spruce, and hemlock are commonly used. Although the growth of coniferous seedlings is generally satisfactory, they do not stabilize spoil bank surfaces quickly, and thus the natural regeneration of hardwood species is retarded. The shortleaf and pitch pines are particularly suitable for sandstone and acid shales. If the soil is highly acidic, Japanese larch is one of the better trees to plant. In a number of places where the earth is moderately acidic, red oak and black locust have been successfully grown; black locust is particularly satisfactory if a quick vegetational covering is desired. It also develops a surface litter of 2–3 inches in six to nine years, stabilizing the spoil bank. Besides stabilizing the earth and modifying the highly fluctuating soil moisture and temperature of the bare surface, the litter affects the beginning of soil formation. The black locust also has an abundance of nitrogen-fixing nodules on its network of fibrous roots, which stimulate plant growth in the soil. Normally, after about ten years, a luxuriant covering of herbs, shrubs, and hardwood reproduction develops as an undergrowth. The planting of this tree should be given greater consideration.

In western Pennsylvania most plantings have been of a single species. In general, mixed plantings of several species would be more desirable because of the greater protection from insect and disease attacks, and the benefits of site improvement. Furthermore, if the season of planting is less favorable to one species than to another, mixed stands offer less likelihood of complete failure. The trees for reclamation projects may be obtained from a state or private nursery, and both seedlings and transplant stock are used. If seedlings are used, the root system and the top should be well developed in order to survive. The transplanted stock is found to be superior, but it is more expensive—and reclamation costs are usually kept at a minimum. Early spring or late fall is the best time to plant trees. After planting, exposure to strong, cold winds and complete covering of snow for long periods reduces the survival rate.

There has been considerable disagreement among conservationists as to whether spoil banks should be leveled before restoration or reclaimed in their original state. A common Midwestern viewpoint has been that leveling the spoil banks with a bulldozer compacts the earth to the extent that it greatly hinders plant growth. It has also been argued that leveling the ridges prevents the miner from making a profit. The evidence to support either of these contentions is largely lacking in western Pennsylvania. If the spoil banks are not leveled it is extremely difficult to traverse them because of the steep slopes. The original spoil banks are also likely to be more porous and consequently so dry that plant growth is almost impossible to maintain. Average profits from strip mining operations have been exceedingly high, so

FIG. 9. The community park at Grove City, Pennsylvania A reclaimed strip-mined area.

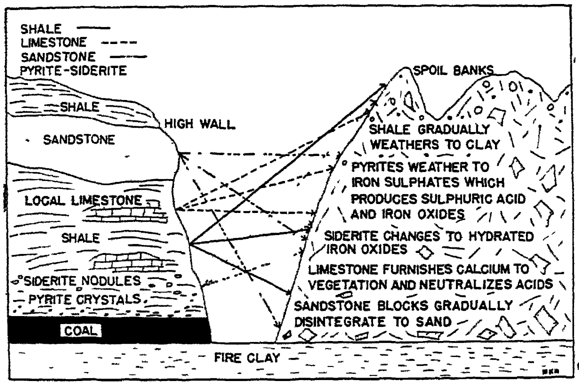


FIG. 10. A diagrammatic sketch of bedrock in the coal region and the location of debris in the spoil banks.

that restoration practices should become a part of normal mining costs.

What is the best method of leveling the spoil banks has been another significant problem. At the present time, since most operations have taken place on a slope, the miner pushes the earth downhill and rarely destroys the high wall. This type of leveling could be greatly improved. The high wall should be destroyed in order to rid the region of a hazard. When the banks are leveled outward from the last furrow, a considerable amount of land which was not affected directly by the mining operation is covered by raw earth. A more desirable method of leveling would be to push the earth into the last furrow made by the mining operation. By this procedure, the trench is filled and the raw earth is limited to the original area of mining.

Present strip mining machinery has been developed specifically for mining coal, with little thought of reclamation. With traditional methods, most of the topsoil is deposited on the bottom and the rocky material on the top of the spoil bank. Two types of excavators have now been developed which would save the topsoil and partially level the spoil banks during the mining operation. The first is a dragline with a 215-foot beam which precedes the stripping shovels. This dragline removes the topsoil and deposits it between the two preceding spoil banks. The ridges left by the stripping shovels are covered, and the intervening valleys partially filled with topsoil. In Illinois a new type of wheel digger and conveyor is now being tested in the field. A large revolving wheel about 30 feet in diameter, with buckets attached to its outer edge, is pressed against the perpendicular face of the high wall of topsoil to be removed. The buckets dump the soil on a conveyor belt, which carries and deposits it over the two preceding spoil banks and their intervening valleys. With either of these excavators the revegetation of the banks is greatly simplified. Neither of these machines has so far been used in western Pennsylvania.

As the reclamation program has developed, a number of practical weaknesses have appeared in

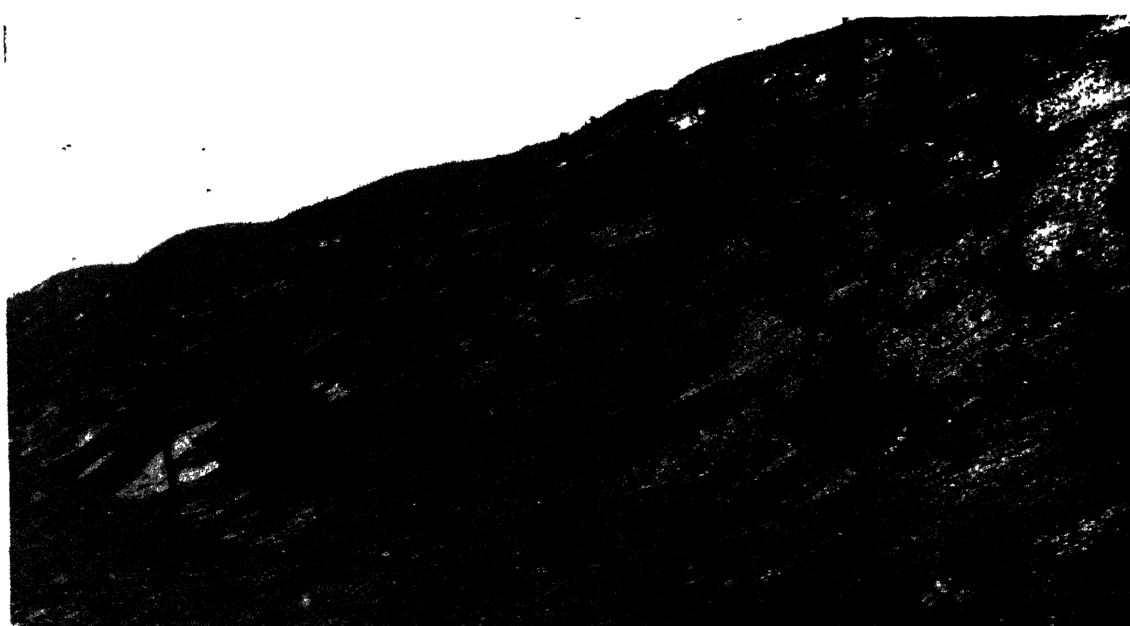


FIG. 11. A spoil bank partially reclaimed in red pine. Growth has been about 8–10 inches a year. Slope of the bank averages 40 percent and the pH rating of the shaly soil is 4–5.

the Conservation Law, which reduces the possibility of complete restoration in a minimum length of time. The present law requires that complete reclamation be finished within a year of the end of mining. This regulation gives little time for any weathering of the spoil bank area to reduce the acidic condition or develop a better soil. This appears to be the single greatest defect of the present law. From field observation during the past five years it would seem more desirable to level the spoil banks immediately upon completion of the mining operation and then allow a period of time to elapse before planting trees. Stripped areas which have been planted three or more years after the mining operation normally have an 80–95 percent growth of trees, whereas trees planted immediately on the exposed raw earth have a 10–90 percent growth, with an average growth of about 40–60 percent. Erosion of the earth in the stripped areas should be controlled from the time leveling is finished until a vegetation covering holds the loose soil in place. This can be done fairly effectively and inexpensively by scattering a layer of mulch such as straw or leaves on the earth; where possible, the planting of rye, grass, or a shrub covering will advance the restoration. In order to implement these proposals the present law needs amending. The strip miner, who has available heavy machinery such as the bulldozer, should level the spoil bank immediately on completion of the mining operation, and then pay the Department of Forests and Waters a sum sufficient to cover the stripped area with a mulch and later to plant it in trees. Most strip miners favor this policy, for they are fundamentally interested in mining coal and not in reclamation work. The owner of the land would also gain, for there would be few failures. At the present time, if the land is planted by the miner and the work approved by the state, and the trees die, the owner must replant at his own expense. There is little indication that many present owners will replant the land.

CAVITATION

At Northwestern University's Technological Institute civil engineers are studying cavitation in flowing water. Turbine blades and walls of conduits in dams have been torn away by the continued occurrence of cavitation, and ship propellers have had to be repaired or replaced as a result of the violence of exploding bubbles of water. Wallis S. Hamilton, associate professor of civil engineering, and two of his graduate assistants, will determine, by means of newly designed research apparatus, which materials can withstand cavitation.

"By changing the curvature of the walls, using materials found to be the most impermeable, and directing bubbles away from the walls before they collapse," Professor Hamilton said, "we hope to design machinery which will operate without damage, even though cavitation occurs."

Researchers at California Institute of Technology are using high-pressure water tunnels and high-speed motion picture cameras to determine how damage by cavitation occurs.


SOME CONSIDERATIONS OF THE BIOLOGICAL EFFECTS OF DDT

C. H. HOFFMANN and J. P. LINDUSKA

Dr. Hoffmann (Ph.D, Minnesota, 1935) has been with the Bureau of Entomology and Plant Quarantine of the U. S. Department of Agriculture since 1935. He is attached to the Division of Forest Insect Investigations and is in charge of a project to study the effects of DDT on gross insect populations important as natural control agents and as food for fish and wildlife. Dr. Linduska is in charge of investigations on the effects of insecticides and herbicides on wildlife and fish populations for the U. S. Fish and Wildlife Service. He has done research in wildlife management and insect control since 1935 and made contributions to the development of insect repellents for the armed services during World War II.

THE much-publicized wartime accomplishments with DDT caused many people to be apprehensive of the insecticide's subsequent application to civilian problems. There were good reasons for concern. A material one ounce of which was reported to be adequate to eliminate mosquito larvae from an acre of water certainly appeared to have potentialities for harm. Furthermore, this high toxicity made it especially adaptable for distribution by plane, thereby reducing operational costs; moreover, the toxicant itself was moderately priced. The whole picture was one of bigger and better control projects involving more and more acreage.

The early estimates of greatly expanded insect-control activities following the development of DDT were not exaggerated. Many crop pests are now being controlled more effectively and economically than ever before. For the first time, outbreaks of several species, particularly pests on forest crops, have been subdued by direct control measures. The well-deserved reputation of DDT as an agent for mosquito control has greatly increased the marsh acreage effectively covered in mosquito-abatement districts.

At left above: N3N trainer plane equipped with centrifugal pump and spray boom (*beneath lower wing*). *Right:* C-47, equipped for spraying. Successfully used to distribute DDT for control of the tussock moth infesting thousands of acres of forest land in the West (1947) and for gypsy moth control over large, inaccessible forest areas in the East (1948).

Although the immediate advantages of DDT as a control agent have been demonstrated on a wide scale, the possible hazards, particularly those resulting from repeated and long-time use of the insecticide, are not so well known. Many investigations of the general biological effects have been made, however. Since 1945 the Fish and Wildlife Service and the Bureau of Entomology and Plant Quarantine have cooperated in studying some of the biological effects of forest-insect control. More than twenty experimental areas, ranging in size from 100 to 300 acres and including various forest types and forest pests, have been studied in detail after application of 1–7 pounds of DDT per acre. Three major insect-control areas, ranging from 100 to nearly 650 square miles, also have been studied to determine any harmful effects. The U. S. Public Health Service, through its laboratory at Savannah, Georgia, has devoted much time and effort to evaluating the biological effects of anoph-

eline larvicides. Investigators of the Tennessee Valley Authority also have reported results from detailed inquiries on the general effects of mosquito control. A number of other agencies and many individuals have either participated in these studies or conducted independent investigations.

For the control of many insect pests of the forest, DDT dispersed as spray from airplanes has proved to be highly effective. In such operations various aircraft and distributing devices have been used. The most common spray apparatus is a simple boom with nozzles attached along the underside of the wings. The open-cockpit biplanes (N3N, Waco, or Stearman) ordinarily are flown 50–100 feet above the forest canopy, and at this altitude the spray swath may range from 90 to 110 feet. Larger airplanes, such as the C-47, are flown at higher altitudes; approximate width of the swath is 500 feet. The C-47, flying at 160 miles per hour, will treat 900 or more acres of forest in about eight minutes. In many experimental areas, filter papers were put out in open sites to sample the deposit left by the DDT spray.

For biological evaluations, study plots were established in sprayed areas and in comparable untreated areas. General observations and population counts were taken in both sprayed and unsprayed areas before, and at various intervals after, the spraying. Insect populations in different habitats were sampled by many collecting and trapping methods. Arboreal species were captured on sheets under the trees by tree jarring, and also in cloth-bottom trays placed on the ground to catch affected insects as they fell. Flying insects were captured in light traps, fly traps, box-area traps, and on boards covered with an adhesive. Certain ground species were taken in traps baited with fish or molasses, and in modified Berlese funnels. Sweep-net collections and general observations furnished data on other species. Insect populations in streams were measured by means of a square-foot sampler. Thousands of terrestrial and aquatic insect specimens, involving 800 species or groups, were collected and identified during the four years of study.

Information on reptiles and amphibians was obtained by making general observations in the field, by marking and releasing, and by confining species in open outdoor cages. Most of the studies on birds were made at the peak of the nesting season, when individuals were confined to established territories. Singing males were tallied, and by means of systematic trips the actual population of nesting pairs could be determined for measured plots. Small mammals were studied by live-trapping and marking, and larger forms by systematic observations. Wherever practical seine hauls were made, to estimate fish populations. Weirs, which were sometimes installed in sections of the streams to stop any affected fish, also were helpful in studying cumulative effects of the poison. In addition, live boxes containing known numbers of different species of fish were placed in some waters.

Generally speaking, aquatic species are affected more by DDT sprayings than are terrestrial animals. This difference may be due partly to inherent differences in susceptibility. It is more likely, however, that the intimate and continuous contact with the insecticide in aquatic environments produces greater effects. In several of the study areas both habitats were sprayed, but because of marked differences in effect on faunal groups in the two types it will be profitable to consider the results separately.

EFFECTS ON TERRESTRIAL LIFE

Insects. Wide variations were observed in the extent to which species of terrestrial insects were killed with DDT. Aside from some inherent differences in susceptibility to the poison, these variations may be attributed to differences in opportunity of coming in contact with the poison. Generally speaking, the species exposed on vegetation were more affected than were those protected under bark, in burrows, in leaf mold, and in soil. Most of those coming in contact with DDT when they were in susceptible developmental stages were severely affected. Caterpillars and flying adults of several orders were killed in large numbers, but species in a protected stage at the time of spray application and during the period when the residue was toxic were unaffected. Hairy insects, such as moths and bees, appeared to be resistant.

In most forest sprayings insects of many families and orders were greatly reduced in numbers even at the relatively low dosage of 1 pound of DDT per acre. The residual effect lasted for about a week. The effects of widespread spraying on biological control agents are of great importance. In the spraying of 14,000 acres for the control of the tussock moth in Oregon, some interesting observations on a larvaevorid parasite have been recorded. Although parasites in flight at the time of the spraying were killed, those that emerged later, plus those that survived in areas missed by the spray, were sufficient to parasitize tussock moth larvae not destroyed by disease or the spraying.

The results of a study made in Massachusetts indicated that a single spraying at about 1.5 pounds of DDT per acre was sufficient to eradicate

small gypsy moth larvae without causing any significant permanent change in the general insect fauna. Caterpillar control enabled the sprayed plot to remain in a healthy condition, with an abundance of vegetation and with normal shade and moisture. In contrast, an adjacent woodland, which was heavily defoliated by the gypsy moth, suffered from drought that followed the loss of shade formerly provided by a dense canopy. Post treatment samples showed more than three times as many insects in the sprayed plot as in the two check plots. Although the DDT spraying caused considerable damage to other insects in the treated area, the mortality was far less in the aggregate than that due to the conditions resulting from a heavy defoliation by the gypsy moth.

The effect of DDT applied at the rate of 2 pounds per acre was not prolonged, and most of the species were present in considerable numbers two or three weeks later. Spraying areas of 100 acres or less that might be repopulated by immigration from unsprayed habitats resulted in the following situation: Tree-inhabiting caterpillars were decimated; Hymenoptera were reduced immediately after the spraying, but the population equaled that of the check area one month later; some groups of flies showed no change, whereas others showed an over-all reduction of about 85 percent for two months afterward; many of the leaf-feeding beetles also were reduced greatly in numbers; aphids appeared to increase, although many of their parasites and predators seemed to be unaffected; most ground forms were unaffected by the spray, but a few having an emergence peak at the time of spraying were moderately reduced; only a few spiders were adversely affected.

In experimental forest sprayings with DDT at the rate of 5 or more pounds per acre, 90 percent of the terrestrial insects were destroyed within a few days. All the tree-inhabiting caterpillars were eliminated, but those infesting low vegetation, although reduced initially, gradually reached pretreatment numbers. The hymenopterous parasites of caterpillars were almost eliminated by the spray but gradually came back, until their numbers were perhaps one fourth of normal at the end of three months. Apparently, larger moths were unaffected, whereas the smaller ones were moderately reduced in number. Some flies were almost eliminated shortly after the spraying and showed little recovery that year. Certain leaf-feeding insects were greatly reduced by the spray. Psyllids feeding on the undersides of leaves, scale insects under their protective coverings, and Collembola dwelling beneath bark were unaffected. Many ground-inhabiting insects, including beneficial scavengers and predators, were not seriously affected. Some flies and their various hymenopterous parasites continued to breed in the humus. Ants were affected by the spray, but their numbers were diminished for about a week only. Many arthropods of the soil were not disturbed by the single heavy application. Spiders were variously affected, species living in exposed webs being killed in larger numbers than those in more protected places.

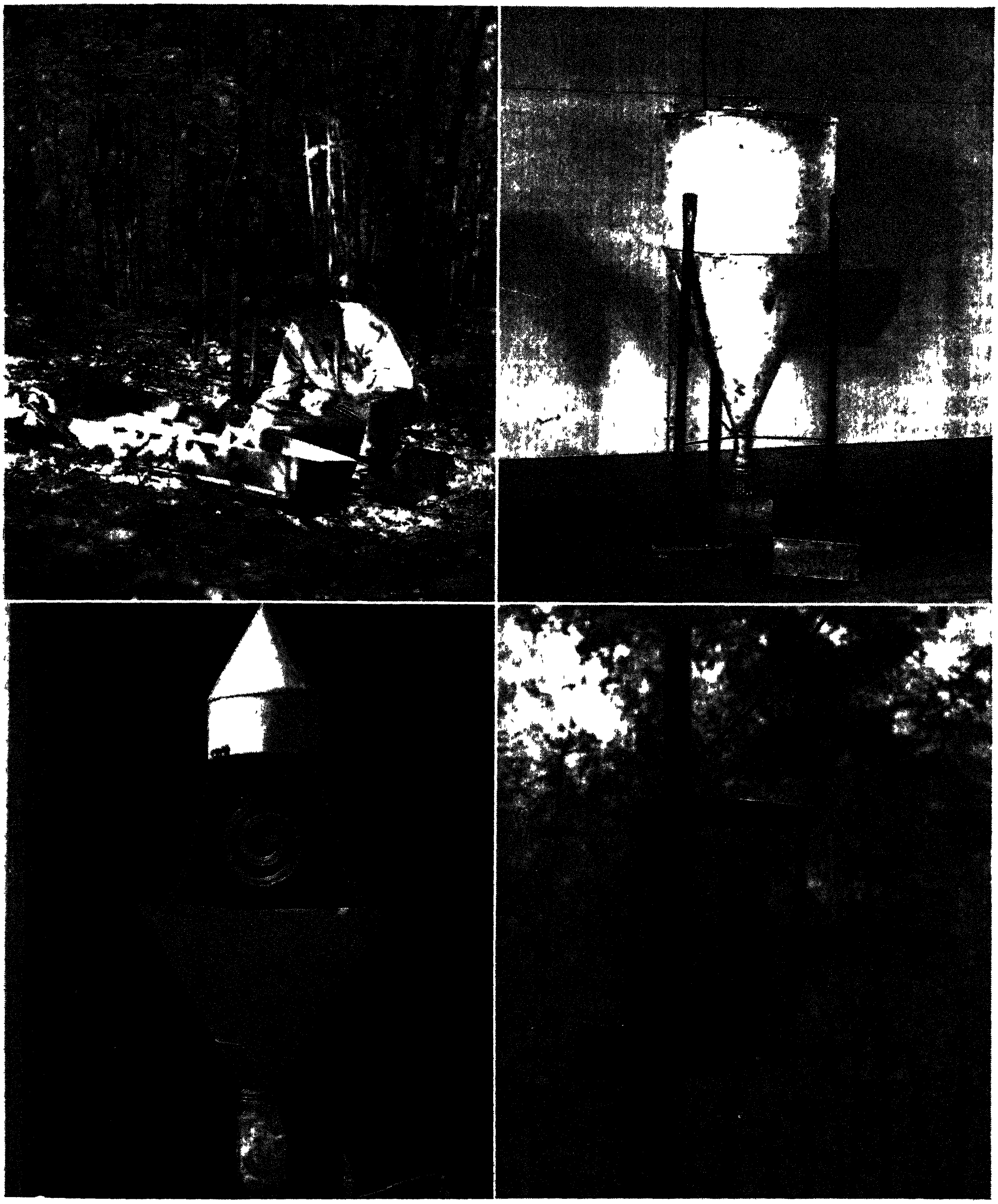
Two outstanding irruptions in insect populations followed a 5-pound per acre application. In one, pretreatment sampling indicated a normal aphid population and what appeared to be a sufficient number of insect parasites and predators to maintain a balance in numbers. Aphids in exposed locations were killed, but others on the undersides of leaves increased in number and later settled on uncontaminated new growth. It is possible that their enemies were affected more seriously by the spray. About six weeks after the spraying, an outbreak of many species of aphids developed on the principal forest trees of the area. Aphid predators gradually responded to the abundance of their prey and became more numerous. These natural control agents could not keep up with the aphid population, however. The most important check on the outbreak was an exceedingly heavy rain, which probably destroyed two thirds of the population. Later, natural enemies caught up with the aphid population, which has remained at the pretreatment level for three years.

The other irruption concerned a mite that was prevalent on oak leaves shortly after the spraying. Approximately one year later there was an outbreak, and noticeable injury was caused by mites feeding on red maple foliage. Similar damage was not evident in near-by areas. A few months later the mites disappeared, and no specimens have been collected in the area for two years.

Studies indicate that a single aerial application of DDT to the forest at the rate of 1 pound per acre, which is sufficient to control many forest pests, does not seriously affect the general terrestrial arthropod fauna. Dosages of 5 pounds or more per acre threaten extermination of many insect species, but do not cause general catastrophic losses.

Reptiles. Scant data are available on the effects of DDT on land reptiles. The few snakes found dead after DDT applications have been associated with dosages of 4 or more pounds per acre. Small numbers of live snakes were seen in the same areas after the spraying. In Texas an experimental attempt to control ticks with 44 pounds of 10 percent

DDT dust per acre was suspected of having caused a heavy mortality of the insectivorous rough green snake. The tendency of most snakes to conceal themselves, preferably in heavy ground cover,



Cloth-bottom tray placed beneath tree canopy to collect DDT-killed insects, and, on right, modified Berlese funnel used to determine invertebrate populations in litter and humus. Heat from light bulb in funnel forces these organisms (7–13,000 per square foot) down through the soil into the alcohol-filled jar below. Shown below is a light trap with coiled argon-mercury discharge tube, whose blue light effectively trapped large numbers of insects, particularly moths. Insects were killed by fumes from pad in bottom of jar saturated with carbon tetrachloride. Stale beer used as bait in fly trap at the right caught up to 8,000 flies per day.

makes it difficult to evaluate populations by direct observation.

Birds. Because most wild vertebrates resist restraint, critical toxicity studies on captive animals are difficult to carry out. Sufficient work along this line has been done, however, with a few species to demonstrate some of the variables in connection with DDT poisoning. At the Patuxent Research Refuge acute and chronic toxicity tests were conducted on four species of birds and three of mammals. In addition to differences in species susceptibility, wide differences in individual effect were apparent. These presumably were due to irregular absorption. Type of formulation had a marked influence on the toxic effect of DDT. With bobwhite quail, for instance, the acute toxic level by oral administration was 60–85 mg/kg of body weight for vegetable oil solutions. When the DDT was administered in crystalline form, the acute dosage was three to four times as great, or about 300 mg/kg.

Combined field-laboratory tests indicated that the method of poisoning in songbirds involves ingestion of the material. Nests, eggs, and newly hatched young of songbirds were sprayed heavily with DDT-oil solutions without effect, whereas nestling birds were killed when fed DDT-contaminated insects. Condition of birds was found to be important in regulating toxic effects. The mortality was increased when a subsequent period of partial starvation was imposed on young individuals.

Laboratory investigations with vertebrates have been of value in indicating some of the factors that may modify the direct toxic effects of DDT, but the application of such findings to field conditions has definite limitations. Although the complex interrelationships of faunal groups are at best poorly understood, numerous investigations have shown that the prosperity of one group may be directly influenced by the status of others. For this reason special emphasis has been given to evaluations under field conditions.

In a number of experimental tests in forest areas, birds were unaffected by a single application of DDT at the rate of 1 pound per acre. Although the general insect population was sharply reduced in nearly all cases, recovery of numbers was rapid, and there was no evidence that the temporary reduction was of any real consequence to birds. Development and survival of nestling young appeared to be normal at this dosage.

Observations on birds in connection with large-scale operations were consistent with the findings on experimental plots. In 1945 the Ontario Department of Lands and Forests, Canada, treated more than 60,000 acres at the rate of 1 pound of DDT per acre for control of the spruce budworm. Intensive studies of birds were conducted on three plots in the sprayed area and on one outside the treated area. Four birds were found showing symptoms of DDT poisoning, two of which subsequently died. Normal development of young was observed in several nests, and no measurable change in populations of adult birds was noted.

In May and June 1947 about 400,000 acres of forest land in northern Idaho were treated for control of the Douglas-fir tussock moth. DDT in oil was applied by planes at a dosage of 1 pound of the toxicant per acre. Detailed studies indicated that the spraying had no apparent effect on a high bird population, which included 44 species. Counts taken after the spraying showed a decline in numbers of 9.5 percent, compared with 10.6 percent in a check area. The slight decline in both areas was believed due to some individuals having completed nesting. Bird censuses accounted for practically all the original individuals throughout the study, and numerous nests and family groups appeared unaffected.

Although dosages of 1 pound per acre or less have not been found to be lethal to birds, some evidences of modified behavior have been observed. On several occasions swallows, flycatchers, and other birds of similar feeding habits have temporarily abandoned sprayed areas. Marsh and aquatic areas sprayed for mosquito control were principally involved.

Studies on birds in areas where four consecutive yearly airplane applications of 2 pounds of DDT per acre were made at the peak of the nesting season have so far shown no deleterious effects. There have been few observations to indicate the effects of aerial applications with intermediate dosages of 2–4 pounds of DDT per acre. With 5-pound per acre aerial applications, however, marked kills have resulted. On one 600-acre tract, sprayed experimentally at this rate for control of gypsy moth larvae, a population in excess of three birds per acre declined in two weeks' time to about one sixth of the original population. One year later the numbers were about 85 percent of the prespray total.

In a second test a 5-pound per acre application to a scrub and sapling growth reduced the number of birds by more than one half. Of the five commonest species in the area, three—the Maryland yellowthroat, the prairie warbler, and the house wren—were reduced by 80 percent. Species with ground-feeding habits appeared to be least affected

by the aerial application. Elsewhere, in connection with attempted tick control, 4 pounds of DDT per acre distributed with ground equipment caused a heavy mortality among ground-feeding species and had a lesser effect on canopy inhabitants.

Additional studies on the effects of high dosages of DDT on birds were made late in summer when birds had completed nesting. These observations were obtained in experimental attempts to control the mountain pine beetle in the Teton National Forest and the Black Hills beetle in the Black Hills National Forest, both in Wyoming. DDT in amounts of 5–10 pounds per acre was applied to irregularly shaped plots, mostly less than 100 acres in size. Detailed censuses of birds were not practicable at this time of the year because of widespread movement. Systematic search over the areas revealed some mortality, but population indices showed reductions hardly commensurate with the high dosages used.

Studies on post-breeding bird populations in Georgia pecan groves revealed similar results. In this area DDT was used for pecan weevil control in dosages as high as 3.5 pounds per acre. No adverse effects on birds were observed.

The nominal effect on birds of heavy dosages of DDT applied under the conditions outlined above is in marked contrast to the situation found for several other areas. Low kills resulting from heavy dosages of DDT appear to be explained by the following conditions: In each case the spray was applied after birds had completed nesting. Relieved of the burdens of nesting duties, and with territorial behavior no longer manifested, the birds were free to range over much larger areas than usual. In addition, all the areas under treatment were small, and the birds with their enlarged daily range probably were flying beyond the limits of the sprayed areas. There is little doubt but that these two factors operated mutually to minimize the effects of otherwise critical dosages.

Mammals. The difficulties of exact census work on mammals have limited the accomplishments in field studies on the group. The larger, wide-ranging species, in particular, have posed real problems in attempts to procure information on possible effects of insect-control operations. However, at least twenty species of mammals, including such common forms as the cottontail rabbit, raccoon, opossum, skunk, deer, woodchuck, and squirrels of several kinds, were under observation in one or another of the studies. Dosage rates under which observations on mammals were made ranged from a fraction of a pound to 7.5 pounds of DDT per acre. In several of the investigations, intensive live-trapping and marking of small mammals were employed in order that population changes might be measured and any abnormal movements of individuals in and out of plots detected. In one study the effects of repeated applications, such as may be practiced in malaria control, were observed. None of these field investigations revealed any measurable effect on the mammal population, although at the higher amounts of 5 and 7.5 pounds of DDT per acre, one shrew and several chipmunks were seen in a condition highly suggestive of DDT poisoning. It is likely that with aerial applications the critical dosage for many mammals is near the 5-pound per acre level. Cottontail rabbits in captivity have been killed by ground applications of 5 pounds per acre of DDT in oil. Because much of the spray is lost when applied aerially, the ground application of the same amount represents a conspicuously more critical treatment.

As with birds, the possible hazards to mammals need not involve only a direct toxic action. In one study, after a comparatively light application of DDT for mosquito control, racoons were secondarily affected by an almost complete kill of crayfish that had been a staple item in their diet. After gorging on the dead crayfish for a few days, they were forced by a depleted food supply to leave the creek bottom and seek other fare in the uplands.

EFFECTS ON AQUATIC LIFE

The high susceptibility of fish and other aquatic forms to DDT has prompted a number of careful studies. In general, investigations have been made along two lines: measuring the hazards of the purposeful application of small amounts of DDT to aquatic areas for mosquito control, and research on the danger of unintentional and unavoidable contamination of productive waters by heavier dosages in connection with forest-insect control and other operations.

The U. S. Public Health Service at Savannah, Georgia, and the Tennessee Valley Authority at Wilson Dam, Alabama, have given particular attention to the biological effects of larvicidal treatments for malaria control. The results have been reassuring in a number of respects. A dosage of 0.1 pound per acre applied by airplane was not found to be unduly harmful to either fish populations or their food supply. Seventeen such treatments at about weekly intervals were applied without causing measurable effects on fish. Moreover, there was no evidence that intricate food-chain relationships of aquatic life were disrupted critically. Since present trends in anopheline control emphasize dosages below 0.1 pound per acre, it

is reasonable to suppose that this important use of DDT will not conflict seriously with recreational and commercial interests.

The control of pest mosquitoes requires larger amounts of DDT, and it is likely that in some of this work such dosages will approach marginal levels of safety for beneficial life. At the commonly used rate of 0.2 pound per acre, however, no disturbing developments were noted In cooperative studies with the Army at Edgewood Arsenal, Maryland, 0.2 and 0.26 pound per acre, applied one month apart, produced slight effects on fish Other experimental sprayings of 0.5 pound of DDT per acre caused a moderate to heavy kill of fish and edible crabs.

In the control of forest insects, aquatic situations in project areas will require careful consideration. Extensive experimentation has been done on this phase of possible injury, and present information indicates that a 1-pound dosage represents the approximate limit under which forest-insect control work can be done without appreciable losses in aquatic environments.

Reptiles and amphibians. In 1948 two Pennsylvania watersheds comprising 52,000 acres were sprayed at the 1-pound dosage for the control of the gypsy moth. Small numbers of dead water snakes were found in some of the lakes and streams, and it is believed that these animals are susceptible to DDT poisoning. Few tadpoles, frogs, and salamanders were killed compared with the total populations.

No adverse effects were observed on several species of turtles, bullfrogs, or water snakes when a stream in West Virginia was treated with a suspension at the same dosage.

In a tick-control project at Bull's Island, South Carolina, amphibians were commonly killed with applications of 2 and 3 pounds of DDT per acre. Reptiles were affected only moderately. In a Maryland woodland, open-topped cages stocked with adult green frogs, pickerel frogs, and bullfrogs, adult toads, and frog and toad tadpoles were observed for a few days before and for nine days after the area was sprayed at a 2-pound rate. The heavy canopy of the mature forest intercepted much of the DDT spray, and probably accounted for none of the animals being affected.

Fish and fish-food organisms. In dirt-bottom ponds treated with 1–5 pounds of DDT per acre, practically all the surface insects and those breathing at the surface were eliminated; however, the auxiliary solvent and carrier in this formulation had a like effect on surface forms. Most of the free-swimming and crawling insects were greatly reduced in numbers, and some were exterminated. About two months after treatment all the ponds had recovered markedly, although the species and age groups present were much different from those found before the spraying. Insects living on the bottom of ponds were not affected appreciably by any of the dosages. In one of the shallow ponds treated at the heaviest dosage, several frogs and large frog tadpoles and a young water snake were killed. Some tadpoles and frogs remained alive in all the treated ponds, however.

Aquarium tests with fish showed that numerous factors modify the toxicity of DDT to this group. Marked differences in species susceptibility were observed, and age groups were also affected differently, young animals showing the least resistance. Well-fed fish were less inclined to poisoning by the material than were those from which food was partially or wholly withheld. As with warm-blooded vertebrates, DDT was appreciably more toxic to fish when it was dissolved in oil than when administered in fat-free carbohydrates or proteins. At given dosages of DDT the type of formulation caused wide variations in effects, emulsions, oil solutions, and suspensions representing the order of decreasing effectiveness. Various conditions of the aquatic medium modified results, and warm temperatures, reduced oxygen tension, and soft water all enhanced the activity. Clarity of the water appeared to be of considerable importance both in field and laboratory tests. With applications of 0.25 pound of DDT per acre, 84–100 percent of the rainbow, brook, and brown trout were killed in dirt-free aquaria. The same dosage applied to identical aquaria containing a layer of mud caused mortalities of 0–39 percent in the same species.

In general, erratic results were obtained when insects sprayed with a suspension or an oil solution at 1 pound of DDT per acre were fed to several species of fresh-water fish. Some were killed after devouring relatively few sprayed insects; others gorged themselves without adverse effect; a few exhibited DDT tremors but recovered later. Fish that were fed insects sprayed with an oil solution of DDT usually were more easily killed than those fed suspension-sprayed insects. Well-fed fish survived in large numbers even though they were fasted after a 3-day feeding on DDT-sprayed insects.

Many experiments with DDT formulations were performed in dirt-bottom ponds and concrete raceways and daphnia ponds to test the toxicity of DDT to different species and sizes of fish. Bluegill sunfish and largemouth and smallmouth black

bass 1 inch in length were killed by suspensions and oil formulations in applications ranging from 0.25 to 1 pound of DDT per acre. Fingerling fish 2 inches or more in length withstood the higher rates of application better. In some instances large fingerling or adult fish survived as much as 1 pound of DDT per acre formulations. Fingerling bluegills, smallmouth black bass, and black crappies were more sensitive to DDT than were largemouth black bass, golden shiners, and trout. In raceways with a continuous flow, brook and rainbow trout, smallmouth black bass, and golden shiners were relatively unaffected by a 1-pound dosage of DDT in suspension. In general, fish mortality from DDT suspensions occurred later and to a lesser extent than from oil sprays.

As would be expected where important variables such as closeness of canopy, size, and volume of stream exist, a wide range of results has been evident. In most cases, however, 1-pound aerial applications deposited about one third that dosage at the water surface. This was sufficient to cause a heavy loss of invertebrates. Some mortality of fish occurred in each case, but not enough to be significant in terms of the total population. Young individuals showed a higher susceptibility than older fish of the same species, and in general forage species appeared to be more seriously affected than common game species.

One- to three-mile sections of cold-water and warm-water streams were sprayed experimentally by airplane at the 1-pound dosage to determine the effect on fish and fish-food organisms. The spray was applied in one swath by a small plane flying directly over and parallel with the stream. The insects in the riffle areas at the lowest stations in a Pennsylvania stream showed a 90 percent reduction. Most of the May flies were eliminated. Certain invertebrates (worms, snails, dragonfly naiads, water mites, and some coleopterous, fish fly, and dobsonfly larvae) appeared to be unaffected by the spray. Stream insects become reestablished rapidly, and within a year certain species of May fly and midge larvae had become exceedingly abundant. These species have a short life cycle and doubtless became re-established soon after the residual effect of the poison was dissipated. Although the aggregate numbers of insects had equaled, if not surpassed, the prespray level, several of the more susceptible species failed to attain their original numbers.

It was estimated that 1.3 percent of the brook trout population was killed. Warm-water species in the lower part of the stream were affected within twelve hours after the spraying. They included fallfish, common shiners, common suckers, and golden shiners.

A section of a smallmouth black bass stream in West Virginia was sprayed to determine the difference in effects with two formulations of DDT applied at the 1-pound rate. A surface film of DDT in oil caused marked reductions in the numbers of surface Coleoptera and Hemiptera, whereas a DDT suspension caused only limited kills in these groups. The oil solution killed 90 percent of the bottom organisms at the lower stations, and the suspension about 70 percent. Both sprays appeared to eliminate May flies, but a year later they were present in pretreatment numbers. Other insect groups were not affected so drastically by either formulation.

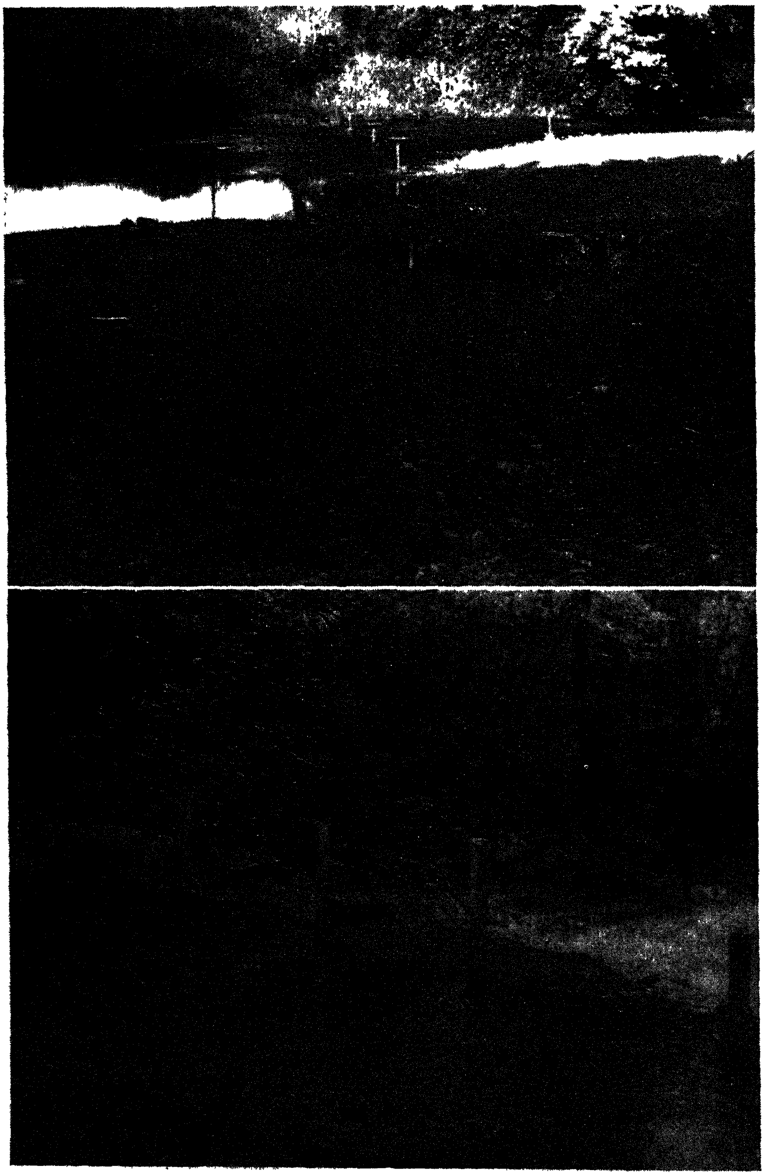
Although minor losses of small fish occurred after both sprayings, the oil spray killed more fish and in a shorter time than did the suspension. Only the oil spray was lethal to adult fish. Spotfin shiners, silverling minnows, and fallfish were noticeably affected by the oil spray, whereas bluntnose minnows, chub suckers, and pickerel were affected only slightly.

A ⅜-mile section of a small creek in the Teton National Forest was sprayed at the rate of 2.5 pounds of DDT per acre. This creek contained a series of active beaver ponds and supported a large population of cutthroat trout. Only 11 trout and about 50 percent of the bottom organisms were killed. The surprisingly small amount of damage incurred as a result of a fairly heavy dosage of DDT was probably due to the nature of the stream. Beaver dams slowed the flow of water so that there was little mixing of the spray, and the stream, which contained much organic debris, was muddy from beaver activity. Other studies indicated that the toxic action of DDT may be reduced under such conditions. It is also possible that cutthroat trout, in common with other species of trout, may be less susceptible to DDT poisoning than are some other fresh-water species. However, May fly survival in the lower part of the stream suggests that the biological and physical conditions of the stream reduced the residual potency of the DDT.

In connection with the program in Idaho for control of the Douglas-fir tussock moth in 1947, studies were made of the effects on aquatic life. Several species of trout (rainbow, eastern brook, and cutthroat), speckled dace, and red-sided bream were unaffected by the 1-pound per acre application of the poison, but cottoids, mountain suckers, and black catfish suffered heavy losses in limited areas. Crayfish, paralyzed by DDT, comprised 99

percent of the stomach contents of 21 brook trout, whereas no crayfish were found in stomachs of fish from untreated areas. The loss of fish-food organisms amounted to 50 percent in one locality. Elsewhere fish-food organisms were practically eliminated from riffle areas, but even in fast-moving streams the effects did not extend far below the sprayed areas.

After the experimental aerial spraying with DDT (1 pound per acre) for the control of the gypsy moth in Pennsylvania in 1948, 70–90 percent of the stream insects in two watersheds were destroyed within three days. That affected insects were fed on extensively by different fish was confirmed by direct observations and examination of stomach contents. Several species of fish of various sizes were killed by the spray or spray drift on ponds, lakes, or streams, particularly where there was double spray coverage. Species most affected in one artificial lake were golden shiners, pumpkinseed sunfish, and yellow perch. Stream species found dead were mostly the common white sucker and brook trout. These studies indicated that few fish in comparison with their total populations were killed in either the lakes or the streams. In some locations, however, only spray drift was involved. Although bottom organisms were drastically reduced in the streams, extraordinary numbers of midge larvae and lesser numbers of surviving invertebrates were present two months after the spraying. Fish seined at this time appeared to be in good condition.



Filter-paper station across a riffle area in a stream caught DDT, which was later analyzed chemically. Amount recovered was correlated with effects on aquatic insects and fish. *Below:* Weir installed on a stream to stop fish killed by DDT and washed downstream.

DDT AND ANIMAL-POPULATION RELATIONSHIPS

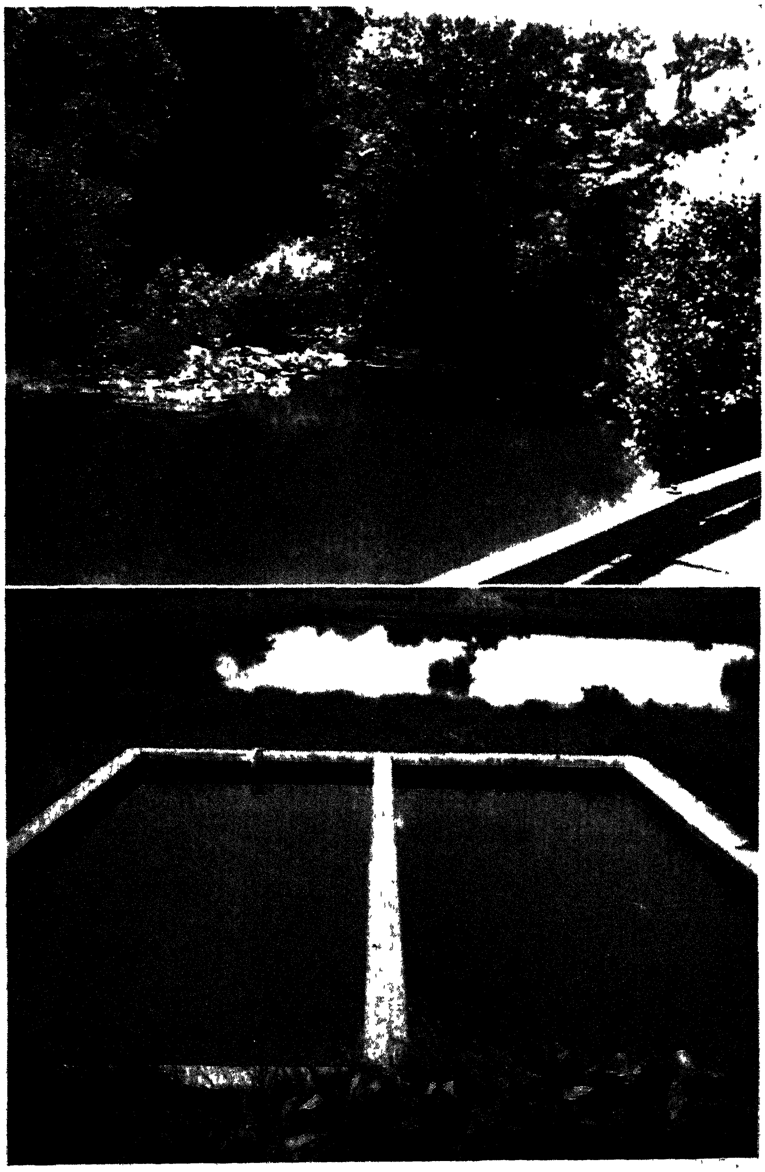
To understand the maze of interrelationships existing in wild populations challenges the ingenuity of ecologists even where conditions are little disturbed. The additional complications that control measures provide increase the difficulty of evaluating the status of many species. In these studies it has not always been possible to distinguish between effects of insecticide application and other changes arising through the normal procession of ecological events. This much, however, seems to be true of most control measures with DDT: Even with applications of a fraction of a pound per acre there is usually a temporary reduction in the numbers of susceptible invertebrates. In forest areas there has been no indication that such a result has caused either a marked or permanent change in the welfare of vertebrates, or in the economy of the

woodlands in general. Although application of the insecticide during the most vulnerable life-history stage of the pest insect can effect a long-time reduction of this species, the effects on gross insect populations appear to be transitory. The sprayed territory is rapidly repopulated by the continual emergence from life-history stages inactive at the time of spraying, and possibly by some invasion from untreated areas. Such observations appear to apply generally with dosages not exceeding 1 pound of DDT per acre.

In aquatic locations a more or less parallel situation has been noted. In these habitats, however, the organisms have opportunity for a more thorough and continuous exposure to the insecticide, and the over-all biological effects are greater. Even in these situations some re-establishment of invertebrates has been apparent in about two months. As would be expected after any general and heavy mortality, regardless of the cause, qualitative differences are apparent in the newly recovered population. Frequently the more prolific forms, midges in particular, predominate in the community. Within a year the variety and number of invertebrates may be at least as great as before spraying.

Of especial interest in these studies has been the constant reminder that a consideration of dosages alone is entirely inadequate for an interpretation of the probable hazards of DDT control operations. Instances have been discussed in which the time of insecticide application and size of treated area were found to modify considerably the effect of a given treatment on bird populations. Under conditions of actual control, weather and certain characteristics of the aquatic environments were also found to minimize markedly the effects of treatment on the fauna. Other observations in the field and laboratory have shown how such factors as the type of formulation and method of application may operate singly or in combination to modify results. Although such variables contribute greatly to the complexity of the problem, they also add encouragement to the prospects of reducing operational hazards by a proper attention to their importance.

The widespread adoption of DDT has prompted various estimates of the hazards involved. Some individuals, interested only in seeing this season's crop brought safely to harvest, refuse to recognize any possibility of dangers or disadvantages in its use. Others, who seemingly visualize static rela-



Live boxes containing known numbers of several kinds of fish were placed in quiet sections of streams, and controlled experiments of different DDT formulations and dosages were conducted in concrete daphnia ponds and dirt-bottom ponds at the U. S. Fish-Cultural Station, Leetown, W. Va.

tionships among living animals, speak vaguely of DDT upsetting the "balance of nature." For wilderness areas, particularly, they urge that control with such materials as DDT be abandoned in favor of natural balances, which they intimate will result from a laissez-faire policy. Neither of these attitudes, of course, represents an intelligent appraisal of the problem. Irresponsible thinking of the first type has resulted in many dust bowls and muddy rivers. The other viewpoint, professing a childlike faith in what nature will provide, is equally in error for its failure to consider realities.

Biologists, and others who have given attention to the dynamics of animal numbers, recognize many designs in the functioning of populations. Insect pests of the forest, for instance, have many natural forces tending to maintain some equilibrium in their densities. During all stages of their life history parasites and predators of their own kind exact a heavy toll. Vertebrates of many species make further inroads on their numbers, and various meteorological phenomena have a further controlling influence. Periodically, however, conditions arise which disturb this balanced relationship, one or another of the pest species flourishes, and some additional means of suppression is required if the woodland is to be preserved. At our present state of knowledge the use of toxicants such as DDT is often the most effective means of bringing such pests under control.

For many types of control the immediate gains, financial or otherwise, are well defined. With some insects of medical importance, and pests of great economic concern to agriculture, the biological hazards of control compare with a different set of values than would be the case, for instance, in suppression of some forest-insect pests. In the latter case the crop may be of comparatively low monetary value and produced in areas of a more direct multiple-use nature. Here the occasion probably is greatest for measuring benefits of control against hazards to recreational and other interests.

There are still many unknowns regarding the biological effects of new insecticides and the many new economic poisons. With reference to DDT, and its remarkable stability under some conditions, one of the most critical needs is for a better understanding of the hazards implied by possible cumulative action. Studies now under way should bring enlightenment regarding this possibility, not only as it could affect humans and wildlife populations but, fundamentally, the soil and plant life. Thus far, investigations have served to allay many early fears of its use out-of-doors, and to indicate some of the limitations. It is very likely that the continuing cooperative studies by biologists of various specialties will be productive of such additional information as is necessary for a circumspect use of DDT generally.

URANIUM RESOURCES

J. K. GUSTAFSON

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HOW does uranium occur in nature? Is there enough uranium to sustain an atomic energy program directed toward tools of peace as well as weapons of war? What is being done in this country to provide a continuing supply of uranium?

In spite of the necessary restrictions placed upon any public discussion of uranium resources, I shall make an effort to answer these searching questions as fully and as comprehensively as possible. Only in this way can we see the problem of wise administration of atomic energy and a sound foreign policy in the proper light.

How does uranium occur in nature? Genetically, uranium occurrences can be divided into four main types: igneous rocks, hydrothermal vein deposits, sedimentary rocks, and deposits of doubtful and perhaps complex origin. Certain oxidized deposits may be considered either as a fifth type or as a variant of the four basic types.

Uranium occurs in trace amounts in most igneous rocks but is concentrated notably in granitic rocks. It occurs in granites in concentrations in the order and magnitude of 0.00003 percent–0.0007 percent. Uranium occurs in granites chiefly in the accessory minerals monazite, xenotime, and possibly zircon. Granitic pegmatites rich in potash feldspar, however, may contain pitchblende, autunite, and other uranium minerals in visible amounts. Some pegmatites, like other uranium ores, have been mined in the past for radium (it is generally considered that radium is in equilibrium with uranium in the ratio of $3.4 \times 10^{-7} : 1$). In general, however, pegmatites are not a large potential source of uranium because the average grade is low, seldom exceeding 0.01 percent U_3O_8, and the tonnage is usually small. There may ultimately be amounts of uranium recovered as a by-product of feldspar or mica mining.

A decided preference of uranium for granitic rocks and acid pegmatites, and the grouping of some of the hydrothermal uranium deposits around granitic intrusions, clearly indicate that granitic magmas are the great primary source of uranium in the earth's crust.

Mesothermal pitchblende veins have yielded most of the uranium and associated radium that have been produced in the world. There are two main types of these:

1. Veins mined principally for lead, zinc, copper, gold, and silver, such as the veins of Gilpin and Boulder counties, Colorado. Pitchblende occurs in these as narrow streaks and patches. Veins of this type have contributed little uranium to world supply.
2. Veins containing cobalt and nickel minerals, in addition to other base metals and minor amounts of gold and silver. It is veins of this character that have produced the famous radium-uranium mines of Great Bear Lake in Canada, the Belgian Congo, the Erzgebirge region of Saxony and Bohemia, and the Cornwall district of England. The three most productive uranium-radium mines of the world are Joachimsthal, Bohemia; Eldorado in Canada; and Shinkolobwe in the Belgian Congo.

Some of the principal features of these three deposits are the following:

1. Each occurs as veins in pre-Cambrian sedimentary rocks.
2. The ore in each case is pitchblende associated with cobalt. At Eldorado and Joachimsthal nickel, bismuth, and silver are also present, whereas at Shinkolobwe molybdenum, thorium, tungsten, gold, platinum, and palladium are mentioned, as is lead at Eldorado.
3. In each case the principal nonmetallic gangue minerals are quartz and carbonates. Hematite, chlorite, barite, and fluorite also are prominent at Eldorado.

Some hypothermal quartz veins also contain small shoots of pitchblende, but these are relatively unimportant.

Marine sediments in many parts of the world contain uranium in low concentrations but in concentrations many times that of other rocks. Notable in this regard are black shales and phosphorites. A number of these formations contain 0.01 percent–0.02 percent U_3O_8, and at least one—the alum shales of Sweden—contains nodules and lenses of a nearly pure bitumen called *kolm* which,

according to published figures, contains 0 5 percent U_3O_8.

Marine uranium-bearing black shales are characterized by abundant organic matter and sulphides and by small content or absence of carbonate material. The beds richest in uranium occur in thin formations of pre-Mesozoic age. The mineral or compound containing the uranium in these black shales has not yet been identified.

Probably all marine phosphorites contain some uranium, and phosphatic nodules in many marine black shales also contain concentrations of uranium. Although the form in which uranium occurs in phosphorites is unknown, the fact that uranium increases in a general way with increase in phosphate content suggests that the uranium may occur in the space lattice of the phosphate mineral The phosphorite formations, like the black shales, are characteristically thin and are generally associated with unconformities, or diastems. These facts of occurrence have indicated to the men who have studied them that these uraniferous formations were deposited in large basins adjacent to low stable land masses during periods when mechanical erosion was at low ebb and chemical conditions in the sea water inhibited the formation of lime deposits. Very possibly those marine sediments derived from granitic land masses are the most highly uraniferous.

Two main types of uranium deposits are not yet properly classifiable. These are the vanadium-uranium ore deposits occurring on the Colorado Plateau and in lesser amounts in other parts of the world, and the Witwatersrand gold-bearing conglomerates of South Africa. The carnotite and roscoelite-type uranium-vanadium ores of the Colorado Plateau occur as small tabular or lenticular deposits impregnating the flat-lying Morrison sandstone and Entrada sandstone of Jurassic age and the Shinarump conglomerate of Triassic age. They are widely but spottily distributed over an area nearly two hundred miles in diameter. The long axis of the deposits, many of which do not contain more than several hundred tons of ore, are nearly parallel to bedding, but the ore does not follow the beds in detail. The mineralogy of the ores is very imperfectly understood. The deposits contain several times as much vanadium as uranium, and the bulk of the vanadium occurs in extremely fine-grained minerals of micaceous habit, probably belonging to the hydrous mica group of clay minerals. The principal mineral has been recognized tentatively as roscoelite. The principal uranium-bearing minerals are thought to be carnotite and tyuyamunite, sometimes called calcium carnotite. The theories of ore deposition subscribed to by most geologists are either: (*a*) that the ore was precipitated from ground water after the enclosing sands had accumulated, or (*b*) that the vanadium-uranium content was deposited during deposition of the enclosing rocks but was widely distributed in them and has experienced considerable subsequent movement and reconcentration by ground water. Small variations in bedding and sedimentary features of the rock appear to have guided the movement of ore solutions, with resulting concentration of the ore. Fossil logs and twigs are often completely replaced by ore and are further evidence that carbonaceous material generally is a precipitant for uranium. These types of ore bodies show a considerable range in both uranium and vanadium content and also a considerable range in the uranium-vanadium ratio Much of the ore contains anywhere from 0.1 to 0.3 percent U_3O_8 and from 0.5 percent to 2.5 percent V_2O_5, although small tonnages of very much higher grade ore are mined from time to time. These deposits constitute the largest readily available source of uranium in the United States but a source greatly inferior to the high-grade hydrothermal pitchblende deposits of other countries.

Recently, along the southwest and western edges of the Colorado Plateau, numerous new prospects of uranium (carnotite)-copper ores in the Shinarump conglomerate have been found which are similar in size and occurrence to the uranium-vanadium ores. Whether these will be important producers is not yet clear.

The Witwatersrand reefs are extensive beds of metamorphosed quartz conglomerate. It has been known for many years that the gold ores contained small amounts of uraninite, but only recently has the possibility of by-production of uranium from this source become apparent. A controversy has raged for many years over whether the gold ores had a hydrothermal or a sedimentary origin. The origin of the uranium in this conglomerate is equally in doubt, but it may not necessarily be the same as that of the gold.

In Canada, where glaciation stripped off any old oxidized soil covering that existed, primary uranium minerals but slightly oxidized occur at the surface. In countries like Africa and Australia, however, primary products are oxidized to depths of hundreds of feet. Here pitchblende, which is a black, heavy, metallic mineral, has been converted in the zone of oxidation to brightly colored secondary minerals, such as torbernite, a bright em-

erald-green hydrated phosphate of copper and uranium; autunite, a lemon-yellow hydrated phosphate of calcium and uranium; or carnotite, the canary-yellow hydrated vanadate of potassium and uranium. As is the case with copper ores, a colorful, flamboyant outcrop does not necessarily mean good primary ore in depth.

Is there enough uranium to sustain an atomic energy program directed toward tools of peace as well as the weapons of war? The answer to this question involves an appraisal not only of the actual quantity of uranium that man can win from the earth's crust if he bends every effort toward this purpose, but also the value of uranium or the measure by which man limits his effort to recover uranium. Recently a number of distinguished men in scientific fields have made some guesses as to the availability of uranium and have come up with gloomy answers. One of these prophets has concluded that there never will be "an atomic age." Another believes that the use of uranium for atomic power will be extremely limited. Before answering the question, let us examine the comparative value, in terms of energy, of uranium and other substances

According to the Smyth Report, if all the atoms in a kilogram of U-235 were to undergo fission, the energy released would be equivalent to the energy released in the explosion of about 20,000 short tons of TNT. We have had dramatic and terrible demonstrations at Hiroshima and Nagasaki of the power of uranium as an explosive. The bombs dropped on Japan had more than 2,000 times the blast power of the British "Grand Slam," up to that time the largest and most destructive bomb ever made. Clearly, uranium is here to stay as a weapon, but we are concerned at the moment with appraising it as a fuel for peacetime energy.

The consumption of about one pound per day of uranium 235 in fission generates heat at a rate equivalent to approximately 450,000 kilowatts, which is the amount of heat that would be obtained by burning about 1,300 tons of coal per day. Even with low efficiency, uranium 235 is obviously a very potent source of power. Although in naturally occurring uranium only one part in 140 is the fissionable isotope uranium 235, by a process known as "breeding," it is theoretically possible to combine some additional uranium 235 with natural uranium and to convert the nonfissionable uranium 238 into fissionable plutonium. By this process, to quote from J. R. Menke's "Nuclear Fission as a Source of Power," "for each pound of fissioned (burst) uranium 235 together with one pound of ordinary uranium we get (*a*) about ten million kilowatt hours of energy in the form of heat, (*b*) about one pound of fissile element (e.g., plutonium) and (*c*) about one pound of new radioelements." This is of the order of about three million times the energy released by the burning of an equal weight of coal. This strange process of eating your cake and having it too is theoretically applicable not only to uranium 238 but also to thorium, the use of which would increase available atomic fuel manyfold

The experts, in groping for an estimate of the amount of uranium available, have approached the problem from two directions. One is to take the average uranium content of a great many samples of rock of different kinds and to conclude that the resulting figure of four parts per million, or 0.0004 percent, is the amount of total uranium available in the earth's crust. On this basis, you can come up with the answer of about 1.5 times 10^{12} tons of uranium in the one-mile layer of earth's crust not covered by water. It can be pointed out on this line of reasoning that uranium is 1,000 times as plentiful as gold, 100 times as plentiful as silver, and almost as plentiful as lead or zinc.

To my mind, such an approach is meaningless except to indicate that uranium is an important material in the earth's crust and, accordingly, that it probably was present in the right places fairly often when geological concentrating processes were at work forming ore bodies. What counts in terms of available uranium is economically exploitable concentrations.

The second approach is to take some prewar figures of uranium production or, more generally, of radium production—calculate the amount of associated uranium from these, and assume that these figures and the ore-reserve data of that same prewar period are a valid measure of the potential production of uranium in the future. This appears to me an equally fallacious approach for the following reasons:

1. Even up-to-date ore-reserve data, whether they be for copper, gold, lead, uranium, or any other metal, mean very little unless one realizes that it has been proved historically time and again that such estimates merely show that only a few years of production are blocked ahead. It is not economically justified for most mining companies to spend more money in development than is necessary to maintain ore reserves for more than one to five years. It is often bad business to make capital investments in development work that will not yield returns for a long time to come. Moreover, in this country, mining companies have to pay taxes on their proved reserves, and this taxing policy inhibits unnecessary blocking out of reserves. Petroleum figures also

have periodically led experts to predict early exhaustion of our oil reserves. According to some early predictions, we should have run dry by now. Yet the American Petroleum Institute figures show an annual increase in the known reserves of petroleum in this country almost every year for a decade or more, despite increasing rates of production.

2. Also ignored or underrated in this approach are our growing technology and its future application to low-grade uranium sources. Most metals have gone through, or are going through, a cycle where high-grade deposits are at first the only commercial deposits, and then gradually large low-grade deposits yield to man's technical ingenuity and become important producers This is strikingly demonstrated in the case of copper in this country. Not so many years ago, you had to have 2 percent copper to make a mine. At the present time, one of our copper mining companies has issued a prospectus calling for the financing to the tune of many millions of dollars of a primary copper ore body averaging less than 0.8 percent copper to be mined by underground methods. The "mining" of sea water during the war as an "ore" of magnesium is probably the most spectacular example of such a development. The ore-reserve-production data approach thus omits from consideration the vast tonnages of marine sediments or by-product production from the South African gold-mining industry. That this latter may be a serious omission is indicated by the statement made in a recent speech by the Minister of Industrial Production of the Union of South Africa, who said, "We believe that we are able to say that the Union of South Africa may produce more uranium than any other country of the world."
3. Neglected by this approach also is the great surge of prospecting for uranium that is going on in all parts of the world. After all, man has only just begun to look for uranium Up until now it has merely been a by-product of the radium or vanadium business, useful in coloring pottery and artificial teeth. My guess is that new deposits will be found by this effort. Dr. W. F. James, of the Advisory Staff of the Canadian Atomic Energy Commission, in a recent talk before the American Institute of Mining and Metallurgical Engineers in San Francisco, announced that already three new properties in Canada are virtually certain to come into production, and that there are three or four promising new prospects in addition.

The commercial aspect of uranium should be kept in mind. The average uranium content of some granites is of the order of magnitude of 0.22 ounces per ton of rock. If we had that much gold in a large granite mass, we would consider it a very profitable mine. Of course, the price of gold is roughly 124 times that of uranium and, generally speaking, it is easier to extract. Nevertheless, if uranium extraction technology improves greatly, and if uranium is ever needed badly enough—i.e., its price is high enough—there will be a lot of it available.

It is pertinent to inquire, "What is uranium worth today to a mining company?" There may someday be established a world price for uranium comparable to but of different magnitude than the price of gold, but at present there is no such price. Foreign uranium is purchased at negotiated prices. The U. S. Atomic Energy Commission has established for domestic uranium a ten-year minimum price of \$3.50 per pound of contained U_3O_8 in a high-grade product, the Canadian government has established a roughly equivalent minimum price of \$2.75 per pound of contained U_3O_8 in Canadian ores and concentrates containing at least 10 percent U_3O_8 f. o. b. railroad, and the United Kingdom Ministry of Supply has offered to buy all uranium ores and concentrates produced in the Colonial Empire during a ten-year period at a minimum price of 13*s.* 9*d.* (approximately \$2.78) per pound of contained U_3O_8 delivered f. o. b. ocean port. Aside from these, I know of no other publicly announced prices for uranium since the development of atomic energy. As one can easily figure out, even a narrow pitchblende vein means high-grade ore at these prices.

Because uranium is everywhere controlled by the governments of the countries in which it exists, it is unlikely that it will ever appear on free competitive world markets in the sense that lead or copper does. In this connection, attention is called to the fact that the Commission's ten-year guaranteed minimum price for domestic uranium is in fact a minimum price. Where larger quantities are involved than the small lots for which this price was established, or under special circumstances, the Commission is prepared by negotiation to establish higher prices which will take into consideration special milling and refining costs, etc.

There is, moreover, at the present time a "subsidy" price in connection with our Colorado Plateau ore-buying program. There is a base price (including development allowance) of \$2.50 per pound of contained U_3O_8 in ores containing 0.2 percent U_3O_8, but payment is also made for the V_2O_5 content at 31 cents per pound, and a haulage allowance of 6 cents per ton mile is allowed up to a limit of 100 miles. Additional premiums are allowed for higher-grade material. It is hoped that private prospecting resulting from this program and the attendant exploration program of the government will sustain a mining industry and will add materially to domestic reserves.

For security reasons, I have had to discuss this subject without divulging much quantitative information as to known reserves or rates of production. For reasons already stated, however, information of this kind is insufficient to measure

future possibilities. In my judgment, the estimates of future uranium supplies that I have seen are far too pessimistic. I shall be content to rest my own case as a prophet on the prediction, based principally on mining experience with other metals, that there will be new high-grade uranium producers found, that the old producers will last longer than people think, that there will be significant uranium production from low-grade ores that are not now even considered ore; that we can get large amounts of thorium when and if we need thorium, and there will be enough source material to permit the use of atomic energy to expand considerably and to go on for generations.

What is being done in this country to provide for a continuing supply of uranium? During the war, the Manhattan Engineering District purchased foreign uranium from Canada and the Belgian Congo and extracted uranium from accumulated tailings of past vanadium operations on the Colorado Plateau. The Atomic Energy Commission took over the atomic energy project from the Army at midnight, December 31, 1946. The Commission has continued to buy foreign uranium, which to this day constitutes a high percentage of our total plant feed. The Commission on April 11, 1948, also announced a three-point program to stimulate the discovery and production of domestic uranium by private competitive enterprise. The major elements of this program are:

1. Government-guaranteed ten-year minimum prices of \$3.50 per pound of contained U_3O_8 for small lots of domestic refined uranium, and of \$3.50 per pound of recoverable U_3O_8, less refining costs, for small lots of ore or mechanical concentrates assaying at least 10 percent U_3O_8, both prices f. o. b. shipping point.
2. A bonus of \$10,000 for the discovery of a new deposit and production therefrom of the first 20 tons of uranium ore or mechanically produced concentrates assaying 20 percent or more U_3O_8.
3. Guaranteed minimum price for uranium-bearing carnotite-type or roscoelite-type ores of the Colorado Plateau area for the period ending June 30, 1954 (Circular No. 3, which extended through April 11, 1951, and Circular No. 4, which extended through June 30, 1949, were combined in new Circular No. 5 on February 1, 1949). Although no important change was made in the pricing provisions of circulars No. 3 and No. 4, several adjustments were made, and the period of the guarantee was extended for approximately three years in order to attract capital and mining development.

The Commission also has extensive exploration activities that are carried out largely by the U. S. Geological Survey. These activities include:

1. A comprehensive geological study and exploration of uranium-vanadium ores of the Colorado Plateau.

2. A comprehensive geologic study of the uranium-bearing phosphate and shale formations of the country.

3. A systematic examination of all mine dumps, mill tailings, smelter slag, and similar products for radioactive minerals.

4. A systematic study and frequent logging of oil and gas wellholes for evidences of radioactive material.

5. Reconnaissance studies of so-called geologically favorable areas for evidences of unusually radioactive materials.

6. Spot examinations of all reported uranium prospects believed to have some chance of being important.

7. Geologic reconnaissance of river placer and beach sand deposits to locate large potential sources of monazite for use if thorium becomes a fuel for nuclear reactors. (The U. S. Bureau of Mines is following after the Survey with engineering cost studies.)

8. Examination of pegmatites as possible sources of uranium and beryllium. (Beryllium is one of several substances that can be used as a moderator in an atomic reactor.)

9. Special studies in connection with existing or proposed nuclear reactor sites, such as Hanford, where there are big and complex problems associated with water supply and waste disposal.

10. Laboratory studies of many kinds involving the mineralogy and chemistry of uranium, and never-ceasing research on the improvement and development of radioactive instruments and techniques.

An equally important part of the Commission's study of uranium resources is its research program to develop new processes which can treat economically the very low-grade uranium materials I have described. Massachusetts Institute of Technology has played and is continuing to play an important part in this program. We are very optimistic about the outcome of this program, although there are many fascinating but difficult minerals-engineering problems yet to be solved. Incidental results of this program, which are nevertheless important, have been the development of new and improved analytical techniques and instruments.

The Atomic Energy Commission, required by the Atomic Energy Act, also licenses all transfers of source material after its separation from the place of deposit in nature. (The term "source material" means any material except fissionable material containing 0.05 percent or more of uranium and/or thorium.) Consequently, anyone buy-

ing or selling uranium or thorium ore after it has been mined must apply to the Commission for a license. Distributors and processors of source material are also required to fill out a simple form each month so the Commission can have a record at all times of where it is and where it goes.

What is the outlook in the uranium industry for students of mineral engineering? At the present time, probably not more than four hundred professional geologists, mineralogists, metallurgical and chemical engineers, and mining engineers are directly engaged in this country's raw materials' program having to do with uranium ores. Most of these are employed by the government agencies or by contractors having research contracts with the Commission. A comparatively small number employed by private companies are engaged in producing uranium and vanadium. It should be remembered, however, that this is an infant industry, which will grow if the present exploration efforts are successful. The program of the U. S. Atomic Energy Commission is firmly based on the assumption that new uranium deposits can best be found, developed, and worked by individuals and private companies seeking profits. Canada has embarked on a similar program emphasizing the opportunity for private enterprise. As new low-grade materials are developed as a source of uranium, it will be mineral engineers who develop them. I am optimistic as to the future. The uranium industry may never employ as many mineral engineers as the copper industry or the gold industry, but I strongly recommend that young mineral engineers, especially, maintain a constant professional interest in uranium as a new metal of enormous significance to this and succeeding generations.

TO A RAINSTORM

Fling down your curtain of splintered spears.
Release your barrage of explicit tears
That break with outraged sputtering
On grass and sea and stone. Here is
A face upturned, unsheathed; a need
Upon it for each clean, clipped bead;
An urge to feel this pristine thing
Born of no hand, no mill, no seed.

ROSE RICHMAN UNGER

ESP—FACT OR FANCY?*

ROBERT A McCONNELL

Dr. McConnell is assistant professor of physics at the University of Pittsburgh. In the field of electronics he has contributed to the development of pulsed Doppler radar and to the theory of the iconoscope. He began the study of the literature of extrasensory perception as relaxation from his wartime research duties, and has since become convinced that the evidence for this phenomenon is good enough to warrant serious consideration by scientists.

THE term "extrasensory perception" has achieved acceptance in the lay vocabulary largely because of the efforts of Professor J. B. Rhine, of Duke University. As a result, it is widely supposed that the phenomenon was discovered by Rhine and that no significant evidence for its existence has been obtained except in Rhine's laboratory. It will surprise many to learn that one might ignore the Duke work without seriously impairing the evidential status of the phenomenon.

SOME EARLY CARD EXPERIMENTS

The work begun at Duke University in 1930 was carried out by means of the "ESP card deck." That deck consists of cards printed with one of the five symbols shown in the accompanying illustration. The standard deck contains a total of twenty-five cards, there being five cards of each symbol.

At first thought, the use of playing cards for the investigation of extrasensory perception might seem a foolish procedure unworthy of a serious investigator. The long association of cards with frivolity and legerdemain does not help to dispel the deep-rooted skepticism that most scientists feel about telepathic phenomena. It is only upon closer examination that the elegance of the card deck as a research tool becomes apparent. Its merits are its susceptibility to experimental control and its sensitivity to slight traces of extra-chance causation. Experimental control is easy because of the simplicity and dependability of the "apparatus." The sensitivity of cards as a tool arises from the use of probability mathematics.

Card calls with the ESP deck can be treated as independent trials with constant probability. The binomial distribution applies with a small correction to the standard deviation necessitated by the fact that in the deck there are known to be exactly five of each symbol.[1] The adequacy *ad hoc* of the statistical methods used in the demonstration of ESP has been attested to by prominent mathematicians.† The mathematics of the cards has ceased to be a point at issue.

A number of outstanding experiments have been performed with the ESP card deck. One of the first of these in point of time and interest is the series of tests given to the subject Hubert Pearce with the cards in one building on the Duke University campus and with Hubert Pearce himself in another.[2] Dr. J. G. Pratt (then a graduate student in psychology) was the principal experimenter throughout the series. Dr. J. B. Rhine witnessed Pratt's handling of the cards in one subseries (in which the scoring rate was well above the average for the rest of the series). The experimenter and subject worked by synchronized watches and without means of communication. The above-described card deck was used: twenty-five cards, five each of five suits. Two runs (fifty trials) were made per day. The cards were shuffled just before using and, of course, with HP absent from the room.

Starting on schedule, once each minute Pratt would remove the top card from the deck and lay it face down on a book in the center of the table without looking at its face. The subject located in another building would record his "impression" of the card 30 seconds later. The cards were recorded by Pratt after completing the runs. Sealed records from the observer and subject were delivered directly to Rhine at the end of each sitting.

A total of 1,850 trials was made under these conditions. The expected mean score was 5 out of

* Publication 3p48 from the Physics Department of the University of Pittsburgh.

† Dr. B. H. Camp, former President, Institute of Mathematical Statistics; Dr. R. A. Fisher, University of Cambridge, England; Dr. J. A. Greenwood, Senior Statistician, U. S. Navy Bureau of Aeronautics; Dr. T. N. E. Greville, U. S. Bureau of the Census; Dr. E. V. Huntington, Department of Mathematics, Harvard University.

25. The actual mean score was 7.53 out of 25. This deviation is 10 8 times the standard deviation. The probability of such a score by chance alone is less than 10^{-20}.

This test was repeated by Dr. Bernard F. Riess, assistant professor of psychology at Hunter College, using another subject.[3-6] Riess began as a skeptic and undertook the following experiment after preliminary classroom trials had shown extra-chance results.

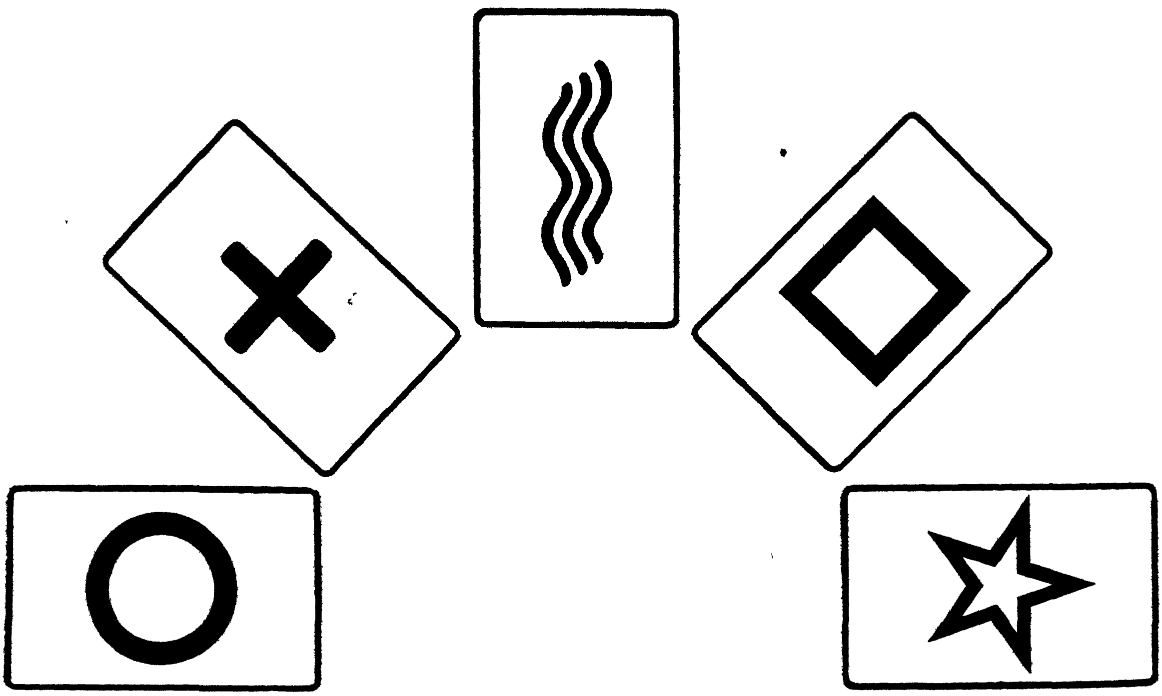
The subject, a Miss S., and the experimenter, Dr. Riess, worked in their respective homes in White Plains, New York. The houses were one quarter mile apart, and their rooms faced away from each other. Beginning at the appointed time, Riess exposed the cards from a shuffled ESP deck at one-minute intervals. He recorded each card as he exposed it, and the subject recorded her guesses on schedule. During the day following each session the subject's record was mailed or delivered to Riess. Fifty trials were made per evening over a period from December 27, 1936, to April 1937, until a total of 1,850 trials had been reached. The average score was 18.24 out of 25. There is no need to apply statistics to such a score to recognize its extra-chance character.

A third experimental series of great evidential strength[7] was that conducted by Dr. Lucien Warner, now professor of biology and psychology at Claremont Men's College (California). Warner arranged this test as a definitive answer to the critics of earlier work. The test took place on October 8, 1937, at Greenfield, Massachusetts.

The subject, MH, was locked in a room on the first floor. The experimenter and his assistant, Mrs. Mildred Raible, occupied a room on the second floor not directly above the subject's room. The doors to both the experimenters' and subject's rooms were closed throughout the experiment; moreover, the rooms were so oriented that even had they been on the same floor, one door could not have been seen by a person standing in the doorway of the other. The experimenters did not talk or whisper to one another during the course of the experiment. A one-way signaling device was provided whereby the subject could flash a light in the experimenters' room to signify that she had written down her guess and was ready for the next card.

The total number of trials, 250, was fixed in advance. Each card was cut from a freshly shuffled full deck; thus, the probability distribution was strictly binomial. The card face was exposed and recorded *after* the subject indicated that she had made her guess. The average number of hits was 9.3 per 25. This score gives a deviation 6.8 times the standard deviation. The probability that this might occur by chance is about 0.000,000,000,064.

There have been other researches[8-10] from this same period whose experimental adequacy is



The "ESP deck" consists of five cards of each symbol—a total of twenty-five cards.

unassailable but not readily depictable in the space of a few lines. Reading the literature for the first time, the serious student will be surprised at the number and quality of the experiments that bear testimony to the existence of ESP.

RECENT ADVANCES

It is sometimes asked why there have been no more Riess and Warner cases in recent years. Such high-score tests under rigid conditions prove only one thing: they prove the existence of an anomaly in science. This anomaly has been given the name extrasensory perception. There are many people for whom the repeated observation of a strange phenomenon is not enough. They must acquire some degree of understanding before they can accept it as real.

In 1938 the evidence for ESP was already extensive. When the American Psychological Association held a symposium on ESP at Columbus, Ohio, two things were made clear: (1) The critics could find no fault with the many well-done experiments; and (2) the majority of psychologists were still unconvinced of the reality of this anomaly. The parapsychologists, as they have called themselves, then decided that all effort should be concentrated on learning more about ESP and that none should be wasted in further attempts to obtain high scores. The research results since that date have rarely been spectacular, but they are exciting and, in their own way, even more convincing than what was known before.

Despite the difficulties interposed by World War II, knowledge of parapsychology branched in several new directions. Recent research advances can be classified under four headings:

1. The systematic study of success-frequency trends within experiments.
2. The discovery of a covariation of sporadic ESP ability when a subject tries several ESP tests in immediate succession.
3. The discovery of a consistent misdirection or displacement of ESP aim to targets just before or after the desired card.
4. The correlation of ESP scoring ability with other psychological measures of personality.

SCORING SALIENCE

Estabrooks[11] reported from Harvard in 1927 that his subjects scored lower in the second half of a test consisting of twenty trials than in the first half. Through the years which followed it was gradually recognized that the rate of success in ESP experiments was likely to follow typical patterns. At the beginning of an experiment the scores were generally highest. Moreover, any one subject tended to improve temporarily with any change in the testing routine, however slight: a new size of card symbol; the beginning of a new data page; any slight change in the scoring procedure. Anything that introduced an element of novelty was found to raise the score—until the novelty wore off. It was found, too, that in those experiments in which the end of the task was given psychological prominence the scoring tended to rise as the end approached. A long list of investigations showed these effects.

Finally, it occurred to someone—or perhaps to a number of people simultaneously—that here was a new kind of evidence for ESP. Here was a secondary effect whose occurrence was a mark of authenticity in ESP data.

A systematic investigation of these effects was presented by Rhine[12] in 1941. Since that time the analysis for "salience effects," as they are called, has become a routine part of the treatment of ESP data. The major significance of salience effects in ESP derives from the fact that they are well known in many experiments in orthodox psychology. They are accepted as characteristic of the operation of the human mind in certain kinds of situations.

COVARIATION OF ESP ABILITIES

The covariation effect was first reported by Gardner Murphy and Ernest Taves of Columbia University[13] in 1939. ESP is known to be a sporadic and undependable ability. It occurs only with some subjects, only for some experimenters, and only at certain times. Under unfavorable experimental conditions it sometimes even reverses itself, giving extra-chance low scores when high scores are desired. Knowing these difficulties, one might ask the following question: Suppose a subject is given two tests at the same sitting. Suppose, for example, that he is asked to guess through an ESP deck and then immediately thereafter to identify cards from an ordinary playing card deck. Will he do well on the second task on the same days that he does well on the first?

In the tests of Murphy and Taves each subject was given four different tasks at each sitting. The subjects were picked more or less at random with no regard for their ESP scoring ability. Even though the final ESP scores were not significantly above chance when considered by themselves, it was found that a significant scoring correlation between tasks did exist;‡ that is to say, a poor ESP subject may still have occasional flashes or spurts

‡ This particular study should not be regarded as unquestionable evidence for ESP, since a rigorous evaluation of the data is beyond present-day statistical methods. *See* page 170, reference 1.

of ability that can be identified by his simultaneous improvement in several ESP tasks.

DISPLACEMENT

The third of four new kinds of evidence for ESP is the "displacement effect" discovered by Whately Carington and studied at length by S G. Soal and K. M. Goldney. The story behind this work is one of considerable human interest.

For five years, beginning in 1934, Dr. Soal had sought unsuccessfully for a subject who could demonstrate ESP using the Duke card deck. Soal was a teacher of mathematics at the University of London, and most of his tests were conducted at that University. During the period in question he had tested 160 subjects and recorded 128,350 guesses. It was natural under such circumstances that he should have been suspicious of the successful work done in the United States. His freely expressed criticisms and doubts are now a matter of record.

Meanwhile, Whately Carington, working in Cambridge, England, had made an unusual discovery. Using drawings as ESP test material, Carington found that subject's reproductions tended to correspond sometimes with the originals for which they were intended but more often with other originals presented in the same series. Carington thereupon suggested to Soal that he rescore his unsuccessful card tests, not for the intended card, but for cards immediately preceding and following. In November 1939, after repeated urging, Soal did just that. To his surprise, he found two out of his 160 subjects who showed an extremely extra-chance displacement score.[14] During 1941–43, Soal and Mrs. K. M. Goldney carried out a study[15] with one of these two subjects, Mr. Basil Shackleton. The probability of chance as an explanation for the results obtained has been calculated as 10^{-35}.

This two-year study has many features of interest. The experimental conditions were ironclad both as to sensory cues and recording errors; moreover, throughout the tests there were always two or three witnesses present in addition to the agent and the subject. This study has been reviewed in greater detail by G. E. Hutchinson in *The American Scientist*.[16] The subsequent criticism of that review[17] offers considerable insight into the scientific status of extrasensory perception.

ESP AND PERSONALITY

The fourth kind of new evidence for ESP is undoubtedly the most important in terms of future promise. ESP ability is a vagrant, unpredictable thing. Why do some people have it and others not? Are good subjects more prevalent among the young or the old, among men or among women? Is there perhaps something peculiar about people who can guess cards correctly? Is racial origin important? Do the insane have ESP ability? These and many similar questions have been asked. For the most part, the answers have been disappointing, although here and there suggestive results which need further investigation have occurred. Recently, however, a vein of pay dirt has been struck, so rich as to attract the major part of present-day investigative effort.

There are in psychology a number of tests designed to measure various aspects of human personality. The best known of these is the Rorschach ink-blot test. Another is the Elkisch expansion-compression measure of personality adjustment. At Duke University Dr. B. M. Humphrey has discovered[18] that in certain ESP drawing experiments, "compressive" subjects show a backward (post-cognitive) score displacement, whereas "expansive" subjects score best on the intended drawing. Similarly, at the College of the City of New York, Dr. G. R. Schmeidler has found[19] that ESP scoring ability can be correlated with several categories of the Rorschach test. The interpretation of these correlations is as yet far from clear, but as evidence for the reality and—even more important—the *normality* of ESP in the personality of the individual, they are landmarks in the growth of a new science.

The evidence for ESP is too extensive to be compressed into a single review. But, so far as it goes, what might one conclude from the above discussion of the literature of this subject? Does extrasensory perception occur?

To set this question in proper perspective it is desirable to refer to the definition which has been given to the term "ESP." Many people bear hostility to the expression "extrasensory perception," which is not justified by the limited meaning that has been attached to it by Dr. Rhine and his followers. In the glossary in the back of every issue of the *Journal of Parapsychology* this definition appears: "ESP: response to an external event not presented to any *known* sense." (Italics added.)

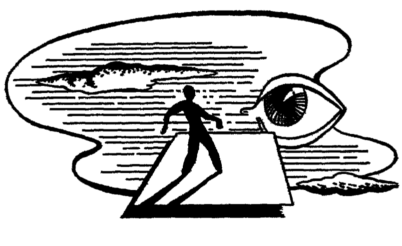
This definition does not imply anything about the nature of ESP beyond mere occurrence. To say that this effect occurs is only to say that there is no counterexplanation for those experiments

that have been offered as evidence. If it be tentatively assumed that the foregoing presentation has been fair and adequate, then is it possible to avoid the tentative conclusion that ESP does occur within the limited meaning of its definition? Beyond the question of mere occurrence many others might be asked. How does ESP tie into the current body of science? Is it *physical* in the sense that it has some lawful dependence upon time and space? Such questions lie beyond the scope of the present discussion. Although by now there is considerable evidence bearing upon these and similar questions, it can hardly be called conclusive in the sense that the evidence for the *occurrence* of ESP is conclusive.

Perhaps for most scientists it will be enough to know that here is a field for research which, like evolution and relativity before it, is slowly gaining acceptance. It is a new field, with new hopes and new opportunities. It will attract the attention of young men who are not afraid to invest their efforts in a speculative venture.

REFERENCES

1 For a comprehensive review of all ESP work and criticisms prior to 1940, *see:* Pratt, Rhine, Smith, Stuart, and Greenwood. *Extrasensory Perception After Sixty Years.* New York: Henry Holt, 1940.
2. Rhine, J. B. Some Selected Experiments in Extrasensory Perception. *J. Abnorm. Soc. Psychol.*, 1936, 31, 216–28.
3. Riess, B. F. A Case of High Scores in Card Guessing at a Distance. *J. Parapsychol.*, 1937, 1, (4), 260–63.
4 Riess, B. F. The ESP Symposium at the A. P. A. (statement from the floor). *J. Parapsychol.*, 1938, **2**, (4), 270.
5. Riess, B. F. Further Data from a Case of High Scores in Card Guessing. *J. Parapsychol.*, 1939, 3 (1), 79–84.
6. Britt, S. H. Possible Explanations of Two "Inexplicable" ESP Experiments. *Psychol. Bull.*, 1940, **37**, 629–30.
7. Warner, Lucien. A Test Case. *J. Parapsychol.*, 1937, 1, (4), 234–38.
8. Martin, D. R., and Stribic, F. P. Studies in Extrasensory Perception: I. An Analysis of 25,000 trials. *J. Parapsychol.*, 1938, **2**, (1), 23-30.
9. Martin, D. R., and Stribic, F. P. Studies in Extrasensory Perception: II. An Analysis of a Second Series of 25,000 Trials *J. Parapsychol.*, 1938, **2**, (4), 287–95.
10. Pratt, J. G., and Woodruff, J. L. Size of Stimulus Symbols in Extrasensory Perception. *J. Parapsychol.*, 1939, **3**, (2), 121–58.
11. Estabrooks, G. H. A Contribution to Experimental Telepathy, *Bull. Boston Soc. Psychic Res.*, 5, 1–30.
12. Rhine, J. B. Terminal Salience in ESP Performance. *J. Parapsychol.*, 1941, **5**, (3), 183–244.
13. Murphy, G., and Taves, E. Covariance Methods in the Comparison of Extrasensory Tasks. *J. Parapsychol.*, 1939, **3**, (1), 38–78.
14. Soal, S. G. Fresh Light on Card Guessing—Some New Effects. *Proc. Soc. Psych. Res.*, London, 1940, **46**, Pt. 162, 152–98.
15. Soal, S. G., and Goldney, K. M. Experiments in Precognitive Telepathy. *Proc. Soc. Psych. Res.*, London, 1943, **47**, Pt. 167, 21–150.
16. Hutchinson, G. E. Marginalia. *Amer. Sci.*, 1948, **36**, (2), 291 ff.
17. Skinner, B. F. Card Guessing Experiments. *Amer. Sci.*, 1948, **36**, (3), 456 ff.
18. Humphery, B. M. Success in ESP as Related to Form of Response Drawings: I. Clairvoyance Experiments. *J. Parapsychol.*, 1946, **10**, (2), 78–106.
19. Schmeidler, G. R. Rorschach Variables in Relation to ESP Scores. *J. Amer. Soc. Psychic. Res.*, 1947, **41**, (2), 35–64.



SCIENCE ON THE MARCH

RECENT DEVELOPMENTS IN CUT FLOWER STORAGE AND SHIPMENT

CUT flowers, in common with most other horticultural products, are a perishable commodity, and the essentially transient character of many flowers has been a deterrent to their use on a large scale. The availability of rapid transport in recent years has caused an increased interest in the development of new methods of storage and shipment. Air transportation has stimulated the commercial production of outdoor-grown cut flowers in California, Florida, Hawaii, and other subtropical and tropical regions, but since some flowers are rather heavy in proportion to value, they are, accordingly, not well suited to long-distance transportation. In general, air transport does not involve serious problems due to altitude effects.

A considerable volume of detailed scientific information is available on the physiological processes of maturation during the commercial storage and transport of fruits and vegetables; very little is known, however, about the corresponding processes in flowers, except in barest outline. The course of respiration of flowers has not been charted accurately, and the relationship of respiration to keeping quality is not certain.

Flowers should be cut and handled in a manner that will prevent plugging of vascular elements. Sometimes the stems are too small or hard to permit the free passage of water; in such cases, splitting or mashing the bases of the stems is practiced. Weak disinfectants are used to prevent plugging due to bacteria and fungal action or to delay tissue deterioration, but these are not always beneficial. Vascular plugging may result from air bubbles, and in such cases wilted flowers can be revived by cutting off the stems under water. Hamner, Carlson, and Tukey have shown that some flowers may have their keeping qualities enhanced by immersion under water in a vacuum, a treatment which fills the intercellular air spaces with water.

Transpiration of flowers may be reduced by coating with certain materials, either by dipping or spraying; for example, a paraffin wax emulsion has been used by florists as a means of prolonging the life of floral arrangements under adverse conditions. Recently workers at Michigan State College have reported that certain flowers lasted longer when coated with a water-dispersible polyvinyl plastic. This material was not detrimental to the natural appearance of white flowers such as gardenias, and there was only partial loss of fragrance. The plastic was also shown to be a valuable aid in the preservation of green decorative foliage and Christmas trees.

One of the most commonly used methods of reducing the respiration rate of flowers is lowering the temperature by refrigeration. The optimal temperatures vary according to the kind of flowers, as might be expected. A few tropical flowers, such as certain orchids and anthuriums, are affected adversely when subjected to temperatures lower than 55°; most flowers keep quite well, however, at temperatures of 38°–42° F. To maintain reduced temperatures during shipment, refrigeration is provided by a combination of ice and solid carbon dioxide. A recent commercial development for refrigeration consists of frozen wet sawdust, combined with a hygroscopic salt, which provides the necessary low temperatures without leakage of water.

Another advantage of lowered temperatures is the reduction of ethylene production. Ethylene in very low concentrations has been shown to be the cause of fading in orchids, "sleepiness" in carnations, and other injuries to flowers. Trouble has sometimes arisen owing to the escape of artificial illuminating gas, which contains ethylene; common storage with ripening fruits which produce ethylene is dangerous. Flowers also produce ethylene, but little is known of the physiological effects of these emanations on the flowers.

Modified atmosphere storage using reduced oxygen tension and increased carbon-dioxide tension has aided the storage of certain fruits. Thornton, at the Boyce Thompson Institute for Plant Research, demonstrated that the use of an atmosphere with increased CO_2 concentration (5–15 percent) also prolonged the life of such cut flowers as gladioli, snapdragons, cosmos, dahlias, and carnations. The treatment was effective only on buds, and even in the bud stage some flowers showed no response. (High concentrations of CO_2 (30–80 percent) can change flower color.) The use of carbon dioxide has been successful in experimental studies on air transport in Europe and has been used in intercontinental shipments; the large air-

shipment industries in the United States, however, do not at present use it.

Another successful method of aiding flower storage is the use of sugar solutions to provide a substrate for respiration. This has been combined with a disinfectant to suppress microorganisms. Such preparations have had an extensive use by florists and have been used by wholesale florists on many flowers as a preshipping treatment. Neff has indicated that salts of cobalt, bismuth, lead, uranium, tin, and molybdic acid in sugar solutions mordant the flower color pigments, preventing fading or changing of color. By this means, flower colors may be stabilized.

Some preliminary studies with phytohormones such as alpha naphthalene acetic acid and similar compounds indicate that flower life may be prolonged and shattering prevented through their use.

Neff and Loomis showed the practicality of storage of flowers in sealed containers under refrigeration. A decade elapsed, however, before the method was developed for commercial use. Flowers stored in a very humid atmosphere with their stems not in water had a lower rate of physiological processes in comparison with flowers with stems in water. The florist industry has recently introduced prepackaged flowers. This combines refrigeration with dry storage in a humid atmosphere modified by the respiration of the flowers. The development work has been done by Laurie and associates at Ohio State University and Krone at Michigan State College. Usually the flowers are packed immediately after cutting, but in some cases a short period with stems in water is advised. The flowers are placed either in moisture- and gas-tight cellophane bags or in waxed boxes wrapped with cellophane, fastened securely to prevent bruising in transit, and the cellophane heat-sealed. The result is an attractive package which will stand handling and in which the flowers are well displayed. Studies at Ohio State University have shown that many kinds of flowers can be kept in sealed containers for 5–8 days at 40°–42° and after removal to room temperature have a normal life. Prepackaging offers possibilities for large-scale marketing of flowers at reduced costs.

In order to promote standardization, Post, of Cornell University, has recently proposed a flower-grading system based upon weight. This weight-grading system is rapidly being adopted by the floral industry.

The numerous innovations in recent decades have been worked out largely on an empirical basis. The advances in fundamental physiological knowledge of floral behavior, which may reasonably be expected, should contribute to further progress in this important, rapidly growing industry.

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RADAR DEVELOPMENTS IN GREAT BRITAIN

BRITISH radar, radio, and atomic scientists are working in a new field of research based on millimeter radio waves. This basic research is proving to be one of the most useful ways of investigating fundamental problems of the constitution of matter, and is likely to produce significant developments. Work on different aspects of this field is being carried on at the Clarendon Laboratory, Oxford, under the direction of Lord Cherwell, wartime scientific personal assistant to Winston Churchill; at Britain's Telecommunications Research Establishment; and at the Signals Research and Development Establishment.

A little more than one hundred years ago, the remarkable Michael Faraday, working in his laboratory in Britain's Royal Institution in Albemarle Street, London, discovered how light may be affected by magnetism. He was then fifty-four years of age. Some years later, at the age of seventy-one, he tried to discover whether a beam of light could be refracted by a magnetic field. He failed to detect any refraction, since the technique both of spectroscopes and of magnetic fields was insufficiently developed at that time. His last entry in his notebook was, "Not the slightest effect on the polarized or unpolarized ray was observed."

Thirty-five years later, Pieter Zeeman, in 1896, observed that the lines of the sodium spectrum broadened when the source of light was placed in the magnetic field of a powerful electromagnet. Using more powerful fields, he resolved single spectral lines into two lines. Clerk Maxwell's electromagnetic theory of light held that the system of electromagnetic waves must be emitted by vibrating electric systems, which today means atoms, protons, electrons. Thus the spectrum of an element is a window through which the single atoms in the group forming a molecule may be watched. From the data given by Zeeman's separation of spectral lines a new value for e/m, the ratio of the

electric charge to the mass of the electron, was obtained. This showed that the electric particles were identical with Sir J. J. Thomson's electron, whose existence he had proved from his cathode-ray tube experiments.

The Zeeman effect has played a great part in the development of our knowledge of atomic structure and of the energy levels of the electrons in their elliptical orbits around the atomic nucleus. So long as optical methods were used for examining the Zeeman effect on the spectra of elements, however, it could only be observed for those elements that could be excited to give out light when comparatively few were enclosed, as gaseous atoms or molecules, in an evacuated electric discharge tube. Thus the splitting of the atomic energy levels (the Zeeman effect) was restricted to the observation of comparatively few of the elements.

But radar has come to the rescue—the radar which was used during World War II to locate enemy aircraft and which enabled Britain's Fleet in the Mediterranean to come upon the Italian Fleet unobserved and destroy it. At the Clarendon Laboratory short-wave radar is being employed to allow the Zeeman effect to be observed in solids such as copper sulphate and thin metallic films. This is tied up with the absorption of very short radar waves (of millimeter wave length) by magnetic fields. Thus it has not only been shown directly by experiment that wireless waves are really streams of low-energy photons, but also that the window into the atom created by the Zeeman effect can be opened for solids as well as gases. It opens up a new and fruitful method for investigating the solid state, and already useful information has been obtained about the electromagnetic fields of force in crystalline solids. The interaction of the regularly spaced atoms in a crystalline solid broadens and displaces the absorption spectrum for a given substance. By measuring this effect, a picture of the manner in which the atoms of a crystal are influencing one another is obtained. Indeed, this particular method of using radar spectra affords one of the few ways by which the exchange forces in a compound may be studied in detail. Thus a further, most useful understanding of the total effect of the individual electric and magnetic fields of the atoms in a crystal is being obtained. These exchange forces are those which play so important a part in the chemical binding of elements to form a molecule.

A knowledge of the magnetic interaction of the atoms in a crystal is particularly important if such substances are to be made use of in reaching temperatures close to absolute zero. One of Faraday's great achievements was to discover the existence of paramagnetic substances. These are substances, which, if suspended between the poles of a magnet in a horizontal plane, will swing around until their longest axis is along the line connecting the poles of the magnet. In principle, it is always possible to cool a paramagnetic substance by first of all magnetizing it in an external field, insulating the substance so that it is thermally isolated, and then removing the electromagnet. The practical success of this method depends upon how quickly the entropy lost by the magnetized atoms can be abstracted from the lattice. Recent experiments show that this interchange can take place very quickly, even when the induced magnetism in the crystal is due to the very small magnetic moment of the atomic nucleus. The nucleus may be regarded as a rotating sphere, as is the earth, and therefore as possessing magnetic poles. These poles naturally enter into the magnetic forces in the crystal. The ultimate aim is to decrease the entropy to as low a value as possible, so that a close approach can be made to the absolute zero.

Experiments being carried on at the Clarendon Laboratory will, if successful, lead to the attainment of much lower temperatures than have ever been reached before. The approach will be made by steps getting within hundredths, thousandths, and perhaps within ten thousandths of a degree centigrade of absolute zero. The Clarendon is now installing a 1,000-horsepower generator to produce the power for the necessary magnetic fields for the experiment. But the most interesting point is that the necessary knowledge of the magnetic fields of force inside crystalline solids has been gained only by using very short radio waves instead of the "light" section of the electromagnetic spectrum.

During the past two years, much of the fundamental work on millimeter-wavelength radio waves was done at the Telecommunications Research Establishment, known everywhere as T.R.E., the world center of radar. This has meant the construction of minute klystrons, which have a power output of 10–20 milliwatts.

With the miniature version of the magnetron invented by Randall and Boot—whose mass production was worked out by one of Britain's principal electric companies—peak outputs of 10–20 kilowatts, with a pulse duration of one fifth of a microsecond, can be generated in the 8–9-millimeter wavelength section. These magnetrons are inconveniently small, however, and therefore small errors in construction are too noticeably magnified, so another source of millimeter radio waves is being fully investigated. This new source is the

The Clarendon Laboratory, Oxford, from Broad Street.

corrugated wave guide, which is a carefully constructed pipe, usually of rectangular cross section. Fitting across the guide are metal teeth, which are almost, in height and width, of the same dimensions as the cross section. Accurately machined cavities between the teeth proceed down the wave guide and act as equally spaced resonators, or klystrons in series. When the radio wave of the wave length that resonates with the cavities is transmitted down the wave guide containing the corrugations, it is slowed down; then if electrons, fired at their new velocity, are sent down the wave guide over the corrugations, they give some energy to the slowed-down waves and increase the power of the transmission.

It is this convenient source of millimeter waves that is making possible further interesting investigations into the behavior of atoms and molecules. The millimeter radio waves are in the small section of the electromagnetic spectrum, which merges into millimeter heat waves on one side and into the centimeter-wavelength radio waves on the other. Thus they have atomic and molecular effect rather similar to heat waves. For example, when millimeter waves are transmitted into the atmosphere, a series of absorption bands is noticed. At 1.25 centimeters wave length the absorption is very marked. The energy of the transmitted wave has been quickly used up in energizing the water-vapor molecules that are present. At a wave length of 5 millimeters, the oxygen molecules absorb; but between 8 and 9 millimeters there is a window in this atmospheric "wall against radar," and millimeter waves may be transmitted with little absorption.

The useful information about molecules is obtained from what is known as the "critical frequency." This is, in effect, the frequency which will be absorbed under certain standard conditions. The mass of atom or molecule, together with various magnetic and electric forces, will allow only a maximum amount of energy to be absorbed, which with the water molecule was, as has been seen, represented by the wave length of 1.25 centimeters, and represents photons of a certain low energy.

With gases the position is further complicated, since the waves cause not only vibrations but also spin the molecule. With ammonia gas, which consists of three hydrogen atoms arranged at the corners of the triangular base of a pyramid, with the nitrogen atom at the apex, the radio-wave energy is used up largely in moving the nitrogen atom from one side of the base plane to the other.

A practical application of absorption is in meteorology. Since the amount of water vapor in the atmosphere has a considerable effect on the dielectric constant of air, a measure of the amount of absorption in a given length of wave guide filled with the moist air, when compared with a similar guide filled with dry air, gives a very accurate determination of humidity.

Since this work deals with wave lengths on the infrared fringe, experiments to refract millimeter waves as light is refracted by glass and other transparent substances have naturally been made. Solid lenses of glass, "Perspex," or other dielectrics, or nonconductors of electricity, must, however, be of very large diameter, and therefore very heavy. A new technique eliminates this difficulty. Instead of, say, a convex lens of solid glass, flat plates of aluminum are stacked apart in parallel planes, as in a radio tuning condensor, but with their exposed edges curved so that, altogether, they form the skeleton of a convex lens. Thus, in the future, it appears that millimeter waves will be guided by a system of wave guides and lenses. This will eliminate much of the trouble where the flanges of two wave guides meet.

Manufacturing limits must be so fine that the slightest unevenness must be avoided. Indeed, the necessity for working with miniature equipment, which allows only very small tolerances in its manufacture, has resulted in a search for new systems of transmission, generation, and detection of millimeter radio waves.

Millimeter radio waves have captured the imagination of Britain's radar, radio, and atomic scientists and are on the way to becoming one of the most useful weapons in the attack on the constitution of matter It is remarkable to realize that this new field of basic research began when, in 1924, Sir Edward Appleton proved for the first time, experimentally, the existence of the Heavyside-Kennelly Layer and of his own Appleton Layer by using the reflective property of long-wave electromagnetic or radio waves. Then, in 1935, came the early experiments, in Britain, on systems to locate and detect aircraft. From detecting vast agglomerations of countless billions of atoms called aircraft, the system has been refined to determining molecules and to allow peering into the inside of crystals, determining facts that are outside the possibilities of long-established X-ray crystallography, founded some forty years ago by Sir William Bragg.

Now, it seems, resonating gas molecules may be useful, in turn, in creating such fine control of the frequency of transmitted radio waves that today's vibrating quartz-crystal methods may be regarded as providing only a rough control. This is likely to come from the work being done at the Signals Research and Development Establishment, near Bournemouth, Hampshire, England. Gases are being vibrated by resonance between the poles of very strong magnets. This work began only in the spring of 1948, and is of such a fundamental nature that it will be some little time before full results are available.

The first-class work that is proceeding, and that has been done, in Britain since World War II in the wide field of radar has already astonished visiting scientists from other countries. And yet it is but the logical development, through generations, of the work of those two great geniuses of a century ago, Michael Faraday and James Clerk Maxwell.

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METHODS OF MEASURING FLAVOR QUALITY IN FOOD PRODUCTS

THE measurement of the quality of flavor in food products is complex, since it involves subjective responses. Three distinct senses—taste, smell, and feeling—are involved in the tasting of foods. When food is taken into the mouth, the taste buds, located chiefly on the tongue, are stimulated; if the food contains volatile odors, the odor detection area located high in the nasal cavity immediately below the eyes quickly perceives the sensation, and the sense of feeling detects differences in texture of foods and cooling, burning, puckery, and stinging sensations caused by some products. It is common knowledge that the true sense of taste discerns well the flavor of substances such as sugar, salt, lactic acid, and quinine. Such products as fruit and coffee, however, depend more on the sense of odor than true taste. Texture is closely associated with flavor, and undesirable

texture is detrimental to the flavor quality of foods. Texture may influence flavor in another way. A thin liquid of strong taste will seem much weaker in flavor if a thickening agent is added; apparently the increase in thickness mechanically affects flavor perception. Although food tasters are conscious of the distinct senses involved in flavor evaluation, at the time of comparing samples they are more concerned with the quality of the flavor than the responses involved.

Fine flavor is a major goal of all attempts to develop new food products. Lack of dependable objective methods for evaluating flavors has led to the wide use of systematic tasting by experienced personnel, using the so-called organoleptic tests. With the development of new and improved foods, the public is becoming more discriminating in the selection of foods on the basis of flavor. As a consequence, food processors, experiment stations, and other public and private institutions concerned with food quality are placing a great deal of emphasis on the development of flavor evaluation procedures. In recent years, the problem has been extensively studied, and many articles concerning the organoleptic evaluation of foods have been published. In many instances in the past, too much reliability has been placed on the judgment of one, two, or three persons relative to the flavor quality of products.

Since tasting for flavor requires extreme concentration, it is necessary to have a suitable room to carry out the tests. The tasters should be able to sit down and have the samples placed before them on a table that is at least partially partitioned from the person next to them. The room should be free from odors and outside disturbances, and the tasters should be comfortable and relaxed. The three distinct senses telegraph to the brain simultaneously the perception of any particular flavor, and one may fail to get the message, or may get it incorrectly, without concentration on the job in hand. Decisions are very easily influenced by comments of others. The room should be well enough lighted to make color comparisons of some products; in other instances differences in color need to be covered up so that they will not influence flavor judgments. Red darkroom bulbs are satisfactory for this purpose.

The number of samples that can be tested without fatigue, the temperature of samples, and time of making tests must all be given consideration. Fairly large numbers of samples of products such as milk, coffee, and tea can be tasted by trained tasters during a day, without excessive taste fatigue; of course, a rest interval is necessary between each set of samples to allow the taste buds to recuperate. On the other hand, after tasting a few samples of jellies and jams or strong-flavored foods, differences in flavor very quickly become difficult to distinguish.

No definite conclusions have been drawn as to the most desirable temperatures for the organoleptic examination of products. There is a general feeling that the product should be examined at the temperature at which it is normally served. There are obvious exceptions to this rule, especially where the tests involve foods that are consumed at relatively extreme temperatures. Bengtsson and Helm[1] state that the optimum temperature for the perception of taste is generally considered to be about 20° C or somewhat higher and that, when 50° C is reached, the gustatory nerves cease to function.

Midforenoon and midafternoon are generally considered to be very satisfactory times for conducting taste tests.

The selection, methods for handling, processing, packaging, and storage of foods in relation to quality of their flavor are problems which are also of great concern to food processors and research laboratories. The flavor evaluation panel is an analytical tool to guide them in their research in the development of foods with the best flavor qualities. Careless use of the tool may result in useless or misleading results. If a systematic procedure is used, however, practical results may be obtained. For most organoleptic tests, a numerical scale should be set up for evaluating the quality of a product. It is a guide for the tasters scoring products and can be used to advantage in the statistical analysis of the results.

Plank[2] discusses types of scales used and believes that it is important that numerical grades for any single food property should be proportional to the quality expressed in descriptive terms. For example, a scale of 1 to 10 has been found to be very satisfactory. A sample rated excellent is given a score of 10; very good, 9; good, 8; slightly good, 7; fair, 6; fair minus, 5; slightly poor, 4; poor, 3; very poor, 2; and extremely poor, 1.

People in many walks of life can become good flavor testers. However, they must be selected on the basis of their ability to differentiate flavors and to repeat flavor judgments, and it is important that they have good flavor memories. Not all tasters do well on all products, so it is advisable, if possible, to make a differential grouping of tasters to use on products on which they do the best work. Since people do not react the same to foods from day to day, it is unwise to depend on

the judgment of two or three persons. It is most important that good tasters have the opportunity to gain experience by frequent participation in the flavor evaluation of specific foods.

If a large number of individuals are available in an organization, it is advisable to make use of as many of them as can be conveniently handled. Careful written records of all tests should be kept. A triple comparison method might be used as a good systematic procedure whereby two identical samples of either the unknown or control are included in the series, and one additional sample—the problem here being to select the one sample different from the other two. Such a test is very helpful in the selection of tasters and should be repeated several times as a thorough check on their ability. With this test, fifteen to twenty persons with the highest percentage of correct answers can be selected for a specific test at hand. Obviously, the scoring is all done without the tasters knowing what the individual samples are; they should know the general purpose of the test, however. For example, it may be desired to determine the effect of storage times and temperature on a product, or temperature and period of heating, or variations in sweetness.

The following test will illustrate a triple comparison procedure, used by the Food Science and Technology Division of the New York State Experiment Station, to evaluate flavor changes in good-quality commercial strawberry preserves resulting from times and temperatures of storage.

1. Control stored at 34° F., for 14 weeks.
2. Stored at 60° F., for 14 weeks.
3. Stored at 100° F., for 14 weeks.

In one test each person making the comparison was given two samples of the control and one sample of the strawberry preserves stored at 100° F. In another test, each person was given one sample of the control and two samples of the preserves stored at 60° F. for fourteen weeks. Each sample was designated by a series of letters or a series of combinations of numbers and letters as a code. In this way those working next to each other in a room would have the same sample with different code numbers, which would discourage a comparison with their neighbors. A summary of the scores taken from the score sheets is given in Table 1.

The results of the scoring for this first comparison indicate that there is a significant difference in flavor between the control and the sample stored fourteen weeks at 100° F. Likewise, the scores show a 100 percent preference for the control over the sample stored fourteen weeks at 100° F. Only scorer N indicated a significant difference in the control samples by the score of 9 and 7 for the same sample; none of the others varied more than 1 point difference for the control samples, and nine of the others gave the control samples the same score.

The sample stored at 100° F. for fourteen weeks was considerably darker in color than the control and had developed a cooked flavor but no objectionable off-flavor. The scoring was done in a darkened room to cover up the difference in color of the samples

TABLE 1

FLAVOR SCORES OF STRAWBERRY PRESERVES

Panel	Comparison I			Comparison II		
	Control	Control	14 weeks at 100°F	Control	14 weeks at 60°F	14 weeks at 60°F
A	7	8	4	8	7	4
B	9	9	6	8	9	9
C	9	9	5	10	10	9
D	9	10	4	8	9	5
E	9	9	8	8	9	8
F	8	9	4	8	9	6
G	9	9	4	8	6	6
H	7	8	5	9	8	7
I	9	9	7	9	9	9
J	10	10	9	10	10	10
K	10	10	4	9	10	7
L	10	10	6	8	10	10
M	9	8	4	8	6	6
N	9	7	3	–	–	–
O	9	9	5	9	9	9
Total	133	134	78	120	121	105
Average	8.9	8 9	5 2	8.6	8.6	7.5

The results of the scores on the second test do not indicate a significant difference between the control and the samples stored at 60° F. for fourteen weeks. Scorers A, D, F, and K indicated a significant difference by their scores of two identical samples. They were either guessing, or their flavor judgment was off for that test. Throwing out these sets of scores, the average scores for the samples would be 8.7, 8.6, and 8.3, respectively. Only two of the fourteen scorers indicated a significant difference between the control and the fourteen-week samples. There might have been a slight difference in flavor of the control and stored sample, but the difference was not significant or great enough for the scorers to detect.

A paired comparison procedure used in obtaining a preferential rating of several lots of a product gives very good results. In this case the preference of one product over another is not in-

dicated by a score. The samples of the product are placed before the observers in a series of pairs and they are requested to state their preference. When no preference is indicated, the reply is not used. Preferably only one comparison is made at a given interval. The samples are given code numbers, and identity of the samples is unknown to those scoring them. If it is desired to rank in order of preference four materials designated for the purpose of illustration as A, B, C, D, a series of six comparisons is made, namely, A *vs.* B, A *vs.* C, A *vs.* D, B *vs.* C, B *vs.* D, and C *vs.* D.

On the basis of the information gained from the preference of persons by this type of study, it is possible to rank them in order. Since such a group of persons is uncalibrated and its reliability unknown, it is necessary to assume that the group follows about the same order as in normal population groups; that is, when the difference between quality of items may be small, 50 percent of the judgments will be found to be unreliable. It is therefore necessary to subject the data to a certain amount of analysis to determine its reliability. The chief objection to this type of test is the time-consuming element.

In many instances a panel of six to twelve persons is used for flavor comparisons of series of samples. It should be pointed out that in these panels people selected for reliable judgments should be calibrated by hundreds of examinations that are a matter of formal written record. A judge may be required to have an average deviation of not more than 1 point from the average scores of the group, and a standard may be set for his ability to repeat judgments on duplicate samples. A numerical scale as previously mentioned is used for this panel method. A beginner can gain experience by working with a group of trained observers.

Wherever possible in the scoring of a product, especially with persons lacking experience, it is desirable to select a very good sample, a fair sample, and a poor sample of the product for reference standards. These samples should be scored by the panel, and the averages of the scores for each sample will designate the score of the sample for reference purposes.

The chief concern in this type of work is the significance of the results. The procedure used, the number and efficiency of the tasters, and the number of tests on a specific product influence this significance; however, the application of statistical analysis to the results is the measure of their significance.

Consumer-acceptance tests are another story in themselves. Many food laboratories interested in developing processing techniques to improve the quality of a product usually do not obtain a large-scale consumer reaction to the product in question even if their interest is obviously in the consumer acceptance of the product. Many commercial firms use panels selected from their own employees to screen newly developed products, and those deemed sufficiently good are subjected to a consumer-acceptance test before they are placed on the market. Some companies set up their own acceptance tests, and other companies use professional agencies whose business it is to collect information on an item. Even a good-sized flavor-evaluation panel cannot be selected which will be representative of consumer acceptance, because such factors as food habits, race, section of country, age, and economic status affect acceptance of foods. It is for this reason that many companies wish to obtain the reaction of their consumers prior to placing new and improved types of products on the market.

REFERENCES

1. Bengtsson, K., and Helm, E. Principles of Taste Testing. Wallerstine Lab. Communication 9, 1946, No. 28. 171–81.
2. Plank, R. P. A Rational Method for Grading Food Quality. *Food Technology,* 1948, 2, 241–60.

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BOOK REVIEWS

OF THE HUMAN MIND

Studies in Analytical Psychology. Gerhard Adler. 250 pp. Illus. $4.00. Norton. New York.

THIS book has emerged from six lectures on analytical psychology presented by the author over a period of fifteen years at various scientific gatherings in London. The lectures have been reworked, expanded, and sequentially arranged so as to provide an effective vantage point for viewing the larger facets and implications of Jungian thought. It is not a systematic presentation of doctrine and theory, but rather a synthesis of important aspects of it, as seen, particularly, in clinical practice.

The psychology of C. G. Jung stresses the constructive role of the unconscious in personality integration. For him and his followers, the unconscious is the vehicle of the wisdom, needs, and energies deriving from countless eras of ancestral experience. It is the potent and creative layer of the psyche, the "matrix" of the conscious mind. This psychology has undoubtedly a greater appeal for the nonprofessional reader than have other varieties of medical psychology. Perhaps it recommends itself especially through its positive orientation and concepts which are not at war with religious philosophy. It also strives for flexibility in dealing with clinical problems, viewing each patient as unique, each individual as "an experiment of ever-changing life and an attempt at a new solution or new adaptation."

Dr. Adler makes an interesting study of the techniques of Jungian psychology as compared with Freudian and Adlerian schools. He discusses dreams as manifestations of the archetypes of the collective unconscious; cycles of life as related to the ego and different psychological problems; the therapeutic use of unconscious forces; the compatibility of religious and analytical points of view; and, finally, the meaning of Jung's work to modern man. As the protagonist of a theoretical point of view in medical psychology—a fascinating but highly controversial one—the author has done an excellent job. His book is readable as well as informative.

Readings in the Clinical Method in Psychology. Robert I. Watson, Ed. xi + 740 pp. $4.50. Harper. New York.

WELL-SELECTED papers, fifty in number, published by leaders in the field of clinical psychology over the past twelve years, provide a comprehensive coverage of the status of applied psychology. The editor has classed the contributions into four groups, for each of which he has written a chapter critically reviewing broad developments or pertinent literature omitted from the readings. The first group is concerned with the clinical method itself; the second, with the functions of the clinical psychologist; the third, with diagnostic methods; and the fourth, with methods of treatment.

Clinical psychology encompasses broad areas of practical interest. In many arenas of modern life, in industry, schools, courts, prisons, institutions, military set-up, in many social settings, and chronological groups it performs the functions of selection, guidance, interviewing, and counseling, delving carefully into problems of normal and abnormal behavior. As a discipline allied with psychiatry, its most familiar function is, of course, psychometrics. The psychiatrist, who has firsthand knowledge of its value in this respect, as an adjuvant to diagnosis with all this implies for the future handling of the patient, has been slow to recognize the therapeutic role which the clinically trained psychologist might play. The Veterans Administration has recognized it, however, and there is a definite movement, in many quarters, toward assigning the psychologist broader functions. It is becoming increasingly obvious that the psychologist can make a useful contribution to the large borderline group between those we consider normal people and those frankly suffering from mental disease.

Clinical psychology bears heavy responsibilities, both in keeping high its standards for training and practice and in allocating its practitioners to the areas where they may specialize to best advantage. It is encouraging that a certifying professional body has recently been organized by the American Psychological Association to pass upon professional fitness. This will help keep at a distance those charlatans and incompetents who have been a menace to scientific psychology and psychiatry as well.

Watson's *Readings in the Clinical Method in Psychology* affords an excellent perspective of accomplishments and problems of clinical psychology. It is an indispensable reference work in the field.

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Basic Principles of Psychoanalysis. A. A. Brill. xv + 298 pp. $3.45. Doubleday. New York.

DR. A. A. BRILL may fairly be credited with having introduced psychoanalysis to America. He translated the works of Professor Freud into English, and was tireless in explaining them to medical and lay audiences by lecture and pen. He was a respected leader in the rapidly growing psychoanalytic society until his death at the age of seventy-four.

This book again undertakes to present the basic principles of Freud in a manner comprehensible to

the layman. Dr. Brill's style is sprightly. Every page is enriched with illustrative anecdotes from his enormous experience. He shows how the unconscious operates in the daily life of normal people, as well as in the queer behavior of the mentally ill. He demonstrates that none of our behavior is "senseless," but its meaning may be disguised because the underlying purpose is not one we can admit even to ourselves. He is especially successful in relating the modes of thought characteristic of early childhood to those observed in the dream, in slips of the tongue, in forgetting and other unconsidered phenomena of everyday life, in choice of vocation, in artistic productions, and in the pathological symptom.

The defect of the book is that it is already "dated." Even as an exposition of Freud, it suffers from the omission of the professor's later work on the ego and superego—that is, his more careful analysis of the forces which oppose direct expression of the primitive sexual "wish." Dr. Brill attempts theoretically to broaden the concept of sexuality to "love," but his concrete illustrations tend to fall back upon sexuality in the old-fashioned sense of mating, with too little consideration of oral and anal components, or the primitive fear of loss of love. The brilliant developments of later Freudian doctrine and contemporary psychoanalysis in the area of character formation and the intricate dynamics of "ego defenses" are almost entirely ignored.

The chapter on major mental disease, the psychoses, is dangerously obsolete. At a time when the label "schizophrenia" is being widely applied to persons previously considered neurotic (who often get along quite well in life), Dr. Brill describes all too vividly the horrid course of the disease as known to psychiatrists twenty years ago. The patient, family, employers, and teachers who happen to encounter this new use of the diagnostic label may be needlessly frightened by a book published in 1949 by a respected authority. At a time when shock treatment is familiar to nearly everyone through the public press, it is strange to find no mention of it except a personal anecdote about a patient who recovered dramatically during an incidental operation, only to relapse completely three weeks later. Psychoanalytic interpretations of the psychoses are not included. One could wish that the editor of this posthumous book had had the courage to delete this section altogether.

It is hard to recommend Dr. Brill's book as an authoritative treatment of present trends in psychoanalysis and psychiatry. The wealth of clinical material makes it valuable reading for any person, lay or professional, with perspective to see it as a simplified exposition of Freud's earlier work, enriched by the personal observations of an astute doctor.

Ruth L. Munroe

Department of Psychology
College of the City of New York

HISTORY SOUTH OF THE EQUATOR

Paraguay. Harris Gaylord Warren. xii + 393 pp. Illus. $5.00. University of Oklahoma Press. Norman.

Mr. Warren's book, although it is only a brief summary of the rich and little-known history of the small and gallant nation of Paraguay, is more noteworthy than any other publication I have read on the subject. The author has spared no effort to compile information and to delve deeply into serious research in order to bring forth an intelligent and comprehensive synthesis of the most interesting events and personalities in Paraguayan history.

Written in an appealing and open-minded fashion, it analyzes the period of Paraguay's history from the time of the bloody conquistadores, through the colonial days, the tyranny of Francia and Lopez, to the present era.

Among the many interesting passages of the book, the tales of the Guarani Indians are especially captivating. The Guaranis, an extinct tribe, left nothing of their civilization but their language, which survived the conquest of the Spaniards, who, with refined cruelty, tried to "humanize" the Indians. Guarani is still the language that pulsates in the heart of every Paraguayan today. One hears it in the streets, in the market places, and in the homes of rich and poor alike. Many books and pamphlets were written in Guarani; the Bible was even translated into that language by the Jesuits. And yet there is not a single school in Paraguay that teaches it; it is learned in the most peculiar and unorthodox manner, from servants, from the peasants, and from the farmers who come to the cities to sell the products of their land.

Paraguay, economically strangled by its neighbors, has been striving desperately to attain economic independence since the days of Gaspar Rodríguez de Francia—one of the most notable personalities in Latin-American history, as described by Thomas Carlyle in one of his essays.

Francisco Solano Lopez, who waged a war against the combined forces of Argentina, Brazil, and Uruguay that practically annihilated little Paraguay, represents the fight to the death against the imperialistic encroachment of Brazil and Argentina, whose armies, under the guise of liberators, invaded the country, annexed a good portion of the land, and carried away with them the loot of victory.

The turbulent legacy that Paraguay inherited from the Spaniards, the Guarani Indians, Francia, and Solano Lopez is still very much in evidence in modern Paraguay, as indicated by the continuous internal upheavals which, combined with the nation's poverty, continually retard its recovery.

Mr. Warren deserves the highest praise for his honest attempt to bring together in book form the loose and much-disputed history of Paraguay. It provides important material to fill the gap in the relations

of the nations of the Western Hemisphere, their people, their sentiments, and, most important of all, it encourages mutual understanding through better knowledge and appreciation of each other's problems, past and present.

CARLOS GARCIA

Pan American Union
Washington, D. C.

WAVE OF THE FUTURE

Engineering the New Age. John J. O'Neill. 320 pp. $3.50. Ives Washburn. New York.

SINCE 1926 Mr. O'Neill has been interpreting the progress of scientific research to the readers of the *Daily Eagle* and *Herald Tribune* In so doing he has had to keep in touch with the investigations undertaken by scientists in all fields of human endeavor. The present volume quite naturally reflects the myriad contributions these studies have brought to the daily needs, comfort, and fundamental understanding of man. The author accepts Gano Dunn's definition of engineering as "the art of the economic application of science to social purposes." Engineering would thus pretty well encompass human life in all its aspects, since among the accredited curricula in departments of engineering mention is made of such branches as aeronautical, agricultural, architectural, chemical, civil, electrical, industrial, mechanical, metallurgical, mining, sanitary, and many others. Doctors and sanitary officials are classed as engineers, applying the results of scientific research to the problems of health and disease.

In the author's imagination engineers should be, and in the future will be, in charge of human affairs and progress, directing man's efforts toward the control of his environment. Mr. O'Neill envisions a time when it will "make no difference where the Great Lakes are, in which direction the Mississippi flows or whether the Tennessee Valley dams are full or empty." For man shall make whatever weather he chooses, rain, shine, cold, or warm. "Farms will be factories in buildings independent of sunshine, rain or soil." Land may then be devoted to other purposes, for playground or scenic satisfaction. An enzyme may be discovered or synthesized which, in the circulating blood, will create all the amino acids we need, thus releasing us from dependence on animal food.

We might by chain atomic energy raise up new mountain ranges within a year's time or tilt the surface of the country differently at will, making the Mississippi run north if we so desired, or locating the lakes more conveniently for the economy or aesthetic joy of the people.

All human activities will be guided by engineers who "chart and control social strains and stresses in a community without regard to any set of political principles or party interests." The future welfare of mankind is to be laid on a firm and stable keel by a general planning committee who will blueprint the lines of development for 10,000 years in advance.

All existing cities will be torn down and rebuilt according to a plan for the whole country, relocating them rationally instead of in the present haphazard manner. Quite understandably this view is focused on the indefinitely distant future when the "multiman" shall have arrived.

E. V. WILCOX

Washington, D. C.

ALL ABOUT SEA URCHINS

A Monograph of the Echinoidea. Th. Mortensen. Vol. IV. 1: *Holectypoida, Cassiduloida,* 363 pp. Illus. 2: *Clypeastroida,* 471 pp. Atlas, with plates. C. A. Reitzel. Copenhagen.

WITH the appearance of this double volume, one of the most Gargantuan of monographs is nearing completion. Only one more volume remains to be published, and it is to be hoped that it will come out during this year, so that the more than eighty-year-old author will be able to look at almost two feet of shelves of a completed supermonograph, in which for once the first-rate typography, the magnificent plates, the detailed text figures, etc., are equalled by a first-rate piece of scientific work. Outside of libraries, of course, very few persons will own this expensive opus, but there is no doubt that for years to come it will be the standard work which every student of sea urchins will consult. Extracts, as well as copies of figures, will find their way into smaller papers, which is perhaps the greatest satisfaction an author can have.

The monograph was begun in 1935—the final work of a hard-working man who had been a devoted student of echinoderms since the end of the last century. His age—around sixty-five—probably seemed just right to him, for although others begin to think of retiring at that age, he probably felt that he had just reached a point where he would not make too rash and youthful conclusions. The invasion of Denmark made comparatively little change in his work, except that he was cut off from examining material in other museums, particularly the unexcelled collections in the United States—in the U. S. National Museum and in the Museum of Comparative Zoology—as well as those in the British Museum, which were hidden away during the blitz. But in the same moment the armistice came, material was again sent across the sea, and he was able to put his final conclusions into the manuscript before it went to the printer.

In the first volume he treated the Regularia, fossil and Recent, and in the present volumes he has dealt similarly with the greater part of the Irregularia—that most fascinating division of sea urchins in which the members have abandoned their almost perfect symmetry and made an attempt or, rather, several independent attempts, to achieve a bilateral symmetry. At the same time, some have discarded their unique

dental apparatus, so well known to all students of elementary biology, namely, the lantern of Aristotle, with its five powerful rodent teeth, which enables its owner to chew up the most leathery kelp and even excavate holes in the rocks wherein he can live. It is also in this unorthodox group that one finds the greatest aberrations of the tube feet, from the simple suction disk of leaflike structures that function as gills, or handlike developments that are able to gather up minute edible particles and stuff them into the toothless mouth. In no other group of echinoderms are the evolutionary trends so rampant, and with the fossil evidence on hand, one is here able to piece the sequence together almost as perfectly as in many of the vertebrates.

The author has worked with these animals for almost sixty years, with an enormous amount of field work and embryological studies besides his museum work. Here in this final study he has transgressed into the field of paleontology and included all the known fossil forms. It was Robert T. Jackson's idea that a paleontologist should be familiar with the living forms in order to interpret the more incomplete fossil data, and the author of the *Phylogeny of the Echini* would undoubtedly be pleased to see the manner in which his old friend, with his extensive knowledge of the living forms, has tackled the fossil species.

The fourth volume follows the pattern laid down in the first volumes, and there is the same evenness in the treatment from beginning to end which makes it such a pleasure to use the work. Extensive lists of synonyms are supplied, although it seems almost humanly impossible that the author singlehanded should have been able to round up every one of them. Clear diagnoses are given, and keys, so that any person with a fundamental knowledge of the morphology of the sea urchins will be able to find his way though the entire work and identify whatever sea urchin, living or fossil, he has before him. There are excellent descriptions, detailed information about distribution, critical discussion of nomenclature, and extensive lists of literature. Those who are accustomed to the American method of indicating where the type is deposited, and how many specimens the author has examined, may deplore the lack of such details, but there is no doubt that the presence of such data would have increased the bulk of the volumes and possibly detracted somewhat from the flowing ease with which the account runs along. Extremely valuable is the abundance of text figures, mostly original, showing the arrangement, the structure of the apical field, cross sections of spines, etc., which accompany the text and supplement the first-rate photographs of the plates. After having examined the volumes, one is still unable to understand how a single man has been able to make so gigantic a contribution, even when in his later years he was relieved of all routine duties and had the wholehearted support of all the workers on echinoderms in the world (not a very large group, it must be admitted). With the many and often well-justified criticisms that have been directed against systematic zoology, it is a satisfaction here to be able to point out a whole group of animals that have been studied as they ought to be, and where a solid foundation has been laid from which one can begin to explore these animals from other angles.

Last but not least, one feels a deep gratitude toward the Danish government and the Carlsberg Foundation, which have enabled the author to carry out his great work. From start to finish he has been freed from worries of all kinds, and in spite of war and occupation, paper shortages, and other difficulties, he has been able to feel that the high standard of printing and reproduction of plates would not be abandoned, that he would never be let down in spite of economic difficulties. Both the author and his country have every reason to be proud of each other.

ELISABETH DEICHMANN
Museum of Comparative Zoology
Cambridge, Massachusetts

HIMALAYAN BOTANY

The Valley of Flowers. Frank S. Smythe. 325 pp. Illus. $5.00. Norton. New York.

A REVIEW appearing in a scientific publication must begin with the statement that this book's title and the claim made on its jacket that the volume is perhaps the best on the subject of Himalayan botany are rather misleading. A good part of the book is dedicated to a cataloguing of plants seen or collected by the author during his stay in the remarkable valley about which he writes. But Smythe's botanizing was of the incidental, enjoyable kind, the kind that makes the world of plants and flowers sometimes mean a great deal more to the amateur than to the botanist, who must concern himself with problems of form, function, and relationships. The author's deep appreciation of the beauty of plant material and his deft use of words in describing their beauties add a delightful dressing to his book. They make most pleasant reading of a mountain-climbing tale that might otherwise be of interest only to mountaineers. For most of us there is fascination in words about faraway places. There are thrills in the details of such adventures as Smythe's mountain-climbing expedition, and there is real lure in such excellent, and in the midst of a hot Texas summer perhaps I may add cooling, photographs as he has included in his volume.

More than anything I have seen in a long time, this book provides an opportunity to get away from it all for a few hours. It is diversionary reading at its best.

W. GORDON WHALEY
The Botanical Laboratories
The University of Texas
Austin

BASIC GUIDE FOR LIVING

Biology for Everyone. W. Gordon Whaley. xi + 374 pp. $2.79. Illus. Garden City Publishing Co Garden City, N. Y.

IN AN exceedingly interesting book, characterized by lucidity of exposition and deft continuity in the presentation of the subject matter, the author gives a well-balanced outline of biology. The chapters fall into three general groupings: six devoted to what used to be called "general physiology;" five on kinds of plants and animals, speciation, evolution, and the origin of life on this earth; and six on biosis—ecology, reproduction and sex, inheritance, plant and animal health. There is a résumé chapter stressing plant and animal science as it affects the individual.

An author presenting the elements of a branch of science could disclaim intent of writing for his colleagues and plead that dogmatism of statement should be charged to a desire to avoid confusing the beginner. But in his preface Whaley announces his intention to "introduce the reader to the science of Biology as a whole. . . . This wealth of knowledge is presented here for those who have not taken courses in the subject and for those who wish to turn to it for review in the light of recent advances." Lest this be taken as the opening gambit for slashing criticism or devastating comment, the reviewer hastens to state that in his opinion the author surprisingly well achieves his purpose and will please both categories of readers.

With profit, we may explore the technique that enables the author to cover the full gamut of biology in a manner acceptable to the trained scientist and at the same time to produce a book entirely intelligible to a high-school student who commands a vocabulary slightly beyond basic English. The author is precise in statement, uses technical terminology as necessary, does not write down to the reader, and eschews Sunday-supplement style. The key seems to be the efficient organization of the subject matter, and the omission of extraneous details, or side excursions, no matter how intriguing these may be to the specialist. The author knows what he wants to say and says it. The reader will wish there were more, and that's the great art of both letter- and book-writing. Consistently, topics are presented according to their historic development, each major contribution to science being tagged with the name of the discoverer. Thus each chapter, after an orienting series of questions and an introductory statement, shows how a particular phase of biology developed, what the building stones were, and who fashioned them. This scheme of presentation not only outlines biology but constantly brings to the fore its dynamic nature, no phase being presented as a closed episode. Much of the charm of the work and of its appeal stems from this portrayal of science as a living, growing thing, always on the threshold of new advances.

Science also has its heroes. The author heads each chapter with the names of those who have made fundamental discoveries. In the appendix, under "Biographical Notes," a complete roster of the scientists is given, the life span shown, and the chief contribution of each epitomized. Usually there is no formal citation of publications, exceptions being books that have had far-reaching effects or have been, in their day, highly controversial.

Probably no one would query the scientists included, but each might note omissions Firsts in discovery are assigned. These conform well with current acceptance, but of course such assignment would receive the customary revision or qualification by the specialist. No scientific worker is an island. But the specialist should not complain because individual discoveries appear in the book as the building stones of our knowledge, whereas general contributions, even the founding of entire fields of biological science, do not fit into the presentation pattern and may not be registered.

The text is effectively illustrated with line drawings by the author's wife, Clare Y. Whaley. These very definitely contribute to the presentation of the subject matter. Their simplicity and originality make them a welcome relief from the stand-by illustrations that have passed from one text to another. Possibly some indication of the relative size of the object or part illustrated would help the uninitiated.

The book gives the core material for a complete biology course. It may find a place as a beginner's textbook. The teacher, by laboratory exercises and discussion, would need to round out the condensed, almost summary, statements. There seems to be a tendency for beginner's books to be encyclopedic and to bristle with footnotes and references. Possibly such books are too formidable and their effect discouraging. Here is a book that a student could and would come to know, and it would build for him an apperceptive mass requisite for adjustment to, and appreciation of, his natural environment.

The final paragraphs of the book deal with man's place in nature. They constitute an admirable statement of the biologist's creed. As Whaley sees it, biology

> provides the basic guide for living. Man's evolution has carried him far. He is much more than an animal, and there is much in his life which is not shared by other organisms, is not translatable into the terms of biology and the other sciences. However, as man is but one of the countless living organisms inhabiting the earth, the fundamental requirement is that he conform to the organic pattern. Only thus can man respect his limitations and realize his potentialities.

G. H. Coons

Bureau of Plant Industry, Soils and Agricultural Engineering USDA Beltsville, Maryland

A NATURALIST IN THE TROPICS

High Jungle. William Beebe. 379 pp. Illus. $4.50. Duell, Sloan & Pearce. New York.

AT THE death of the Venezuelan dictator Juan Vicente Gómez, in 1935, work was halted on an extraordinary structure nicknamed *Rancho Grande,* situated at the mountain pass of Portachuelo above Maracay. This grandiose but unfinished building, part of which has been outfitted for laboratories by the Creole Petroleum Corporation, stands at the center of a vast reserve of unspoiled cloud forest. Dr. Beebe's most recent book recounts, in rambling style, the "lighter aspects" of zoological field work conducted there during parts of the years 1945, 1946, and 1948.

Fortunate indeed must be the naturalist who finds himself stationed at a place so integrally a part of the primitive forest. We learn that marsupial frogs and even fers-de-lance inhabited its rooms, blue-and-white swallows bred in its crevices, and in its neighborhood one might hope to see almost any bird or mammal of the coastal Andes.

As the author himself has written in his preface, this volume attempts to give only the more inconsequential phases of his work in Venezuela. The book is directed to the reader whose interest in natural history is nontechnical and who must usually visit jungles from an armchair. The reader with a more technical interest may be at times exasperated by having his curiosity aroused by some diverting discussion of the strange ways of bird or butterfly, which is not carried on to a satisfactory conclusion. For him, however, there are two appendices, one giving the scientific names for creatures mentioned in the text in vernacular, the other listing the published scientific papers that have resulted from Beebe's Venezuelan expeditions. In any case, it may safely be said that no naturalist who has experienced a taste of life in the New World tropics is likely to lay the book down without twinges of envy and nostalgia.

The beautiful photographs and the excellent typography add much to the volume's interest, if anything more were needed than the author's infectious enthusiasm for his subject, whether describing the family life of bat falcons, the fauna of a forest pool, or the effects of unprecedented rainfall upon the Andean slopes.

H. G. DEIGNAN

Smithsonian Institution
Washington, D. C.

BRIEFLY REVIEWED

Carl Alsberg—Scientist at Large. Joseph S. Davis, Ed. xi + 182 pp. Illus. $2.00. Stanford Univ. Press. Stanford, Calif.

THIS fine volume is a collection of five biographical essays upon the life of Carl Alsberg, together with three of Alsberg's own papers and a classified bibliography of his publications. Alfred L. Kroeber contributed the first paper, "The Making of the Man," a warm account of Alsberg's personal life. Donald D. Van Slyke reviewed his work in the natural sciences; Fred B. Linton, his career as Chief of the Bureau of Chemistry, U. S. Department of Agriculture; Robert D. Calkins, his life as a university professor and administrator; and John B. Condliffe, the contributions of Alsberg as a social scientist beyond the university. Alsberg's own three papers are well chosen to give an idea of the breadth of the man: "Progress in Chemistry and the Theory of Population," "What the Social Scientist Can Learn from the Natural Scientist," and a Commencement address at Reed College.

HOWARD S. MASON

National Institutes of Health
Bethesda, Maryland

A Concise Encyclopedia of World Timbers. F. H. Titmuss. v + 156 pp. $4.75. Philosophical Library. New York.

OF VALUE primarily to the woodworker rather than to the wood technologist, the macroscopic descriptions of nearly two hundred commercial timbers are concise but hardly encyclopedic. The common names used are those of British commerce. Although a handy and useful book, it is overpriced.

HENRY CLEPPER

Society of American Foresters
Washington, D. C.

CORRESPONDENCE

DEFINITELY NOT

Naturally I don't know how many other SCIENTIFIC MONTHLY readers have written in about this, but even if nobody else did I want you to know that several of my friends and I considered the "modern art" used on pp 322–328 (May issue) definitely out of place in THE SCIENTIFIC MONTHLY.

I can't criticize Mr. Kahn's drawings à la Aurignacien from the point of view of the art critic, but the readers of THE SCIENTIFIC MONTHLY expect photographs, diagrams, and drawings, and not nonsense, especially such as appears on pages 324 and 327.

If this should become a trend we'll have to open SCIENTIFIC MONTHLY with trepidation; any moment you might spring Tovarisch Picasso on us. Or are glamour girls in four colors the next item on the list?

WILLY LEY

Montvale, New Jersey

BUT, ON THE OTHER HAND . . .

My dentist tells me that many of his patients speak appreciatively of THE SCIENTIFIC MONTHLY, which we find in his waiting room along with recent copies of popular magazines.

I, a layman, have just reread Stanley A. Cain's article on "Plants and Vegetation as Exhaustible Resources" and chuckled again at Matt Kahn's drawings.

I miss specific reference to healthy human relations in this matter of seeing the problem whole, though I think it is implicit in some of Mr. Cain's statements. Research into the facets of human relations . . . is needed now on a large scale.

LUCILE MALKIN

Vancouver, B. C.
Canada

OUR POLICY IS APPROVED

I am writing to say that I enjoyed reading the article in the April SCIENTIFIC MONTHLY on "La Montaña Llorona" by Archie Carr, Jr.

It is a rare thing for a scientist to write with such literary value; reading it was a genuine treat.

I am glad that your editorial policy approves the inclusion of a wide variety of subjects. As a reader, I also approve. More and more I come to depend on THE SCIENTIFIC MONTHLY to give me a breadth which it would not be easy otherwise to obtain.

DOUGLAS E. SCATES

Office of Naval Research Projects
American Council on Education
Washington, D. C.

PERMISSION GRANTED

This is to inquire from you whether I may translate into Urdu articles out of your magazine, for purposes of broadcasting at the radio. . . . You well know that Pakistan needs to be industrialised. Prior to that, her public has to be made scientific-minded. In allowing me the requested facility, you will be helping the cause of my country, for which I will be very much thankful to you.

F. U. KHAN

Karachi, West Pakistan

OPUS 1 FOR THE NEW HYMNAL

WITH A BOW TO ISAAC WATTS

"The Church of England has appealed for new hymns, for the atomic age."—*The New Yorker.*

O God, our help in ages past,
Hear now our brand-new psalm;
We'll sing about a mighty blast,
I. e, the atom bomb.

Under the shadow of Thy throne,
By brains and intuition,
We've builded something all our own
From dust and nuclear fission.

A thousand ages in Thy sight
Have yielded us plutonium;
But watch us change it overnight
To superpandemonium.

Before the hills in order stood . . .
We'll blow the hills to powder.
When Russia has bombs just as good
We'll have 'em even louder.

Time like an ever-rolling stream
Moves to the very brink;
Our will is now the thing supreme.
(It's later than You think.)

Thy word commands our flesh to dust;
Now we revert the process,
And at the world, if cause be just,
We'll thumb our proud proboscis.

O God, our help in ages past,
This is the way we feel:
You've helped us long enough; at last
Move over—we'll take the wheel!

PAUL H. OEHSER

Washington, D. C.

THE SCIENTIFIC MONTHLY

SEPTEMBER 1949

MAKAPANSGAT

GEORGE B. BARBOUR

Dr. Barbour (Ph D., Columbia, 1929) is professor of geology and dean of the McMicken College of Liberal Arts at the University of Cincinnati, and has just concluded a term as president of the Ohio Academy of Science. Born in Scotland and educated in the universities of Great Britain, Germany, and the United States, he spent twelve years in China, where he was associated with Davidson Black, P. Teilhard de Chardin, W. C. Pei, and Franz Weidenreich in the Peking Man discoveries. He was geologist with the University of California Expedition to Africa in 1947–48 and has also traveled extensively in Europe, Siberia, Southeastern Asia, and the Caucasus.

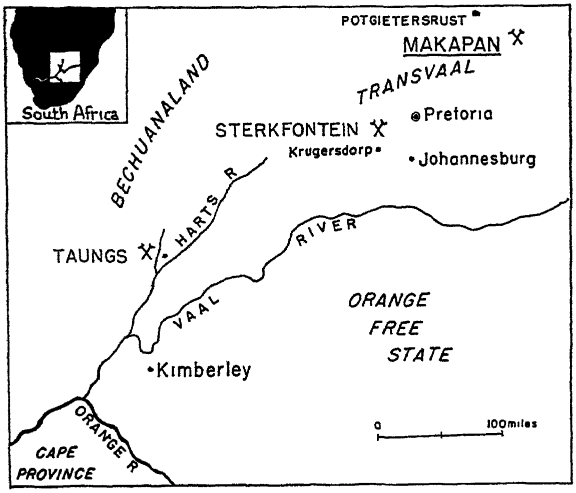
MORE significant light on human prehistory has come from a secluded valley in the northern Transvaal, 125 miles from Pretoria, than from any other single locality in South Africa. Caverns in the hillside have yielded proof that they were inhabited not only by men of the Old and Middle Stone Ages, but also by creatures so primitive that they span the gap between undoubted apes and true men. These exciting discoveries have been almost disconcerting.

From the capital city, the Cape-to-Cairo highway strikes north across the Bushveld along the old caravan route taken by the early Dutch *Voortrekkers* in 1838, and then northeast around the edge of Springbok Flats to a gap in the mountains through which the Magalakwin flows to join the Limpopo. The stream has so low a gradient that after heavy rain it floods a strip 1,200 yards wide for 50 miles before entering the narrows. The settlement on the riverbank where the God-fearing pioneers outspanned their teams for the night still attests by its name—Nyl-stroom—their error in mistaking this broad flood for the headwaters of the Nile, the "River of Egypt" of which their Bibles had told them. Five miles beyond the entrance to the water gap, a tributary valley heads back into the limestone hills. Here, in reprisal for an attack on their camp, the Boers drove the native chief Makapaan and his marauding tribesmen into a cave, piled brushwood, faggots, and stones in the entry, and set light to the bonfire. Makapaan's men were virtually wiped out, and the Boer leader, Potgieter, was killed. For a century the Valley of Makapansgat and the town of Potgieters-rust have been famous landmarks.

I

The Historic Cave is only one of a number in the Transvaal Dolomite that flanks Makapansgat Valley. Some of the caverns were worked extensively over several decades for the thick layers of cave travertine with which most of them were floored at an early stage in their long history. Once these layers of high-grade lime had been extracted, the overlying mass of rock debris choking the caves made further excavation risky, and the miners kept moving on to new prospects, little suspecting the value of the precious rubble over their heads.

In 1925 Professor Raymond Dart noted that the breccia forming the roof of one of these caverns contained quantities of bone fragments. Some of them were blackened as if charred, and, when lab-



oratory analysis confirmed the presence of free carbon, Dart concluded that there seemed "to be little doubt from the evidence available that the bone bed is the 'kitchen-midden' result of human occupation at a remote epoch."* His critics suggested that the carbon might have been due rather to bush fires, blasting powder, or even to sightseers' bonfires. For more than twenty years Dart's daring conclusion was accepted with distinct coolness—or at least placed in cold storage.

In 1937 Dr. van Riet Lowe visited Makapansgat. In a miner's drift close to the Historic Cave he found that a huge block of roof rock had fallen to the ground. This proved to be a consolidated mass of limestone rubble, evidently an old cave filling, on the upper surface of which was a layer of broken bones and stone implements of advanced Paleolithic type, somewhat resembling the Acheul culture of Western Europe. Artifacts of this kind have been known to occur widespread throughout South Africa, but almost always on the ground surface, or in layers of superficial soil or river gravel—never before unquestionably on the very spot where their original owners had dropped them in a once-inhabited cave. The block of breccia had detached itself along the bone layer that had been an occupied site, and further collapses of this less coherent floor-earth showed by the color-banding and ashy layers that the cave had been inhabited repeatedly over a considerable period of time. It was appropriately referred to as the "Cave of Hearths."

* Bibliographic references to this and other problems relating to the various Australopithecinae sites will be found in a recently published paper, "Ape or Man?—An Incomplete Chapter of Human Ancestry from South Africa," Presidential Address, Ohio Academy of Science. *Ohio J. of Sci.*, 1949, 49, 129–46.

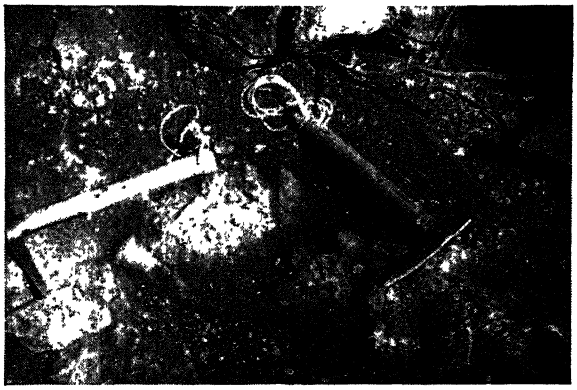
A hundred yards away, van Riet Lowe found another abandoned limeworkers' drift with hearth layers of even brighter color-banding, which earned it the name "Rainbow Cave." But the stone implements embedded in the floors of this site show a marked advance in technique, and belong to a Middle Stone Age flake industry known as the Pietersburg culture. Lance heads of this type recall the Solutrean of France, the general workmanship closely resembling the Levallois technique.

The importance of the locality could no longer be gainsaid, and the two sites were proclaimed a National Monument by the government. Thanks to a grant from the Bernard Price Foundation at the University of Witwatersrand, the Archaeological Survey was able to embark on a scientific excavation of the Cave of Hearths.

Shortly after arriving in the Transvaal as a member of the University of California's African Expedition under Dr. Charles L. Camp, I was invited to study and report on the prospects of success at the Makapan sites. It was felt that experience gained at the Choukoutien caves near Peking, where *Sinanthropus* was found, might be of value in laying plans for more extended sapping operations, and might also serve to encourage the directors in their hope that further excavation would yield even more important results. These hopes were borne out by the almost immediate exposure of two hand-axes of Chelles-Acheul type and shortly after by the recovery of a fractured human jaw of unmistakably Neanderthaloid type. The entire deposit had been consolidated by calcareous cement, so that the cave and its filling constitute a "sealed" site. There is thus no reason to doubt that the spent hearths and the Paleolithic tools belonged to men who once inhabited this South African cave and who resembled their relatives of the Old Stone Age north of the Mediterranean.

The Cave of Hearths, the Rainbow Cavern, and Makapan's Historic Cave are situated close together, high on the side of a valley that ends abruptly in a cliff-walled amphitheater three quarters of a mile to the east. Half a mile in the opposite direction, the westerly dip of the strata brings the soluble dolomite down to lower elevations, and the most extensive workings of all are only 50 feet above the level at which the alluvial floor of the valley abuts the base of the hillside. At this point, however, the contact is buried under an immense dump of waste rock. One of Professor Dart's photographs shows this Limeworks site

viewed from across the valley. Above eye level, beyond the upper road, are seen the sinkholes and cave openings by which access underground was first gained. It was in one of those to the left of the center that Dart's original find was made. Below the upper road are the dumps of debris from the caves—gray in the half tone, but red and yellow in the field. In the left center stand the deserted lime kilns, and beneath them a broad fan of white refuse from the kilns, approached from the right by the lower road. Between the native kraal of conical-roofed huts with their brushwood fence (extreme right, below) and the nearest corner of the refuse dump is the flat-topped sieving platform. Here more than six hundred tons of breccia have recently been broken up and carefully inspected for any trace of fossiliferous material worth transporting 150 miles to the laboratories of the Witwatersrand University in Johannesburg.



Hammers point to Old Stone Age artifacts in the Cave of Hearths.

At the time of my visit in 1947 the higher caves were receiving the lion's share of attention, but I urged in my report that more intensive work also be done at the Limeworks. One afternoon when the native crew had knocked off work at the Cave of Hearths, Mr. James Kitchings, who was supervising the excavations, took a busman's holiday in the Limeworks Cave. He was rewarded by finding the damaged occipital bone of a primate which Dart recognized as of the same genus as the *Australopithecus africanus* described by him in 1926 from the Norlim quarries at Taungs 300 miles to the southwest. The Makapansgat individual, however, differed sufficiently in anatomical detail to warrant the establishment of a new species, and the earlier discovery of evidence of fire suggested the name *A. prometheus,* after the Greek hero who brought fire to man. Further excavation by E. R. Hughes and Kitchings revealed the almost perfect jaw of a twelve-year-old adolescent, an infant parietal, and part of the face of a female adult.

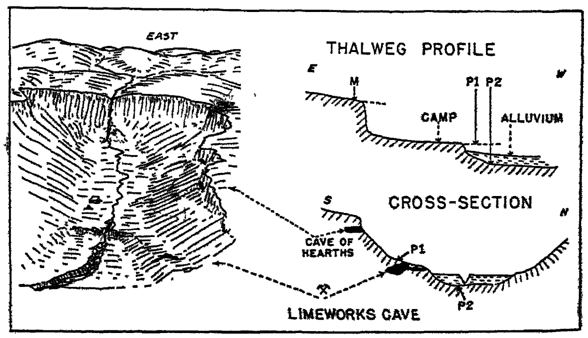
The finds were all made in blocks of breccia from the "southern cavern," access to which is

Entrance to Cave of Hearths.

- Limeworks site. (Photo by Professor Raymond Dart.)

through the dark shaft seen at the right in the photograph. This is actually an artificial entrance opened by the limeworkers, who were following a six-foot seam of travertine down into the hillside. As in the other caves, the travertine layer of virtually pure lime carbonate once floored a cavern that later became choked with fallen rock and breccia. In the normal cycle of cave development, travertine forms during the stage when water is still percolating freely through the bedrock fissures and dripping from the ceilings, whereas the caves only become habitable by man or beast at a later, drier period; hence, the fossiliferous layers usually lie above the dripstone layers and beneath the zones of coarser collapsed roof rock. To the left of the black entrance hole can be seen the banded and distorted layers of sand and lime which normally overlie the travertine, and which represent successive floor levels of the original cavern.

In order to inspect the fossiliferous layers *in situ*, one must find a place from which the travertine layer has been removed, and then look up at the roof. I am indebted to Professor Dart for the photograph, which is the only good picture thus far secured of an exposure of this kind. It shows the *under* surface of a layer of bone breccia, with a patch of the cream-colored banded travertine still in place, forming the frieze of the gallery roof. Obviously, quarrying away more of the travertine seam would further sap the cave filling and might reveal a wealth of additional fossil material. In the meantime, more than twenty tons of fossiliferous breccia have gone from the dump to the laboratories at Johannesburg for study, and continuance



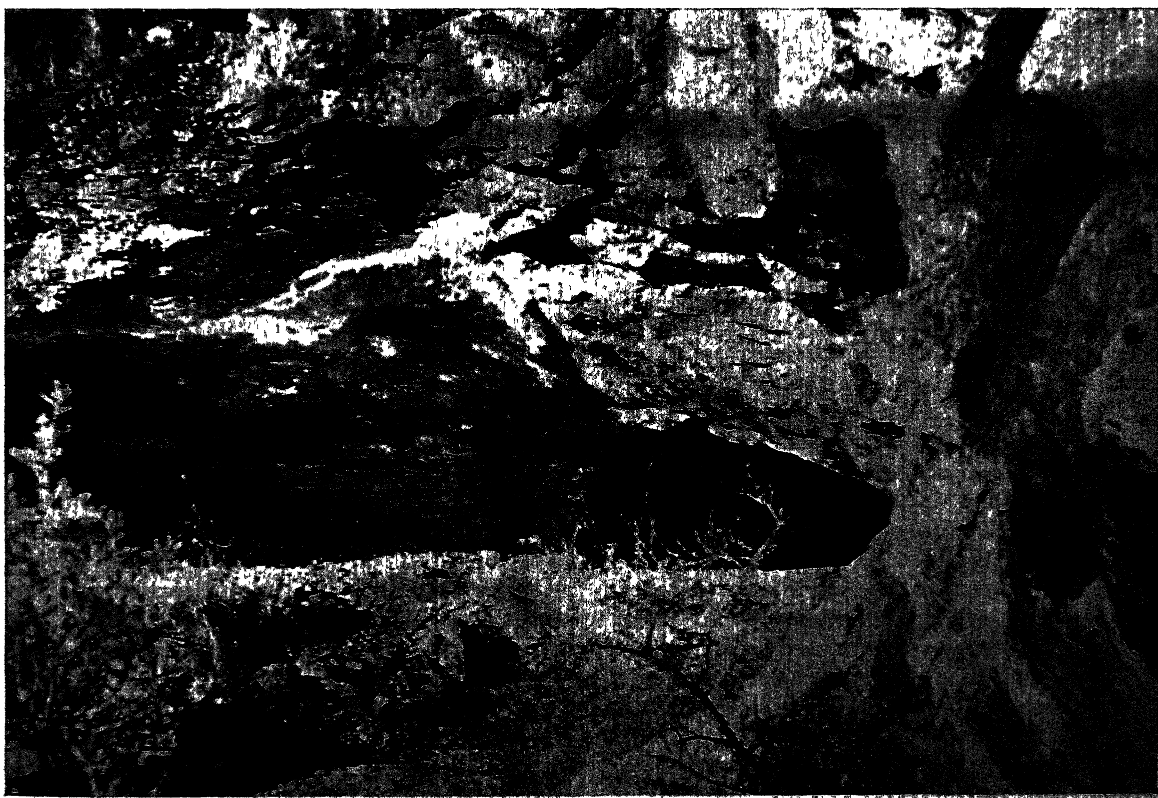
Makapansgat Valley. Relative position of erosion surfaces *M* (Miocene?), *P1* (Pliocene?), *P2* (Pleistocene). (Courtesy of *Ohio Journal of Science*.)

of the work is bound to yield further important results.

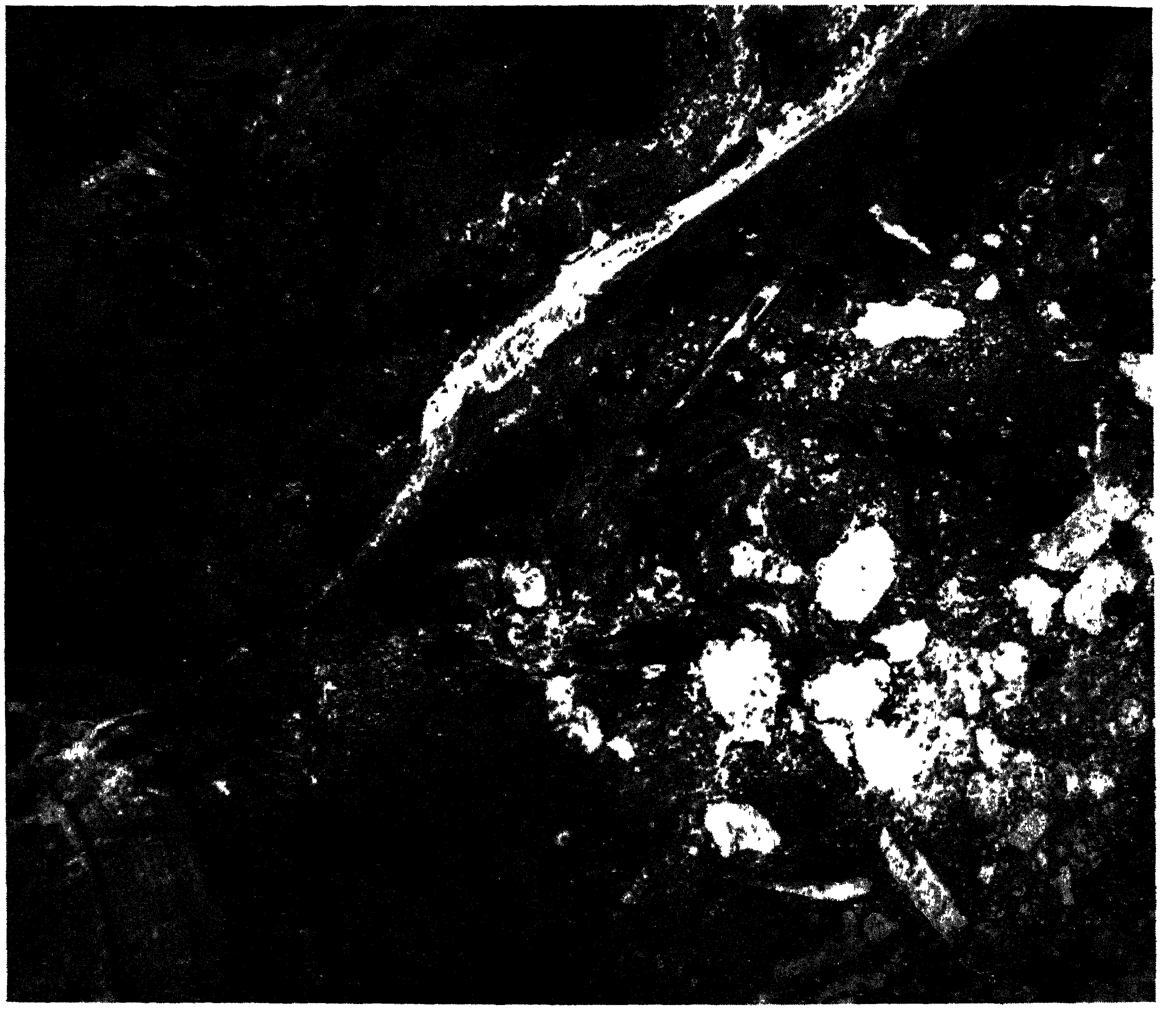
The special significance of the Limeworks discoveries lies not merely in what can now be determined regarding the nature and human status of the creatures who once inhabited these caves, but in the link they furnish between the various other sites from which Australopithecine remains have been recovered. Makapansgat throws light on some of the problems of the caves in the Krugersdorp area—Sterkfontein, where Broom's first discovery of *Plesianthropus* (1936) continues to be followed by further finds; the Kromdraai fissure, in which he found *Paranthropus* (1938 and 1948); the diggings at Bolts Farm, where the University of California Expedition unearthed further faunal material in 1947; and Taungs, the scene of Dart's original find and of recent excavations by the American scientists. The exact status of these various primates from the Transvaal has been a matter of keen debate. The Australopithecinae clearly belong to the same large group that includes both man and the anthropoids. Skull measurements show that their brain volume places them in the hitherto unfilled gap, or "No-man's-land," between the largest-brained apes and the smallest-skulled humans. The dentition is more human than apelike, and there are other anatomical details that appear to be manlike rather than anthropoid. In fact, LeGros Clark has gone so far as to say that there is nothing in their anatomy which definitely precludes the possibility of their being ancestral to man of the later Ice Age days.

II

The party of the University of California Expedition to which I was attached was unsuccessful in attempts to secure further Australopithecine material in any of the two dozen localities excavated in the vicinity of the original Taungs site. But Drs. Camp and Peabody found, in addition to important faunal remains, evidence of peculiar interest bearing on the predatory behavior of these remarkable primates, evidence which deserves special mention. At Makapan and the Krugersdorp localities, the remains of the Australopithecinae invariably occur along with skeletons and broken bones of other animals. Among the rather frequent fossil items are skulls of the baboons *Parapapio africanus*, *P. broomi*, and *P. jonesi*. Dart had been impressed with the number of these skulls that showed signs of having been smashed in from



Entrance to "Southern Cavern," where *Australopithecus prometheus* was found. (Photo by Professor Dart.)

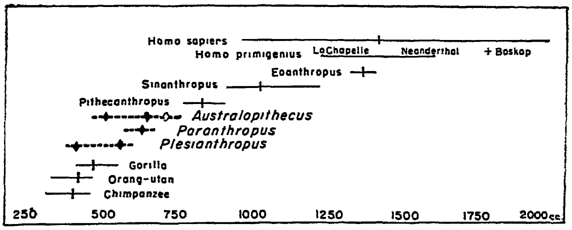


Breccia and burnt bones in cavern roof. (Photo by Professor Raymond Dart.)

the front, rather than crushed from the side, as would be more likely had falling roof rock dropped on a dead animal. Half a dozen baboon skulls found near Taungs by Camp's party showed this same feature. In all, some forty-two out of a total of fifty-eight crania of *Parapapio* from the three Australopithecine localities were found to have suffered the same drastic injury. Careful study of the fossilized brain cases convinced Dart that not only were the assaults committed while the baboons were still alive, but that in many instances the depressions in the skulls show a double indentation, as if made by the twin condyles on the end of the long limb bone of an ox or some other large-hoofed animal. In other cases the blow seems to have been struck with a wooden bludgeon or a stone weapon. It is impossible to avoid the conclusion that *Australopithecus* chose this way of felling his prey. It even seems possible to deduce that *Australopithecus* was right-handed because, although 64 percent of the damaged skulls from the three localities were fractured by blows delivered directly from the front, 17 percent were struck on the left side as against 5 percent from the right side. The remainder showed attacks from the rear, again usually with the right hand.

No stone implements have as yet been identified. But from the bone breccia at the Makapansgat Limeworks has recently come evidence of a very primitive bone and horn culture recalling similar implements found in association with Peking Man in the North China caves which Breuil regarded as showing definite evidence of human shaping. Taken along with the use of fire attested by the charred bones and signs of carbon, the facts point to the handiwork of an early human being who must have been *Australopithecus* himself.

The Upper Pleistocene age of the two cultural



Brain capacities of Australopithecinae compared with those of other primates. (Courtesy of *Ohio Journal of Science.*)

stages represented in the Cave of Hearths and the Rainbow Cave offers no serious problem, but the date of occupation of the Limeworks Cave—which is clearly of much greater geological antiquity—is not so easily determined. The dolomite formation in which all the caves occur has been exposed at the surface for more than half a dozen million years. Caves could have formed and filled whenever ground-water conditions were sufficiently favorable. Since the various caves appear to have formed at intervals over a long period, their contents should not be expected to be strictly contemporaneous. Indeed, there are sufficient differences between the fossil animal types found along with *Paranthropus* at Kromdraai and those from Sterkfontein, where *Plesianthropus* lived, not a mile away, to suggest that the occupation of those two sites was separated in time by some tens of thousands of years. The precise geological age of the creatures is therefore still a subject of discussion. But the faunal association shows sufficient affinities to those of the Villafranchian horizons in Europe and Southwest Asia to warrant assuming an Upper Pliocene date. This age falls safely within the wider, and somewhat vaguer, limits reached by estimates based on other criteria—physiographic, climatic, stratigraphic, or anatomical. It would place the days of these distant African relatives of modern man distinctly more than a million years ago, and shortly before the onset of the colder climate which heralded the Great Ice Age in Europe and North America.

The site of Troy shows evidence of eleven distinct periods of occupation over an interval of nearly four millennia in early classical times. Some of the prehistoric caves in France have revealed as many as six superposed stages of stone-age culture and have been reinhabited in historical, if not recent, days. The famous Choukoutien locality showed signs of human occupation ranging from that of Peking Man in the early Pleistocene up to Paleolithic and later Stone Age times. It has remained for Makapansgat Valley to reveal near-by cave sites entered by men in the Pliocene Epoch, at two stages in the Pleistocene, and again today.

THIRD HAND

I think astronomers must often feel
Like Alice in a stranger Wonderland,
Watching the telescope's long fingers steal
The stars, and knowing what it means to have
Another hand.

FRANCIS BARRY

GROWING QUARTZ CRYSTALS

E. BUEHLER and A. C WALKER

Mr. Buehler joined the Bell Telephone Laboratories as an apprentice in 1930 and has been studying quartz crystal growth in the Chemistry Department since 1945. He is now a technical staff associate. Dr. Walker (Ph.D., Yale, 1923) has been a member of the technical staff of the Laboratories since 1923. He has worked in the field of physical chemistry on problems in textiles and fibers, humidity control and measurement, polarography, vitamins, and ammonium compounds. His wartime experience in growing ammonium dihydrogen phosphate crystals for sonar equipment used to detect submarines was employed in the design and operation of a pilot plant for the growing of ethylenediamine tartrate crystals for telephone purposes and in research on the growing of quartz crystals.

THE *Annual Report on the Progress of Chemistry* for 1947 lists practically all the work on the subject of "Crystal Growth" for the previous fifteen years, yet there is no mention of the growing of quartz crystals. The increased activity in the quartz field since 1946 seems to be due to two factors. The first stems from the realization, in 1944, that despite the increase in value of piezoelectric quartz owing to an expanding demand, new producing areas were not being found either in Brazil or elsewhere. The prospect was further clouded by the possibility that export embargoes or other restrictions might be set up to conserve this important mineral resource. The other factor is that Professor Richard Nacken, in Germany, succeeded in growing some large single quartz crystals by a hydrothermal process as the culmination of about twelve years of geological research on the question of the formation of such crystals in nature.[1]

The Bell Telephone Laboratories started an investigation of this subject in March 1946, based on information gleaned from several investigators who visited Germany after the war, particularly Mr. J. R. Townsend, of these Laboratories, and Professor A. C. Swinnerton, of Antioch College. After a relatively few experiments made with equipment similar to that used by Nacken, and with the process he described, it became apparent that Nacken had made substantial progress in the art of growing quartz at temperatures and pressures near the critical state of water, i.e., about 374° C, and 3,200 pounds per square inch.

This report summarizes further progress that has been made in the Laboratories since March 1946. Several methods of growing large single crystals of quartz have been under investigation. One of these has been brought to a state of development recently whereby clear crystals weighing up to nearly one quarter of a pound have been grown in the brief period of twenty days (Fig. 1).

The process by which this crystal was grown is illustrated by Figure 2. Enclosed in a steel tube, or "bomb," having relatively thick walls to withstand the great pressure developed within it, are the three elements needed. These are a "seed" of clear quartz free of inclusions, misoriented areas, and other defects; a layer of broken quartz in the bottom serving as nutrient SiO_2 for seed growth; and sufficient alkaline aqueous solution to fill the free space within the bomb at room temperature to about 80 percent of its capacity. Long before the liquid is heated to the operating temperature indicated—400° C at the bottom of the bomb and 380° C at the top—the liquid expands sufficiently to completely fill the free space, and the pressure then builds up to about that indicated in the figure —15,000 pounds per square inch. The bomb is hermetically sealed at top and bottom with suitably welded steel plugs and is placed on a hot plate. The bomb and hot plate are enclosed in a well-insulated oven, and this simple arrangement is left to "cook" for several weeks. Growth takes place on the seed in an orderly manner according to the following procedure: The broken quartz dissolves rapidly at the very bottom of the bomb in the hot alkaline solution, consisting of 5 percent sodium carbonate, and forms a strong sodium silicate solution, substantially saturated with silica at 400° C. Motion by convection in the liquid is very rapid, and the dissolved silica is quickly transmitted to the slightly cooler region in the vicinity of the seed, where the solution is then supersaturated with respect to silica. This is a basically important condition necessary for crystal growth, and silica deposits out on the seed. Because of continuous motion in the liquid, the growing surfaces are constantly bathed in fresh supersaturated

liquid and the partially depleted liquid is carried down to the bottom, where it is again saturated at the higher temperature. This process is sufficiently rapid to transmit surprisingly large amounts of silica, and the flow across the growing surfaces is uniform enough to produce the clear growth seen in Figure 1.

This relatively simple process was not achieved without considerable effort, expended not only at the Bell Telephone Laboratories, but elsewhere as well.* In order to appreciate the significance of the progress made in this work it may be well to outline some of the important background information, and the necessary steps leading to the success thus far achieved.

The Nacken process. Even in our first experiments using the Nacken process, clear growth was obtained on suitable quartz seeds in weak alkaline solutions. Conditions were somewhat similar to those shown in Figure 2, in that the seed was suspended near the top of the bomb, a supply of nutrient material was placed in the bottom, and the free space within the bomb was partially filled with the alkaline solution. In the Nacken process, however, advantage is taken of a difference of approximately tenfold in the solubility of amorphous and crystalline silica in such alkaline solutions, and the nutrient used was amorphous silica. Furthermore, the bomb was kept at substantially constant temperature at or near the critical point of water, this being 374.2° C. The liquid filling was equivalent to only about 30 percent of the free space at room temperature, and the alkali concentrations were less than about 0.5 percent calculated as sodium carbonate. In addition to the fact that growth takes place owing to the difference in solubility between amorphous and crystalline silica, Nacken attributed significance to a somewhat mysterious mechanism of transport of silica in the vapor phase near the critical temperature of water.

Presumably the process can be carried out at constant temperature because of the large difference in solubility of the two forms of silica, although the results seem to be somewhat better with slight, controlled fluctuations in temperature about the critical temperature, and with liquid fillings somewhat in excess of 30 percent, this

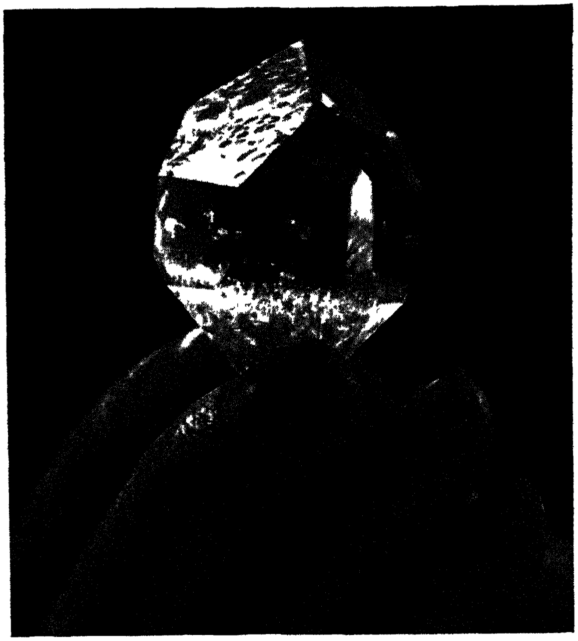


FIG. 1. Fine example of single quartz crystal in the form of a bipyramid, with unusually clear side, or prism, faces, grown under high-pressure hydrothermal conditions at the Bell Telephone Laboratories in a period of twenty days. The roughness on the top face, accentuated by the lighting used, is superficial and similar to surface irregularities sometimes observed on such faces of natural quartz crystals. The original seed consisted of a piece of irregular Brazilian quartz with no natural faces, but with three roughly formed faces on top approximating the natural angles of the major rhombohedron. The bottom of the seed was a flat face cut normal to the vertical, or Z, axis. The Z-cut face had no surfaces even approximating the proper rhombohedral angles. Growth occurred so perfectly on the top faces as to leave hardly a trace of the "join," and the bottom filled out so rapidly to form the natural rhombohedral faces as to entrap some mother liquor and gas in this cloudy area. All the material below the faintly visible white horizontal line was deposited during the twenty-day period, and this line marks the location of the original Z-cut face.

amount being just enough to completely fill the bomb with liquid at 374.2°, and is defined as the "critical" volume. In all cases, however, the results were disappointing because, although growth was surprisingly rapid during the first few hours of operation, it was substantially completed in about eight hours and consistently dropped off to nearly zero after the first twenty-four hours. Careful study of the German reports indicated that the German experimenters experienced the same difficulty and had found no satisfactory solution. Nearly 150 experiments were performed in the Bell Laboratories in an effort to control the rate of solution and deposition of silica, using amorphous silica as the nutrient material. Some of the factors investigated were size and shape of bomb;

* Other workers on this problem have made important contributions to a better understanding of the process of hydrothermal growth. Among those with whom we have exchanged information are Brush Development Co., Cleveland, Ohio; Professor A. C. Swinnerton, of Antioch College, Yellow Springs, Ohio; Squier Signal Corp Laboratories, Fort Monmouth, New Jersey; and the Naval Research Laboratory, Washington, D. C.

solution composition and concentration; addition agents; pressure and temperature; degree of fill; agitation, such as rocking, mechanical stirring, simple convection, and cyclic temperature changes; and continuous feeding of amorphous material.

Extremely rapid, clear growth of quartz is possible under certain conditions with the Nacken process, but because of the great difference in solubility of amorphous and crystalline silica it is evident that the process cannot be depended upon to give high growth rates over long periods.

Because growth did not cease entirely after the first day, but continued at a rate of something less than 0.001 inch per day, and also because the amorphous material became covered with a film of redeposited quartz during the run, it was concluded that further experiments were justified, using crystalline quartz as nutrient material.

It follows from equilibrium considerations that if amorphous and crystalline silica are both present as solid phases, the solution in immediate contact with the amorphous phase must be supersaturated with respect to the crystalline phase to the extent of approximately 1,000 percent, due to the tenfold solubility difference. Experience with water-soluble crystals indicates that with more than about 5 percent supersaturation, spontaneous crystallization cannot be prevented. It appears, therefore, that the Nacken growing cycle is somewhat as follows:

Two to four hours are required for the bomb to be brought up to 375° C, and during this time the alkaline solution dissolves silica from the amorphous layer in the bottom at a continually increasing rate, but not fast enough to achieve the 1,000 percent supersaturation with respect to quartz. Sufficient supersaturation develops, however, to promote rapid growth on the seed crystal suspended from the top of the bomb during this period. As the operating temperature is approached, the liquid becomes more and more supersaturated with respect to quartz, and spontaneous seeding occurs, mostly around the individual particles of the amorphous material. Very quickly each particle becomes enclosed in a continuous layer of fine quartz crystals, the local high degree of supersaturation with respect to quartz falls to zero, and growth practically ceases, except perhaps for the very small temperature difference that may exist between bottom and top of the bomb.

There is no need to consider, in the Nacken process at least, a mysterious transfer of silica through the vapor phase, although this may occur.[2] With amounts of liquid present equal to, or in

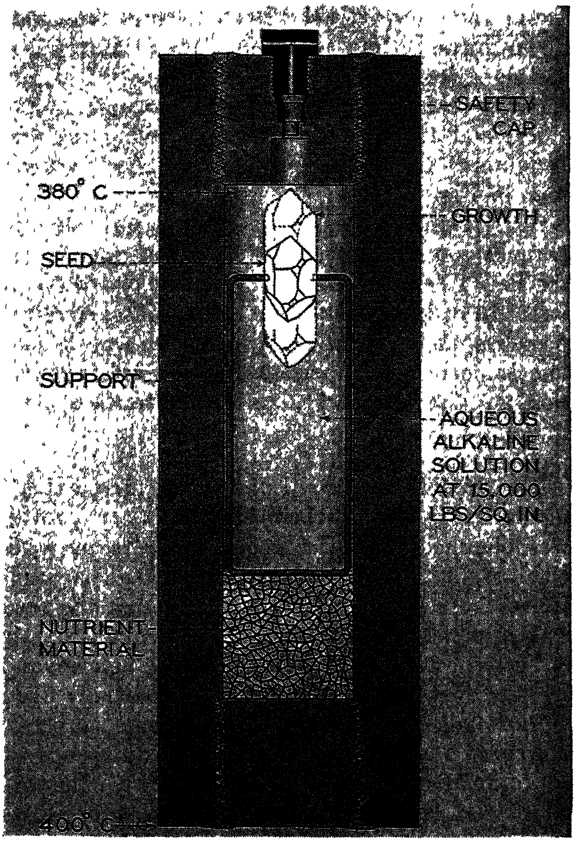


FIG. 2. Section through bomb used to grow quartz crystals.

excess of, the critical volume, the seed plate suspended from the top is either completely immersed in the supersaturated liquid or constantly bathed by liquid in the form of strong convection currents or clouds of droplets,† and conditions are favorable for rapid, clear quartz deposition, so long as supersaturation is maintained. Our experience indicated that growths of as much as 0.003 inch in thickness occur during the first four hours, about 0.006 inch in the first six to eight hours, a maximum of 0.008 inch in the first twenty-four hours, and thereafter the rate decreases to the order of 0.001 inch per day or less.

Growth through density change of solvent. To improve the rate of growth using crystalline quartz

† That violent disturbances occur in the liquid phase within such a bomb at high temperature is indicated by the following: In a bomb 8 inches long by 1 inch inside diameter, the nutrient material—20-mesh quartz—was placed in the bottom in a concentric space around a central wire basket 0.5 inch in outside diameter by 3 inches long, open at the top. After the run at 375° C, with 48 percent liquid filling, *all* the ground quartz was found *inside* the basket which had wire mesh small enough to retain it.

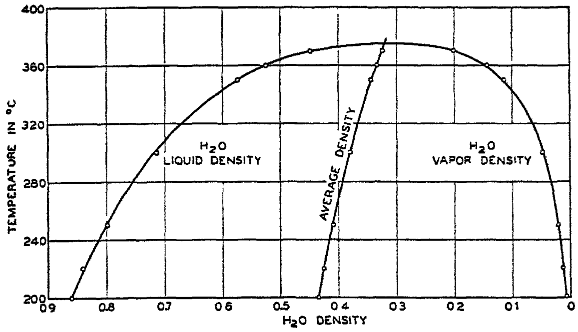


FIG. 3. Liquid-vapor density relations of water near the critical temperature of 374.2° C.

as nutrient material, consideration was given to the possibility of utilizing density changes such as are likely to occur in liquids at the critical point. This may be understood by reference to Figure 3, a graph showing the density changes in water under pressure as the critical temperature of 374.2° C is approached. This temperature is defined as the point at which liquid and vapor have the same density, this being about 0.32 as indicated by the intersection of the average density line with the liquid-vapor density curve. If a bomb is controlled at 372° C ± 2.5° C, the liquid density can vary from about 0.45 at 370° C to 0.32 at 374.2° C, and the vapor density will change from about 0.2 to 0.32 similarly. These are relatively large changes for a temperature difference of but 4° C, and the solubility of silica in either phase should be profoundly affected.[3,4]

Numerous experiments were done to explore this possibility, and growths were obtained (Fig. 4.) Half of this seed plate was located where vaporization of the liquid occurred near the hot wall of the bomb. Appreciable growth is visible on this half, whereas the remainder is substantially unchanged from its original thickness. These density tests were not encouraging, however, because of the narrowness of the growing zone, and spurious seeds were difficult to avoid. Growth was continuous throughout the run and at a rate comparable with the Nacken process in its early stages.

Higher pressures and temperatures. Three important observations derived from the preliminary work indicated the direction of effort leading to the present successful procedure for growing quartz at high rates. These are:

1. Clear quartz could be deposited at rates as high as 0.03 inch per day, but only for short periods of a few hours, and in the presence of much spurious seeding, using amorphous silica as the nutrient material.
2. Growth at very slow rates, perhaps a maximum of 0.001 inch per day, could be sustained with crystalline quartz as nutrient material, and with no spurious seeds.
3. Growth at a rate of about 0.03 inch per day could be sustained with crystalline quartz as nutrient material in narrow zones within the bomb where marked density changes occurred, and this suggested that better results might be obtained at higher solution density.

To secure such higher solution densities it seemed logical to fill the bomb with more liquid, for at the same operating temperature the greater the percentage fill above that corresponding to the critical volume the greater the pressure. In addition, operating at 400° C, rather than at 375° C, in some preliminary experiments, had seemed to give better results. This direction of attack is consistent with observations made in growing water-soluble crystals at room temperatures, where better results are usually obtained with more soluble salts.

These considerations led to the experiments

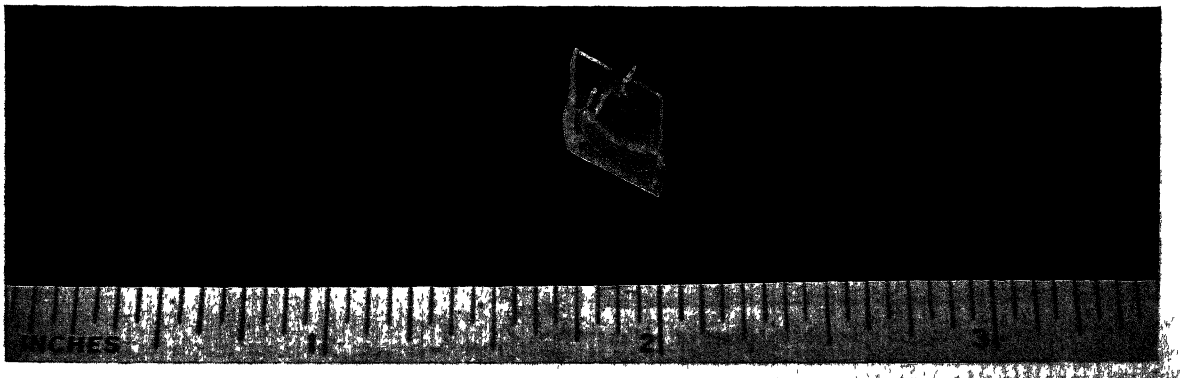


FIG. 4. Quartz plate with growth in zone of density change.

with liquid volumes of 80 percent of the free space at room temperature and with solution concentrations of 5–10 percent sodium carbonate. After overcoming the serious limitations in bomb design and metal creep caused by operating at 400° C, and pressures of 15,000 pounds per square inch, greatly improved rates of growth were repeatedly obtained, up to about 0.1 inch per day. As indicated in Figure 2, the bottom of the bomb was at 400° C, and the gradient to the top was 20° C. These temperatures were measured by thermocouples mounted on the outside wall of the bomb—the internal temperature gradient between top and bottom is probably less than this but cannot be measured with any facilities now available. Tests have been made with bombs having internal diameters of 1 inch, 2 inches, and 3 inches, and essentially the same results may be expected with

FIG. 5. Quartz crystal on right shows effect of sodium oleate on growth—more growth and less veiling of seed plate.

any size bomb when proper consideration is given to obtaining comparable temperature gradients along the length from top to bottom.

Nacken reported best results with several alkaline media in this order of excellence: sodium oleate, sodium stearate, sodium carbonate. In our early work, concentrations of these substances in water ranged from about 0.001 percent to 0.5 percent by weight, and in all cases the clarity and smoothness of growth seemed best in the sodium oleate solutions. At concentrations above 0.5 percent, heat decomposition of this sodium oleate seems to seriously interfere with the growth operation. Attention was therefore turned to solutions of sodium carbonate in concentrations up to 10 percent—in some cases with additions of small amounts of sodium oleate below 0.1 percent.

With these conditions, clear crystals weighing up to six grams were grown in fourteen days and with rates as high as 0.028 inch per day. Spurious seeds were completely absent; all the dissolved silica was transported and deposited where desired. Trials made with amorphous silica as nutrient material, for comparison, invariably resulted in a coating of the inner wall of the bomb with spurious seeds, and some such seeds were embedded in the growing crystal.

Figure 5 shows crystals grown with and without the addition of sodium oleate. The crystal on the right was grown in a solution containing 5 percent sodium carbonate and 0.01 percent sodium oleate; the one on the left, without the oleate addition. The seed plates from which these were grown were 3/8 inch square by 1/2 inch thick, cut with the major faces approximately parallel to one of the natural pyramid faces (they are designated in quartz processing terminology as "CT" plates). In general, clear growth takes place only on surfaces cut parallel or nearly so to natural growing faces, and for quartz these are the major and minor rhombohedral faces; therefore the amounts of growth on these two crystals, formed under comparable conditions of time and temperature, may be seen as the relatively transparent amounts on either side of the thin seed plates, shown edgewise in the picture. It is evident that the crystal grew somewhat longer in the presence of the oleate than in its absence, and furthermore the "join" of the grown material with the original seed is much clearer and better in the test employing this addition agent.

More recently we have grown the crystals shown in Figure 6. Seven plates, each 3/8 inch square by 1/2 inch thick, were mounted one below the other

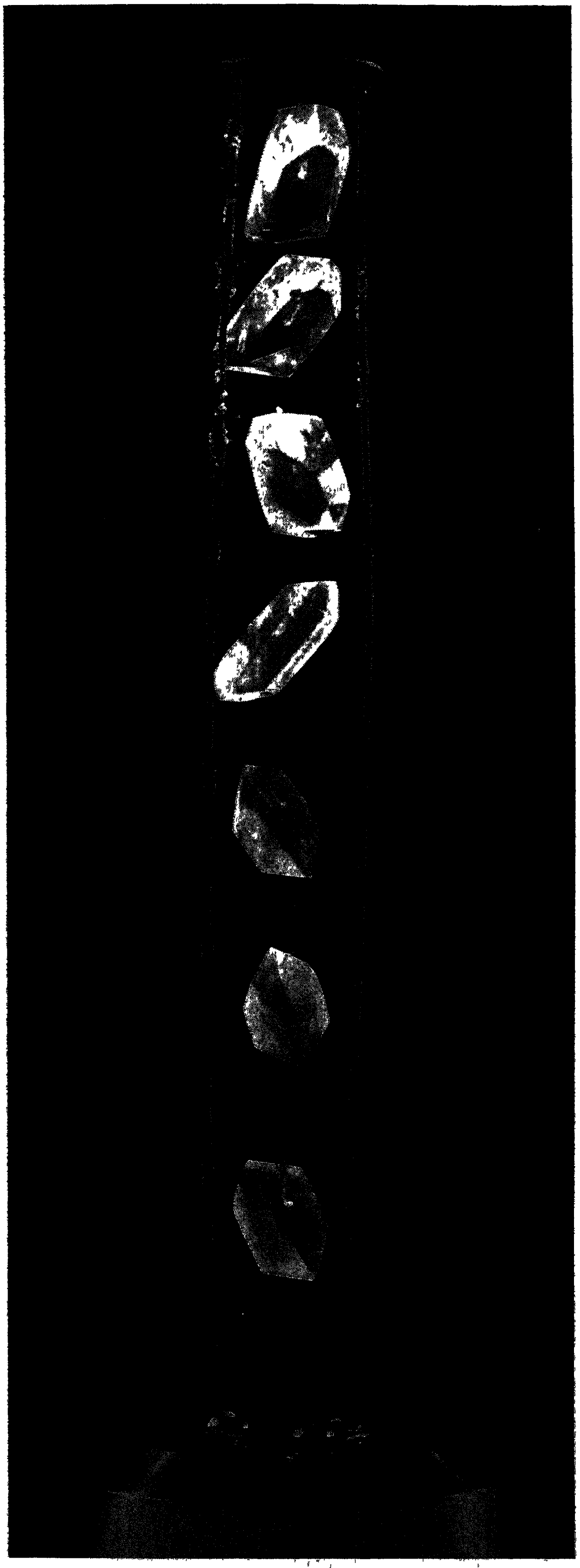


FIG. 6. Arrangement demonstrating growth of several quartz crystals in a single bomb.

in a 1-inch diameter bomb, 12 inches long. The surprising result shown here is that growth in substantial amounts occurs on all these crystals, even on the lower one suspended just above the layer of nutrient material in the bottom of the bomb. Thus it is clear that the entire free space within the bomb above the nutrient material may be used to grow crystals. Furthermore, these crystals grew to lengths of 13/16 inch to 1 1/16 inches in ten days, starting from plate thickness in this case of 0.05 inch. The rate of the largest crystal was of the order of 0.1 inch per day, and that of the slowest, near the bottom, was slightly more than 0.07 inch per day.

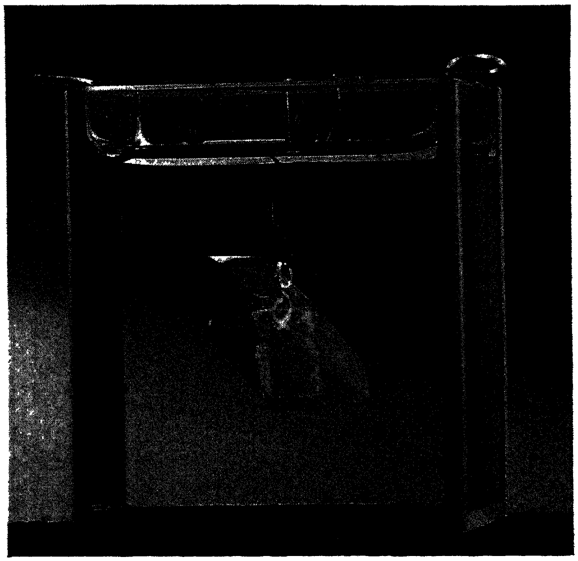


FIG. 7. Quartz crystal in liquid of like index of refraction. Growth is almost transparent, the seed clearly visible.

The growth in this test is clear and unmarred by spurious seeds. This is shown by Figure 7, a photograph of the top crystal in Figure 6, immersed in a liquid having the same index of refraction as quartz, and suitably illuminated. The original seed is plainly visible in outline, but the portions added to each major face are substantially invisible, indicating the clear and perfect quality of growth. The outlines of the full crystal are faintly visible because of slight etching of the surfaces which sometimes occurs during removal from the alkaline liquid in the bomb.

It now appears that the rate of growth of quartz in such bombs, utilizing quartz as nutrient material, is a function of the temperature gradient along the bomb and the size of nutrient material used. Although the true gradient cannot be measured inside

FIG. 8. Effect of bomb leak on quartz growth and spurious seeds.

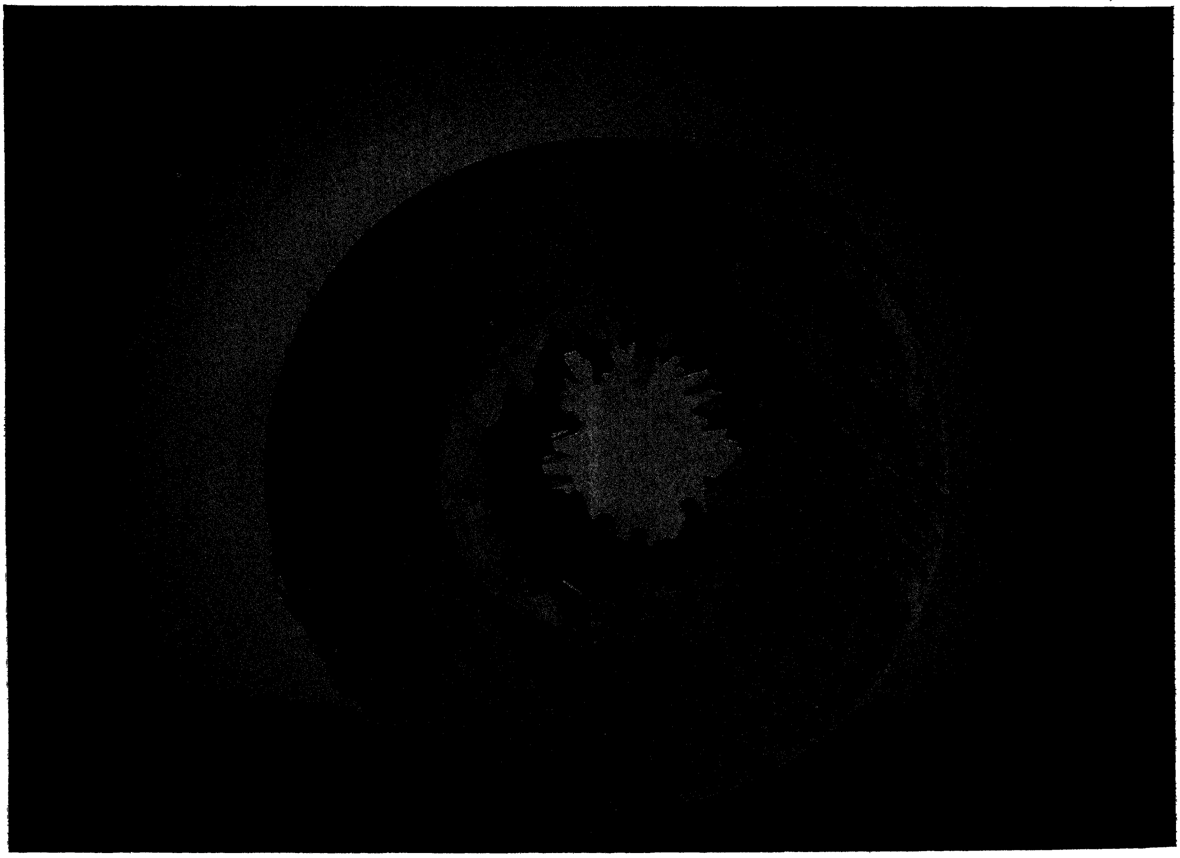


FIG. 9. Section of bomb that leaked, showing geode-like structure of spurious quartz seeds.

the bomb, the apparent gradient as judged by thermocouple measurements made on the outer wall is sufficient criterion for control purposes. It is still too early in the development to quantitatively relate temperature gradient, size of nutrient material, and bomb dimensions to growth rate, but these factors can be correlated, and sufficient progress has been made so that it is clear that large perfect quartz crystals weighing up to one pound or more may be grown at commercially practicable rates under reproducible conditions.

One further development is of interest to geologists. In Figure 8 are shown one large, doubly terminated pyramid, and a smaller, perfectly formed quartz crystal grown from the 3/8 inch square plate shown on the left. The pyramid grew from a double cap seed 1/2 inch long and attained the length of 1 1/2 inches in ten days, or a rate of 0.1 inch per day. This is the first time such a high rate was attained in our work, but it has been repeated many times since. These crystals were grown in two bombs in the same furnace under identical conditions of fill (80 percent), solution (10 percent sodium carbonate), and temperature gradient. The smaller crystals were in one bomb, the bipyramid in another. It was found that the bomb containing the pyramid seed had developed a very slow leak in the top during the run, and about half of the alkaline solution was lost. This leak must have been small indeed, for normal experience with leaks is that all the liquid is lost in a matter of seconds, not days. The rapid growth in the leaky bomb may be explained by one of two possibilities or a combination of both. The leak resulted in a somewhat greater temperature drop in the vicinity of the seed than was the case in the bomb which did not leak, and this caused a greater degree of supersaturation, which increased the growth rate but also gave spurious seeds. Another possibility is that after leaking away part of the fluid the leak sealed itself by silica deposition, and the conditions more nearly compared with those in a bomb having about the critical volume of solution ‡ This smaller amount of liquid may have resulted in a two-phase condition inducing much more violent convection currents than would be the case in the single-phase system. Strong density changes between liquid and vapor may aid greatly in promoting rapid growth. No spurious seeds were found in the bomb which did not leak, and the crystals grew in this bomb at about 0.02 inch per day.

The conditions inside the bomb which leaked, as shown in Figure 9, resembled to a noteworthy degree the conditions reported as being present in some natural quartz deposits, called geodes. Perhaps natural quartz crystals did not require thousands of years to grow, as has been postulated. The escape, through cracks in the earth's surface, of superheated alkaline solution containing dissolved silica may have resulted in the formation of the large natural quartz crystals in a matter of days or months rather than centuries.

REFERENCES

1. Office of Technical Services Reports, PB 6498, PB 18,784, PB 28,897. Dept. of Commerce. Washington, D. C.
2. INGERSON and MOREY, *Econ. Geol.*, 1940, **35**, 772–85, found large deposits of quartz as fine needles near the top of a steel bomb in which many experiments had been performed at high temperatures and pressures, and they concluded that the silica must have been transported to this point through gaseous water.
3. HANNAY and HOGARTH, *Proc. Roy. Soc., London*, 1879, **29**, 324, described the deposition of a solid from a liquid on suddenly heating the liquid being held just below the critical temperature so that it is vaporized in a pressure tube, and attributed this deposition of the solid to rarefaction—change in density of the solvent.
4. KENNEDY, G. C., *Econ. Geol.*, 1944, **39**, 25, described the changes in solubility of silica glass in water on passing through the critical temperature region, and concluded that the data "strongly suggests that solubility in the gas phase is not only a function of temperature, but is very much an inverse function of the specific volume of the water vapor."

‡ An 80 percent fill, with 10 percent sodium carbonate solution, at 400° C, may not be much above the critical conditions for this concentration. Therefore, it is possible that the bomb without a leak contained but a single liquid phase under these conditions, whereas the leaky bomb may have had two phases near the end of the run.

SOME OF MY BEST FRIENDS ARE SCIENTISTS

F. BARROWS COLTON

Mr. Colton is president of the National Association of Science Writers and a member of the editorial staff of the National Geographic Magazine. *He was for four years on the staff of the Springfield* Republican *and for five years a reporter and science writer for the the Associated Press. He was the sole correspondent at sea with the U. S. Atlantic Fleet on war maneuvers the year of Pearl Harbor and in 1947 was a member of the National Geographic Society–U. S. Air Force solar eclipse expedition to Brazil.*

JUST over one hundred years ago, when Dr. W. T. G. Morton demonstrated ether anesthesia in Boston, it was a newspaperman, Albert Tenney, of the Boston *Evening Journal,* who publicized the story and helped bring the new technique to the attention of surgeons. There is nothing new, therefore, in the interest of the press in science as a source of news, nor in its ability to render a real service to science through publicity.

The use of ether anesthesia to relieve the pain of surgery was big news in 1846, which was why Albert Tenney wrote the story. Tenney was no science writer, in today's professional sense, but he was an alert newspaperman who knew a good news story when he saw one. That is still the most important fact about science writers of the present day. They are primarily reporters looking for news stories that will interest their readers. In other respects, science writing has come a long way since Tenney's day. It has become a valuable adjunct of science, an important aid to public education, and a rapidly growing specialty in the field of journalism and radio.

It is probably safe to say that science writers understand scientists better than scientists understand science writers. But science writers are so important to science that scientists need to have some knowledge of their working methods and problems.

In the process of writing about science, modern science writers not only are covering the news but are rendering a real service to science as well. Publicity is gradually giving the public not only more knowledge of science but an understanding of, and respect for, the scientific method of working and thinking. It is helping to build up an interest in science among the people who pay taxes and contribute to endowments to support research.

No longer does popular fancy place the scientist in his old role of a kindly but rather peculiar old chap with a beard who spends his time in such seemingly obscure pursuits as chasing butterflies or collecting rocks. The scientist has become the man who released atomic energy, developed radar and television, made rain with dry ice, and performed near miracles with new medical and surgical techniques. His prototype is no longer the shy professor but a clean-cut, rather youngish fellow, alert and up to date, who is doing things the layman realizes to be practical and worth while.

This increase of popular interest in, and respect for, science has resulted in a growing use of science news by newspapers, magazines, and radio, and a corresponding rise in the number of science writers, who report and explain the news of science to the layman. They perform an essential and specialized role. They are in a very literal sense the "interpreters" of science, translating its variegated technical dialects into words that the newspaper, magazine, and radio audience can understand. That audience, incidentally, includes many scientists who cannot possibly keep up with all developments outside their own fields.

From small beginnings, science writing has grown into an important, well-established, responsible branch of journalism. Science Service, endowed in 1921 by the newspaper publisher E. W. Scripps, pioneered in the serious reporting of science for newspapers. Once this agency was almost alone in the field, but today there are scores of science writers on the staffs of the press associations, individual newspapers, and weekly and monthly magazines, which is the best possible testimony to the soundness of Mr. Scripps' judgment in encouraging popular science writing. In the magazine field, a pioneer was my own publication, the *National Geographic Magazine,* whose editor, Dr. Gilbert Grosvenor, began publishing accurate, understandable science articles as early as the turn of the century. These were profusely illustrated, for photographs, especially in color, often tell a story better than words. Today all magazines appealing to the general reader cover science.

When I began writing about science nearly twenty years ago, full-time science writers were

few, and they had to work under the handicap of the bad name that many newspapers had acquired by treating scientific meetings as subjects for the staff humorist, by inaccuracies, and by oversensationalism.

Today the National Association of Science Writers, organized in 1934 to promote accurate, responsible science reporting, has nearly 100 active and associate members, with high standards for admission to membership. There are many able interpreters of science in the information and public-relations offices of government agencies, universities, research foundations, and industries. A number of these are associate members of the N.A.S.W. Many other writers who do not cover science full time report it ably on occasion.

The N.A.S.W. is an affiliated society of the American Association for the Advancement of Science. In 1938, only four years after it came into existence, the organization was awarded the Clement Cleveland Medal for its contributions to the control of cancer through publicity. To encourage new science writers, the AAAS administers the George Westinghouse Science Writing Awards, through which two prizes are given annually by the Westinghouse Educational Foundation for the best newspaper and magazine articles on science. Thus today science writing has become a well-recognized, respected, and generally accepted adjunct of science in the United States. It is helping to gain support for science and to educate laymen in scientific thinking.

When I want to plague some of my scientific friends, I tell them how I became a science writer. One day in the Associated Press office in New York my boss, never noted for wasting words, sent for me and rather bluntly asked: "Ever write any science?" I admitted having covered a few assignments on scientific subjects. "Ever have any science in school?" I confessed that I had had two bouts with chemistry in high and preparatory school, in both of which I came off the loser, but that in college I had passed in botany and geology. The boss's expression did not change at this hardly impressive recital of scientific background. He said: "How would you like to go down to Washington and cover science?" I said I would like to try it.

Contrary to what some scientists may darkly suspect, this story does not prove that the newspapers or magazines think so little of science that they carelessly assign any unqualified, uninformed reporter to cover it. Its real point is that a science writer first of all must be a reporter. He must have the layman's point of view. In writing his material scientist. He knows what interests the layman and, equally important, how much the layman can understand and absorb. His training qualifies him to do this. The scientist's training usually does not.

Of course any science writer must learn about science, and he does, partly by reading, but even more through his daily experience in covering science news. Science writers try hard to build up their background, to get the news straight and write it accurately, but they cannot do the job alone. It is up to the scientist to learn how to explain his work so that the writer understands. With a little thought and effort this can be done.

Once the press service director of the AAAS telephoned me at the Associated Press to say that some abstracts of papers to be read at a forthcoming meeting of the American Mathematical Society in Minneapolis had been sent to him. My first reaction was that there could not be any news in mathematics. But with the patient help of a mathematician we were able to pick out a few subjects that would have some popular interest when the implications of the results were explained. Half a dozen news stories were written which described the applications of some of the mathematicians' work but contained no mathematics whatever.

Newspapers printed the stories. They helped show the public that mathematics does have some practical use. One newspaper editor congratulated the AP on having a reporter who understood higher mathematics. (I've never heard the last of that one!) Thus the science writer can successfully cover a subject of which he knows little by enlisting the aid of scientists who can explain it and because he himself knows what will interest the reader.

Before a small group of scientists and writers discussing science and the press, I once made the rash suggestion that it would be very helpful if all scientists had at least a brief course in journalism, or something akin to that, as part of their training. Of course this aroused anything but enthusiasm. If scientists did have such training, however, they would acquire a better understanding of the problems of the science writer, and would be better able to explain and write about their own work in clear and interesting fashion. It is significant that the winner of the 1948 AAAS—George Westinghouse magazine science writing award was a scientist who had once worked on the Brooklyn *Eagle!*

To illustrate the pitfalls of scientific jargon for the layman, Dr. George R. Harrison, of MIT, tells this classic story as reported in the Washington *Star:*

a limited command of English wrote the National Bureau of Standards and said he found that hydrochloric acid quickly opened drainage pipes when they got clogged and asked if it was a good thing to use.

A Bureau scientist replied:

"The efficacy of hydrochloric acid is indisputable, but the corrosive residue is incompatible with metallic permanence."

The plumber wrote back thanking the Bureau for telling him the method was all right. The scientist was a little disturbed and showed the correspondence to his boss—another scientist. The latter wrote the plumber:

"We cannot assume responsibility for the production of toxic and noxious residue with hydrochloric acid and suggest you use an alternative procedure."

The plumber wrote back that he agreed with the Bureau—hydrochloric acid works fine. A top scientist—boss of the first two—broke the impasse by tearing himself loose from technical terminology and writing this letter:

"Don't use hydrochloric acid. It eats hell out of the pipes!"

Any science writer will tell you that one of his perennial problems is getting scientists to explain their work in simple, brief, clear language. When he finds one who can, he is likely to be suffused with a happy but unfamiliar glow. Any magazine editor can tell you sad tales of his struggles to get eminent scientific contributors to write interesting and understandable articles.

All this is clear enough, since most scientists have no training in popular writing, are accustomed to thinking and speaking in technical terms, and have little conception of how limited a background the average layman has in science. There are exceptions—some scientists are outstanding in their ability to speak and write about their work in simple terms. Albert Einstein has done a good job of explaining his own theories of relativity. Dr. Alexander Graham Bell once said that a good scientist could explain his work understandably to any intelligent layman.

On the other hand, the science writer is an expert, in his way, in the task of explaining science to the layman. He does it, sometimes, in ways that some scientists do not like, but in general that is because they do not understand his problems. One problem is to write a story that people will read. If it is not read, it might as well not be written. The scientist reads technical journals as a duty to keep abreast of developments, but the newspaper or magazine reader feels no such compulsion. If a arouse his interest, he will quickly turn to something that does.

Also, to get a science story printed at all, the writer must arouse the interest of a busy editor who is pretty ruthless about what does or does not get onto a crowded press association wire or newspaper page. He must therefore put the main interest and point of his story in the first paragraph, and do it in words that not only summarize the story but tell it interestingly enough so that the reader will read on.

If someone has discovered a successful treatment for the common cold, that is what the reader wants to know. He has little interest in the history of the search for such a treatment, unless the story is unusually dramatic and colorful. A science news report must be written with the essential facts stated first, followed by the name of the person or persons who did the work, and what little background is necessary for understanding what was done. This is exactly the opposite of the procedure in writing a scientific paper, in which the previous work upon which the present result is based is meticulously outlined, with proper credit to all concerned, and with the new material placed near the conclusion.

Space, too, is one of the science writer's problems. On the jam-packed wires of the Associated or United Press it is a rare science story that rates more than 400 words. Even individual newspapers seldom give their staff science writers much more space than that, unless the story is a truly big one. Actually, 400 words or so is about all the average newspaper reader wants to bother to read. Into that brief space the writer usually must crowd enough essential details to make his story accurate, enough background to show why it is important, enough color to make it readable, and the names and affiliations of the scientists who did the work.

Time also is forever prodding the reporter. Newspaper and magazine deadlines are inflexible. An Associated Press reporter, for example, knows that somewhere in the United States some newspaper that takes his service is going to press almost every minute of the entire day or night. Time especially is a factor when a science writer is covering a scientific meeting. His reports of the addresses on the program usually must be written before the papers are delivered if they are to get to the newspapers on time. Accounts of papers read at a morning session, for example, normally appear in the afternoon newspapers of the same day. An afternoon newspaper is made up very early in the morning, so all but the most important news stories that appear in it must be written the

A morning newspaper is made up during the previous evening and night hours; therefore, news reports of papers read at an evening session likewise must be on the way to the newspapers about the time the speaker is being introduced, and preferably earlier. If a reporter is forced to go and listen to an evening address, his story must be written so late that it probably will not get into the next morning's paper at all.

Press association reporters have even earlier deadlines. Their stories for morning papers must be moving out on the wires by 4:00 or 5:00 P.M. the day before, and for evening papers by the previous midnight. All this has been said many times before, and many scientists have paid attention and have been cooperative, but the point bears repeating again.

It is of vital importance that speakers at scientific meetings provide complete copies of their papers *in advance* to the press, if possible at least twenty-four hours ahead of the time of delivery. Then the reporter can write his story at some leisure, with all the facts before him, and he has time to seek out the speaker for additional particulars if necessary. If he has no copy of the paper, he must try to waylay the speaker whenever and wherever he can to learn what he is going to say, bothering him perhaps at inconvenient times, and making hasty notes that are far less satisfactory than a copy of the paper, and more likely to result in an inaccurate or incomplete story.

Even an abstract is better than nothing, provided it really summarizes the speaker's conclusions. But if you want to make a science writer see red, wave before him an abstract such as this: "A new treatment for the common cold will be discussed, with results of its application to 1,000 volunteer subjects. The percentage of favorable responses will be stated, and the chemical composition of the drug and method of administration of the treatment outlined."

In the case of unusually technical papers, it is extremely helpful to provide a brief nontechnical explanation of the main points in addition to a copy of the paper itself. The extra time this takes probably will be no more than the speaker otherwise would have to spend in explaining the more obscure points of his paper to reporters who would seek him out for enlightenment anyway. He will make a real contribution to the good relations of science and the press and to the public's better understanding of science.

Many scientific organizations already are doing a good job of making available complete information not only about the papers delivered at their meet-

Another thing to be remembered is that all reporters want to get what is known as "a good play" for their stories. This means that they try to produce news reports that will be given front-page space in a newspaper, or at least a top-column position on an inside page, or a correspondingly prominent position in a magazine. For a press association reporter, a good play means that a high percentage of the newspapers receiving the service use his article prominently. Obtaining a consistently good play means prestige and salary increases for a reporter. It is the best possible stimulus to his morale.

Most important factor in getting a good play is the subject matter of the story, which must have the widest possible interest for all types of readers. That is why medicine gets a better play than phytopathology, and why making it rain with dry ice gets more headlines than the discovery of a fossil dinosaur in Wyoming.

No honest science writer will distort facts to get a good play, but he will use all legitimate means to make his story interesting. He is likely also to keep the headline writers in mind, and to insert in his lead paragraph, if he can conscientiously do so, a word or phrase that will fit conveniently into a headline and attract attention to the story. After all, he is striving to get people to read his report.

Here is a good place to emphasize again that reporters do not write the headlines on their stories. This is done by the newspaper copy desk, which coldly appraises the news value of every story that goes into the paper, and strives to write a headline that will summarize what a story is about and attract attention to it. (Critics of headlines might try writing a few sometime—they would soon find that only a certain number of letters and spaces will fit in a line between the two sides of a column.)

Another much-discussed point in science writing is what constitutes accuracy. No science writer worthy of the name is ever deliberately inaccurate, and takes all possible care to avoid it. Yet, inevitably, someone is accused of being inaccurate, of using wrong emphasis, of telling only part of the story, or of failing to give proper credit to all who have contributed to some scientific accomplishment.

One veteran science writer has truly said that there are two kinds of accuracy, equally legitimate —that of the technical journal and that of the newspaper (or magazine) report. No one is going to take the popular account and use it as a guide to reproduce the work that it describes. Any scientist who wants further information will get in

E. Slosson, first editor of Science Service, once said: ". . . The would-be popularizer is always confronted by the dilemma of comprehensible inaccuracy or incomprehensible accuracy and the fun of his work lies mainly in the solution of that problem."

Ninety-five percent, probably, of the laymen who read a science story will be interested only in the details given there. Anything more would be boring if not incomprehensible to them. Further, they will remember only the gist of the story, very likely not much more than was mentioned in the first paragraph or two. Any attempt to give full technical details would make the story unreadable and dull. A news story can be accurate in essential details without being complete. As another science writer has said, a science news story need be accurate only to the first decimal place. Such a story is far more likely to be printed and read, and to give science favorable publicity, than a long, technical article so filled with meticulous detail that it is over the heads of most laymen.

For the same reason, it is unwise to burden a story with a long list of names and affiliations of all those who have contributed even in a minor way to the result. These names mostly will mean nothing to newspaper readers. They will take up precious space and bog down the story's readability.

In some ways the magazine science writer has an easier problem than a newspaperman. His space is more generous, he can cover a subject more fully, and more often can take time to have his copy carefully checked. Otherwise, however, the magazine science writer must follow the same general rules as the newspaperman. From the first sentence his articles must try to catch and hold the attention of the reader. The reader of any popular magazine wants to be entertained, or at least informed in an entertaining and interesting manner. He cannot be expected to read an article that does not hold his interest. So the magazine science writer must strive constantly to hold his reader's interest, to be crystal clear, to avoid dullness at all costs, to inject humor, thrills, anecdotes, and human interest. Though he can use more detail and background than the newspaper writer, he must make these as interesting, dramatic, and colorful as the main point of the story.

Science writers sometimes feel that some scientists are oversensitive about publicity, somewhat lacking in a sense of humor and proportion. No one wants scientists to adopt the classical politician's attitude of "I don't care what you say about me as long as you mention my name." But the cause of good publicity for science would be helped if scientists would venture a little further from their ivory towers, if they would develop slightly thicker skins at times, and not eternally condemn all the press merely because in one isolated instance they have had their work written up in what they consider a somewhat inaccurate or incomplete way. One reason for this sensitivity, of course, is the reaction of the scientist's own colleagues, who may criticize him severely, or at least kid him about the news story almost as though he had written it himself. Science writers find it hard to understand why a man should be blamed for getting publicity when he has done something of legitimate news value.

Publicity for science pays off in unexpected ways. News stories about research in physics and the sense of smell at Yale brought that university large financial gifts for further work along those lines. A news story about stone dart points made by Folsom man in New Mexico brought a flood of letters to the Smithsonian Institution showing that such points had been found all over the United States, indicating a far wider distribution of this ancient people than had been realized. A scientist published in a technical journal an account of raising bigger and better baby chicks by feeding them tobacco of high nicotine content as protection against disease. It attracted little notice, but a newspaper story about it brought him a flood of inquiries from chick raisers.

Good science writing is not easy. In his technical journals the scientist knows he is writing for an audience that understands his specialized language. The science writer must try to reach an audience that ranges all the way from scientists themselves to people with extremely limited background. I still remember the telegraph operator, sending an Associated Press dispatch, who looked up from the copy and said, "What's this word 'physics'? I never heard that before."

A few science writers have acquired so much knowledge of science that they seem to have lost the layman's approach. They get too technical. I read one article by such a science writer that was so obscure in its meaning that I had to get a physicist to explain it to me.

In trying to set forth the science writer's point of view, I may have seemed to preach at the scientists. This is done, however, in a friendly spirit. Mutual understanding is the best way to promote good relations between scientists and the press. Some of my best friends are scientists!

NEW WORLDS FOR STUDY

JAMES HILLIER

Dr. Hillier (Ph.D., Toronto, 1941) has been a research engineer at the RCA Laboratories, Princeton, New Jersey, since 1940. He has specialized in electron optics, microscopy, diffraction, and microanalysis. His article is from an address given during the AAAS Centennial Celebration in Washington, D. C., September 1948.

IN AN absolute sense one might consider including the electron microscope in a series of lectures concerning giant machines for research to be an error, for it is indeed a dwarf compared to the new particle accelerators. On the other hand, what it lacks in physical size it gains by the breadth of its applications.

In the scientific study of the structure of matter, man's interest has shifted to progressively smaller "fundamental" particles. The work with the large accelerators concerns the study of the present-day fundamental particles and represents the frontier of our basic knowledge in science. By contrast, the electron microscope is used to study a range of larger structures which, for various reasons, including lack of instrumentation, has received scant attention in the past. This is the range in which the molecules are organized into the solid structures with which we deal in everyday life. Although the work with the electron microscope cannot be considered fundamental in the present physical sense, it can be considered as representing the frontiers in many sciences. Now the biologist, metallurgist, geologist, industrial chemist, engineer, or, for the matter, anyone interested in the organization of molecules into heterogeneous solid structures can begin to link his studies with those of the physicist and chemist. More specifically, the electron microscope provides the only means for studying the heterogeneous organization of solids between the level of the large molecule and that which is an order of magnitude above the resolving limit of the light microscope. It is obvious that the number of structures to be studied in this range is very large and that the electron microscope has, in fact, provided science with a new world for study.

The magnitude of this new world is difficult to comprehend and even more difficult to describe. When we discuss the capabilities of the electron microscope, we customarily use the term "useful magnification." At the present time we say that the best electron microscope can provide a useful magnification of 200,000 diameters on a suitable specimen. This is a large number, a number that falls in the same category as many numbers of interest to the astronomer, and quite beyond true comprehension by the human mind. In an attempt to realize some of the significance of this number, let us consider the role of the electron microscope in another way. Let us suppose, as an example, that a research worker took as a project the examination under the electron microscope of an entire plant or an entire animal. He would soon find that he would not live long enough to complete his work. A very rough calculation would show him that it would require forty years to take just a fast glance at the structures in one square inch of the surface of his object. If he wanted to photograph that square inch for careful study at the highest magnification, he would find that more than six thousand years would be required for the job. When we consider that all the objects of nature involve organization in volume, the time required to make the complete study of even a small portion of that object again goes beyond the powers of comprehension.

A scientist has no other means of depicting the heterogeneous organization of matter in the range of dimensions extending from the limit of the light microscope down to the level of a large molecule. In the study of crystals the very regular arrangement of the molecules makes them susceptible to study by interference techniques, whereas in the case of simple plant viruses and of some animal viruses it is the uniformity and simplicity of external shape that make them susceptible to studies with the ultracentrifuge, with polarized light, etc. Since these constitute only an extremely small proportion of all the structures that may be encountered in this range, it may be said that the new world opened by the electron microscope is a completely unexplored one. The only landmarks in it are those around its edges. They are to be found in the studies made near the limit of the light microscope or in the studies made by chemists interested in the larger molecules. Quite obviously, the scientific explorer who ventures into this new world must use these landmarks as bases for his expedi-

Electron micrograph of the initial stage of germination of a spore of a soil bacterium (*B. mycoides*). Obtained in collaboration with Dr. George Knaysi, Cornell University. (Magnification, 21,000 ×.)

tion and must maintain close contact with them throughout his research; otherwise he is in great danger of becoming hopelessly lost.

At the present time a survey of the literature showing the results obtained with the electron microscope would reveal, in a rather striking way, that the valuable work is being done by those groups, and only those groups, who have a thorough understanding of this situation. At the same time, such a survey would reveal that, as yet, no group has succeeded in penetrating very far into this new world. We have on the one hand a number of workers who use the techniques of the high-molecular-weight chemists and physical chemists as a base of operation. These groups use light scattering, low-angle X-ray scattering, X-ray diffraction, the ultracentrifuge, electrophoretic measurements, polarized light measurements, etc. as the starting point for their explorations with the electron microscope. On the other hand, a number of workers, in particular the bacteriologists, the metallurgists, and the industrial chemists, have used the high-powered light microscope as their starting point. It is by no means my intention to belittle the already important contributions of the electron microscope, but it is nevertheless my opinion that the really important contributions of this new instrument will come when these two groups of explorers begin to make contact in the inner regions of this new world.

I

Let us turn now to a more specific examination of the electron microscope in an effort to define more precisely its place in scientific research. As most readers are undoubtedly aware, the resolving power of the light microscope is limited by the relatively long wave length of the illumination used. This is a fundamental limitation which prevents any microscope from resolving object points separated by less than half the wave length of the illumination used. In other words, a light microscope using visible light can never be expected to produce useful magnifications in excess of approximately 1,000 diameters. If ultraviolet is used, this limiting figure may be raised to 2,000 or 2,500. The electron microscope owes its existence, at least in part, to this limitation and to the fact that almost twenty-five years ago L. deBroglie suggested that electrons must also have something periodic in their nature. In other words, a wave length could be associated with them, and, according to DeBroglie's formula, that wave length would be very small. For electrons of the velocities used in the electron microscope that wave length is roughly 1/100,000th that of visible light. DeBroglie's work did not immediately bring up the possibility of an electron microscope with high resolving power. That concept had to await publications of H. Busch, in which he showed that any electric or magnetic field possessing axial symmetry would act as a true electron lens for the electrons passing very near to the axis of symmetry. Shortly after Busch's work it was pointed out that there was in effect a complete analogy between geometrical light optics and geometrical electron optics. Thus, in principle at least, it would be possible to construct electronic counterparts of any optical equipment. In the early 1930s a few research workers were already interested in constructing electron microscopes, and over the succeeding years the development has been very rapid, with all the scientifically active countries contributing their share to that development. There are now several highly practical instruments produced commercially in various parts of the world. During its development the fundamental design of the electron microscope has changed very little. In fact, that development has involved mainly the study and understanding of the most fundamental form of the instrument.

As has been stated in the literature many times, the electron microscope is a rather exact analogy of a light microscope. Thus, the light beam is replaced by an electron beam, and the sequence of glass lenses—the condenser, the objective, and the

eye lens—is replaced by a corresponding series of electric or magnetic field lenses. The electronic system differs physically in that the path of electron beam must be highly evacuated, so that tricky mechanical devices are necessary to permit insertion of the specimen and the image-recording means in the microscope. It differs also in that the electronic image is always projected as in photomicrographic apparatus and that it is necessarily observed visually only after transformation into a visible image by means of a fluorescent screen. This is done during the operation of the instrument, but a photographically recorded image is always made for subsequent careful study.

In use in research problems the electron microscope provides an image, in a true geometrical

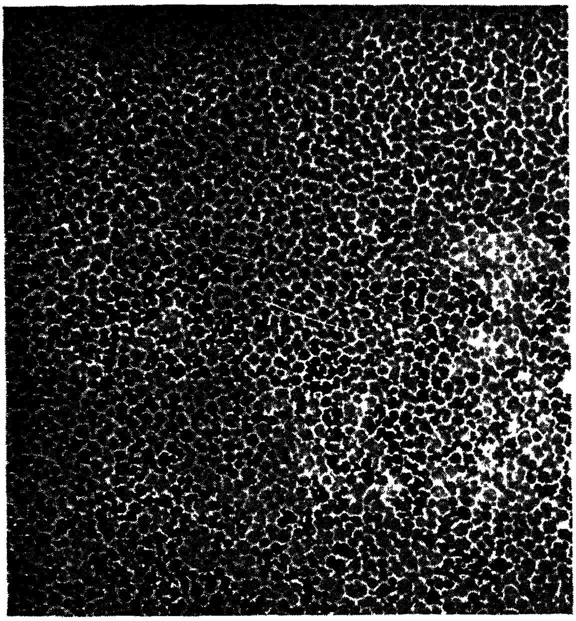
Asbestos fibers. At this magnification the asbestos fibers take on the appearance of fine tubes. (30,000×)

sense, of the object placed in it. This image differs considerably in quality from the image produced in a light microscope. This is in addition to the fact that the image possesses better resolution. When the electron microscope was first introduced, the most commonly voiced objection to it was that it would be exceedingly difficult to interpret the images obtained. Such objections were justified only if their authors realized that exactly the same objections could be raised in the use of a light microscope. It is only in an exceedingly small fraction of the use of any microscope that interpretation is made by a direct analysis of a single image. Instead, it is much more common practice to use the microscope merely as an aid to our sense of vision, in which method the individual or isolated image becomes valueless. It is only from the relationship of one image to many others, from the experience of the microscopist, and from the conditions of the experiments that the value and interpretation of the microscope image are obtained.

As with the light microscope, the images produced by the electron microscope often possess ambiguities that can be resolved only on the basis of extra experiments. Since these ambiguities are quite often the result of the specific mechanism of image formation, it is not surprising that those encountered in electron micrographs are quite different from those encountered in photomicrographs. To understand them, it is necessary to go further into the mechanism of image formation in the electron microscope.

The electron microscope is essentially a monochromatic instrument, and hence differentiation of the image points must be produced by variations in intensity. In the electron microscope, these differences in intensity are produced for the most part by differences in the electronic scattering power of the different object points. The electrons irradiating an ideal object point constitute an extremely sharp, solid cone, at the apex of which the object point is situated. Some of the electrons that traverse the specimen are scattered, however, so that the electrons leaving the object point constitute a rather diffuse and somewhat broader cone than the incident electrons. It should be pointed out, in passing, that absorption of electrons by the specimen does not play a significant part in the determination of the intensity of the image points. The electrons in the outer edges of this cone are prevented from reaching the corresponding image point, either by an apertured diaphragm in the objective lens, or by other characteristics of that lens. This causes diminution of the intensity of the image point. The electronic scattering which occurs within a given point depends upon the thickness and the density of the specimen at that point, and hence the variations of intensity in the image are, in a sense, a measure of the variations in the mass per unit area of the specimen. This leads to the first ambiguity that is encountered in electron microscope images—namely, that in a single image it is not possible to determine whether a reduced intensity corresponds to an increase in thickness or to an increase in density. In practice, there are numerous methods that resolve the above ambiguity. These methods usually involve demonstrating the three-dimensional geometry of the specimen independently of the density differences. Stereoscopy, which can be accomplished at high magnifications, or shadow-casting are two possible methods.

An ambiguity that is much more difficult to resolve arises with regard to the dependence of resolving power on contrast. In light microscopy, it is well known that the numerical aperture of the objective determines the resolving power and also has some (though little) effect on the contrast obtainable in the specimen. In the actual use of the light microscope, however, contrast is usually considered to be independent of the resolving power and is introduced by means of specific absorption such as that produced by specific staining. In electron microscopy, as already indicated, contrast is obtained by differentiating among the differing scattering powers of the object points. This control is accomplished for the most part by the aperture of the objective lens, but this aperture also controls the resolving power by controlling the aberrations in the uncorrected objective and by controlling the diffraction effects. Experimental work has shown that those apertures which exercise a significant control over the contrast lie in the range where the diffraction effect is of appreciable magnitude. Thus resolving power and contrast in the electron microscope are very closely related and competing phenomena. Moreover, in organic specimens in particular, the contrast produced by chemical differences in the structures of the specimen are usually so slight as to be unobservable in the image. Thus, the nonexistence of differentiation in the electronic image of many biological structures does not necessarily indicate a lack of structural differentiation in the specimen. The same problem has of course existed throughout the use of the light microscope, but for the most part it has been adequately solved by the use of selective staining techniques. At present only the most elementary and primitive selective staining



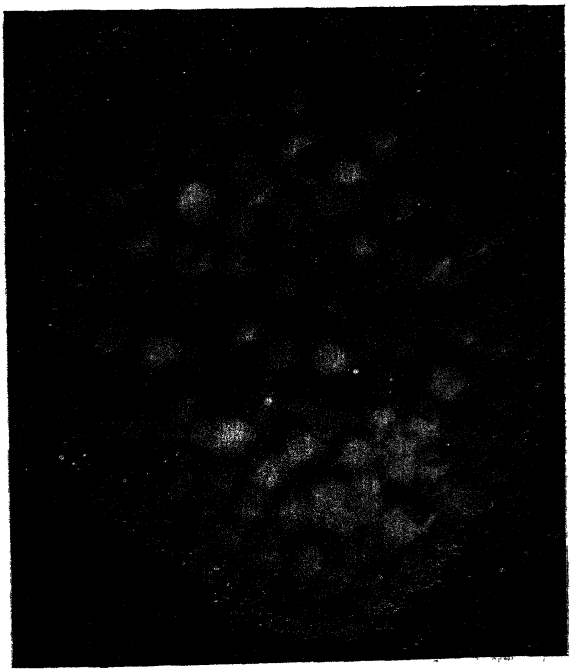
High-magnification electron micrograph of a partly crystallized evaporated film of antimony. (45,000×.)

methods have been tried with the electron microscope, though the possibility of their effective use definitely exists.

The third ambiguity that should be pointed out is for the most part due to the present method of operation of electron microscopes. The control of contrast by an appropriately sized aperture leads one into the range of aperture sizes, which are difficult to make and maintain and in general tend to limit the resolving power of the instrument. In the common use of the electron microscope the aperture sizes are considerably larger than those which exercise a significant control over the contrast. Contrast in that case is obtained by the differential action of the lens aberrations on the widely scattered rays and on localized phase-contrast phenomena.

The exact details of this effect are too complex to discuss here, but the results are of sufficient importance to be described. One of these is the production of a variation in the contrast and the resolving power from point to point of a single image. This means that in certain parts of an image the resolution and the contrast will be the maximum obtainable with the system, whereas in other parts of the *same* image the resolution and the contrast will have deteriorated to the point where they will not be much better than could be obtained with a light microscope.

It is not unusual for a light microscopist to attempt to push his interpretations beyond the resolution shown in the images. Since in most cases he has no alternative, there is a certain justification in his method; however, he can expect to receive the criticisms and objections of his contemporaries. In electron microscopy, on the other hand, the situation at present is more serious. The somewhat inexperienced electron microscopist can fall easy prey to the error of making interpretations in some parts of his image where the resolution may be as bad as 1,000–1,500 Angstrom units on the basis of a resolution of 50 Angstrom units demonstrated elsewhere in the *same* image. This error frequently finds its way into the permanent literature and is not picked up by the worker's potential critics, because very few of them are as yet completely aware of these possible difficulties of interpretation. No better example of this is to be found than in the literature concerning the use of the electron microscope in the field of bacteriology. As experience has been gained by the electron microscopists, and methods of preparing bacteria for the electron microscope have been improved, it has become a more or less accepted fact that the healthy vegetative bacteria cell is completely devoid of internal structure. These developments have convinced most bacteriologists that the structures seen by the early electron microscopists were artifacts of preparation. We are now convinced that this lack of differentiation in our ideally pre-



Chloroplast from a spinach leaf. Specimen provided by Dr. J. Turkevich, Princeton University. (Magnification, 10,000×.)

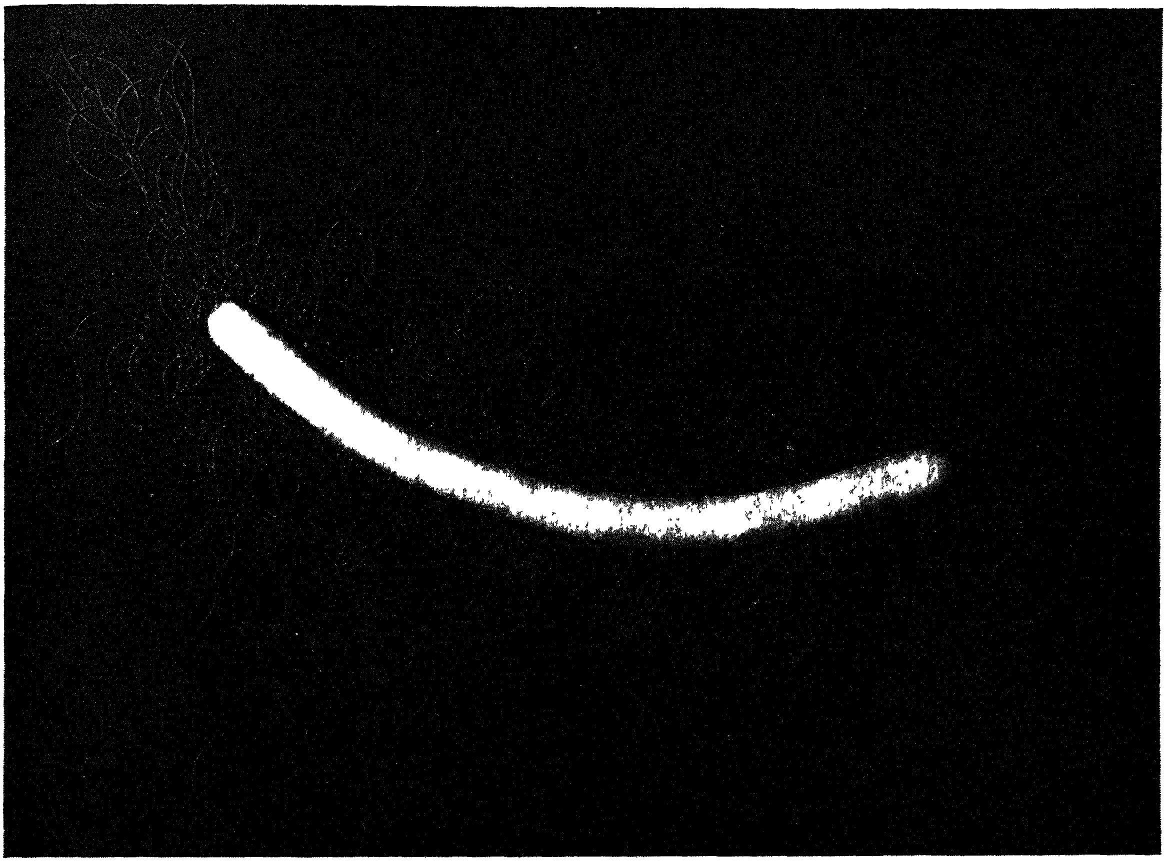
pared specimens is due entirely to lack of contrast in the electron microscope images. It is not to be inferred that we consider this an undesirable situation to be in. On the contrary, we are now in an excellent position to start experimenting with techniques designed to introduce contrast in specific ways and thus delineate in a controlled fashion the structure of the bacteria.

II

At this point we should discuss another aspect of electron microscopy. The specimens we prepare for examination on the electron microscope must satisfy a few rather stringent conditions. The conditions themselves do not present any particular problem as far as providing a specimen of which it is possible to get a good electron microscope image. There is in addition to the technical requirements, however, a more fundamental requirement that the final prepared specimen must provide either an accurate representation of the specimen as it was in its original state, or at least if distortions have occurred that they be *known* distortions.

The technical conditions are simply that the specimen must be sufficiently thin, dry, and also that it must be capable of withstanding a vacuum and a certain amount of electron bombardment. It it now known that the last two are invariably satisfied with the first two. These conditions introduce no particular problems for inorganic specimens. In biology, however, the problem of obtaining a specimen which is at once thin and dry and representative of the original specimen is indeed a difficult one. The actual difficulty lies not so much in satisfying these conditions simultaneously as it does in the fact that there are no independent criteria for deciding when they have been fulfilled. This means that both the investigation of structures and the determination of their validity must

Deeply etched surface of a rolled copper sheet. (21,000 ×.)



Swarming proteus, showing a large number of flagella. (Specimen prepared by Dr. C. F. Robinow.) (7,300 ×.)

be carried on with results obtained from one instrument. The method is straightforward enough but does require considerable work and some imagination on the part of the microscopist. It involves preparation of the specimen by as wide as possible a range of techniques and a careful study of the results produced by each, with a view to discovering those structures that are persistent in the techniques or to devising a structure that could act as a point of origin for all the variations observed. The ultimate desire is, of course, the development of a technique of specimen preparation which provides an accurate representation of the specimen and thus requires no multiple check.

The electron microscopist now has at his disposal a range of reliable techniques for every conceivable type of solid material. These include satisfactory sectioning techniques for tissues, growing techniques for the study of bacteria, replica techniques for the study of almost any type of surface, and dispersion techniques for the study of any type of particles or fibers. The accuracy of representation obtained in the electron microscope images has been satisfactorily checked in many cases, with the particular exception of the thin tissue sections. The technique for cutting such sections is so new that there has been, as yet, no opportunity to investigate the different fixing and embedding techniques or to determine the accuracy of the representation except insofar as it has been possible to compare results with those obtained with the light microscope. In these cases, at least, the correlation has been excellent.

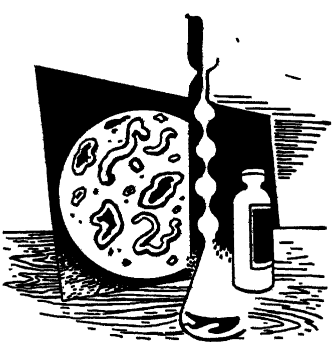
As was indicated in our initial analogy, the number of problems to which the electron microscope can be applied is almost beyond comprehension. This is particularly true in the biological sciences, where a complete understanding of any individual entity requires knowledge of the heterogeneities of the structure to the point where the physical chemist can take over. This amounts to almost a two hundredfold improvement in resolving power over that of the light microscope. But resolving power is defined on a linear scale, whereas all real structures are, of course, three-dimensional. In effect, this means that to give a complete description of biological entity requires obtaining 10^6 as much information as can be obtained at the limit of a

light microscope. This figure is somewhat frightening because it gives a clear-cut illustration of how little we know of these structures. At the same time, it should give encouragement to the graduate student looking for research problems. It should also provide him with a warning to choose from the many problems an example that is at least representative of a large general group of structures. It is only by such a judicious selection that he can hope to do research of lasting value. In my laboratory, for instance, two of us have made a total of 40,000 exposures in the past nine years. Taking as a rather conservative mean value for the magnification, 10,000 ×, we now realize that in this time we have photographed *only one square millimeter of our world.*

III

In the preceding, I have discussed the more general role of the electron microscope in scientific research. This discussion should not be closed without mentioning a rather specific use of the electron microscope which, although trivial compared to the broader considerations of the above, nevertheless constitutes at present a major part of its application in industry and research. I refer to the use of the instrument in the measurement of the particle size of pigments, catalysts, viruses, etc. It then becomes merely a means of expanding the scale of a ruler. Even in this application the electron microscope bears little resemblance to most scientific tools. For instance, to obtain a mean value for the particle size of a given material, it is necessary to compile the statistics from measurements made on individual particles. If the material has a very uniform particle size and shape, the results obtained with the electron microscope seldom do more than confirm those provided by indirect measurements. On the other hand, the electron microscope can provide results when the sample is heterogeneous with regard to particle size and shape, a case in which the indirect methods break down.

An attempt has been made in this article to describe the electron microscope primarily as an extension of our sense of vision, providing access to the world of heterogeneous structures represented by the first and succeeding levels of organization of molecules into solid structures. This world is a new one in that no other means for studying it has been available. It is an extensive world into which the scientific worker can proceed only with caution and with considerable deliberation as to the general value of his work.



THE CARIBBEAN ISLAND ECONOMY

CARLOS E. CHARDON

Dr. Chardon (Hon. Sc.D., Dartmouth, 1936) is director of the Instituto Agricola Nacional of the Dominican Republic. He was called to the post from Puerto Rico, where he was Commissioner of Agriculture, Chancellor of the University of Puerto Rico, Executive Director of the Land Authority, and Director of the Institute of Tropical Agriculture. He has conducted agricultural surveys for the governments of Colombia, the Dominican Republic, and Venezuela, and has recently been appointed general manager of a modern sugar mill being erected near Cuidad Trujillo.

THE economic problems of the Caribbean islands cannot be approached in their overall aspects—with reference to planning, operation, and actual solution of some of the present dilemmas—unless students of the area take into consideration the close geographical relationship between the islands. We must consider first that the Caribbean is a *geographical region*; and, second, that we are dealing also with a series of *political entities*. For more than three centuries the political patterns have prevailed over the geographic reality, which has brought about a disarticulated economy among islands forming part of an archipelago. In considering separately the political units and their problems, by far the majority of students have neglected the fact that in a sound economy the political units (without interference with their sovereignties or those of the metropolitan governments) may in many cases play an important economic role with reference to the economy of the others. So far, efforts have been isolated and fragmentary; the perspective of the whole has been ignored, and each country, or each colony or group of colonies, has fought its own battle, trying to solve its problems independently.[1] This reminds us of the man who failed to see the forest for the trees.

The above statements are self-evident: we know of half a dozen plans for the economic reconstruction of some of the political units of the area; the files of the Colonial Office in London are full of elaborate reports of West Indian Royal Commissions. Dozens, perhaps, of able and well-balanced brains and scores of pounds of stationery have been used with little or no practical result. To make it more forcible, until recently there has never been an economic concept which represents an integration of common interests and which would serve the mutual needs of the geographic group.

Recent reports, however, indicate that the problem of the economic integration of these islands is occupying the minds of a selected few. I myself expressed it very emphatically in my 1937 report to President Trujillo.[1] Erik Williams[2] recently wrote that trade in the Caribbean follows the flag and that each unit is geared toward some export market in which it enjoys tariff protection in return for certain obligations. Thus each unit forms part of some economic bloc. Further on, he says that interinsular relations are virtually nonexistent and that the Caribbean is, in fact, a geographical expression. It is really a collection of individual units, each functioning independently of the others. Colonel Standley, Secretary of State for the British Colonies, advocated "some organization which will enable them to find some solution of problems which are as common to Porto Rico or indeed to Cuba and Haiti as to Jamaica. If the Caribbean is to survive and prosper it must be fitted to the world as a whole."

I

A number of American scholars have pointed out that there has been an integrated Caribbean policy, at least on the part of the United States. Chester Lloyd Jones, for example,[3] has shown that even before the middle of the nineteenth century, a Caribbean policy began to develop in the United States (p. 6). Further on, after touching on the subject of the results of the Spanish-American War and the building of the Panama Canal, he says:

> Broader than the interest in Cuba or in the Canal, has been the Caribbean policy of the United States. The political phases of this policy have developed from the inability of the weaker states to maintain normal conditions within their own borders and in their relations to other nations. Its economic features . . . touch the problem of the safety of the United States.

The problem is, even admitting that there has been such a policy, that there has been no over-all planning. When Robert T. Hill, of the United States Geological Survey, presented for the first time to American readers a vivid description of

the West Indies,[4] he discussed rather thoroughly the economic and social problems of each island, but unfortunately missed the picture as a whole. This eminent geologist failed to recognize the Caribbean as a functional geographical area in the economic sense. This attitude of studying *pêle-mêle* is typical of the imperialistic ideas of his time. As a result of the war with Spain, the American Mediterranean had just been opened up for American capitalistic enterprise, and Hill candidly admitted that he belonged to a nation of "Yankee traders." "This compliment," he says, "although not so intended, classifies us among the mostly highly civilized nations, which are those which excel in commerce, and signalizes our need of foreign markets."

When this statement was made Hill perhaps did not realize that he was not speaking for the United States alone; the same practices had already been followed for three centuries by "good old Spain" and the "highly civilized nations" of Great Britain, France, and the Netherlands. Even pacific Denmark once had a small bite of the highly nutritious and well-seasoned West Indian pie. We must admit, however, that colonial policies have changed greatly since World War II. The problems facing the territories and colonial possessions of the sovereign nations are so complicated that a Caribbean Commission, which includes representatives of the United States, the United Kingdom, France, and the Netherlands, is now actively engaged in studying these problems and proposing practical solutions to them. It appears that in past years outside interests ate too much of the pie and left too little for the natives. The problem now, as we see it, is to restore an equitable portion of the whole menu to the natives and persuade the barons to be satisfied with a smaller share. In plain words, the work of the Caribbean Commission is to prescribe a reducing diet to the barons and by all means possible to improve that of the natives.

C. L. Jones, in commenting on the condition of the European colonies of the Caribbean, makes the following statement:

> All the island colonies, with the partial exception of the Bahamas, have in our day become black man's islands in which the population has increased to the point where the locally produced food supply is insufficient to meet the demand.
>
> Most of them [the colonial islands of Europe] have not had an economic development sufficient to allow collection of public revenues greater than are necessary for the maintenance of the minimum standards of order, communications and public health.

The Caribbean Commission is defective in that its members are dealing with less than one third of the geographical area of the islands. Thus the commission covers a limited portion of the area, and its efforts, even if successful, will suffer that limitation. With the results already achieved and the experience acquired by the Caribbean Commission, perhaps the establishment of a much larger commission comprising representatives from all the area may be advisable, but such a proposal naturally will have to be dealt with in the higher circles of diplomacy.

Most of the islands of the Caribbean area, because of their single-crop system of agriculture, are obliged to import large amounts of foodstuffs from distant countries, for they cannot themselves produce them in competition with a highly protected product such as sugar. To be sure, the economy of by far the vast majority of the islands is based traditionally on one export product: sugar. They are known as "sugar islands" because in many of them—from Cuba, the largest, to tiny Barbados—sugar is king. Sugar accounts for a large share of the cultivated acreage—four fifths in Antigua, three fifths in Cuba, two fifths in Puerto Rico. The sugar industry is the greatest single employer of labor in the Caribbean and dominates its external trade to the extent of nine tenths in Barbados, four fifths in Cuba, and nearly two thirds in Puerto Rico. It accounts for nearly nine tenths of the freight hauled by the public railway systems of Cuba and Puerto Rico.[2]

This situation is accentuated by the fact that the sugar industry is dependent upon the outside world not only for its market, but for its capital ownership, and control. Foreign ownership is implied in the term "absenteeism," and the physical remoteness of the owners adds to the distance between social classes which springs from race or color or nationality or culture. The proprietor tends to be outside of, not part of, the local or national community.[5]

With reference to these dangers, three economists who made careful studies in Puerto Rico have expressed the following judgment:

> It is often considered an evil for a country to be largely dependent economically upon the production of one commodity. Such a situation, usually increasing exposure to the vicissitudes of the so-called business cycle, is held to constitute a potential source of economic instability; for, with a country's eggs all in one basket, the danger of sudden dislocation and loss, both cyclical and non-cyclical, is likely to be greater than were its economic activities diversified. This evil is likely to be aggravated when the product in question is an export commodity, dependent upon external markets, and consequently one whose price is subject to the influences of a world market and world prices.[6]

Be this as it may, the sugar economy is so profoundly entrenched in the economic framework

of these islands that we do not foresee any substantial deviation from it in the future. Moreover, a rapid change might upset the present economy—and why should there be any change if with few exceptions the only steady policy of the capitalistic mother countries involves sugar?

During World War II, however, the dangers of the single-crop system were clearly demonstrated. Because of the scarcity of shipping facilities, there was a sudden decline in the imports of foodstuffs. At the close of the great conflict, the postwar economy, through tradition and necessity, had to be cast in prewar molds, and the much-talked-of diversification of crops was again put aside. (We cannot dismiss this subject without saying that during the peak of the submarine campaign, in 1943, the Anglo-American Commission (now the Caribbean Commission) was established. This newly created agency dedicated its efforts primarily to the supplying of foodstuffs for the territories and colonial possessions of the United States and the United Kingdom. A system of convoys was adopted and the situation was greatly relieved.)

II

In the midst of a great scarcity of food in the "sugar islands" the Dominican Republic mobilized a part of its resources as a food-producing nation and was able to supply a substantial part of the needs of Puerto Rico and other neighboring islands. The fact that this country had escaped the influence of sugar capitalism, and had dedicated a major portion of its efforts to the development of a diversified system of agriculture, suddenly placed it in a unique position as a food-producing nation in the Caribbean.

This performance points out potentialities for the future, precisely because within the Caribbean area the Dominican Republic is the *exception* to the rule. The country does not need to go into a multiple system of crop production; indeed, it already has a diversified agriculture—more so than any of the neighboring islands. For this reason, it is a country of far more food-export potentialities than any other area in the Caribbean region.

Before going into the export business, the country had first to do everything possible toward cutting down the imports of the main articles of food. Statistics show that the imports of such staples as rice, lard, vegetable oils, butter, cheese, prepared meats, and soup pastes (*pastas alimenticias*), which, during the decade 1920–29, amounted to $36,059,400, dropped to $10,008,500 in 1930–39, and to $447,171 during 1941–45.

Along with this significant reduction came an appreciable increase in the export of foodstuffs to the other islands of the Caribbean. From $3,440,124 in 1942 this figure rose to $9,335,932 in 1946. The other islands benefited through these food exports were, in the order of importance: Puerto Rico ($5,970,637); Dutch West Indies ($1,746,533); Cuba ($962,950); French West Indies ($550,766). The balance was distributed unevenly between the British West Indies and the Virgin Islands.

The great purchasing power of Puerto Rico is derived mainly from (*a*) the sale of agricultural products, plus the sale of the products of its increasing industry; and (*b*) from the considerable sums of money contributed by the Federal government, especially for the armed forces. Under the first item, the exports during the fiscal year 1946–47 were sugar, molasses, and rum, $95,567,000, or 53.5 percent of the total; tobacco, $9,125,000, or 9.1 percent; and fruits (fresh, canned, or preserved), $3,562,000, or 2.0 percent. This makes a total of 64.6 percent. In industrial products, needlework stands out with $39,268,000, or 22.0 percent, of the whole. For the Dominican Republic, the exports during the calendar year 1947 were as follows: sugar and molasses, $54,260,306, or 65.2 percent; cacao beans, $12,951,938, or 15 6 percent; coffee, raw or roasted, $5,126,412, or 6.2 percent; and tobacco, $4,561,435, or 5.5 percent. Thus the total of classical export products of Puerto Rico amounted in 1946–47 to $108,-254,000, whereas those of the Dominican Republic during the calendar year 1947 amounted to $76,900,000.*

But now appears a very important difference between the economy of both countries. The Dominican Republic, with a diversified agriculture, imports relatively small amounts of food and food products. In the calendar year, the figure was only $2,704,681; in Puerto Rico, during the fiscal year 1946–47, the figure was enormous, amounting to $95,481,494. Furthermore, the imports into the Dominican Republic of $2,704,681 are balanced by exports (mostly to other islands of the Caribbean) during the same year of $3,167,271 in edible animal and vegetable products. Thus the country is practically self-sufficient from the standpoint of food. No other country or colony in the whole Caribbean area has such a record.

There is still another very important item that should not be forgotten. During the fiscal year

* It is to be regretted that the official statistics of both countries are not strictly comparable, since those of Puerto Rico are based, following the usual procedure in the United States, on fiscal years ending June 30, whereas those of the Dominican Republic are based on calendar years ending December 31.

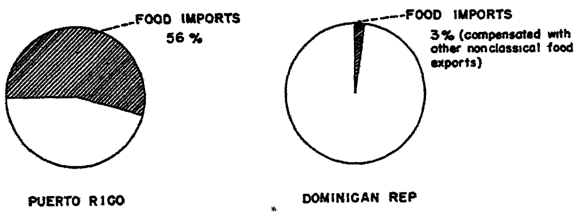


FIG. 1. Some comparative statistics.

1946–47, Puerto Rico imported $10,539,512 of unmanufactured wood, sawmill products, and wood manufactures. The Dominican Republic, during 1947, imported only $476,720, a figure that is more than balanced by an export of forest products amounting to $1,044,842.

Figures 1 and 2 show that for every dollar obtained from products exported from Puerto Rico, $0.56 has to be spent in purchasing food and food products abroad. In the Dominican Republic, for every dollar derived from export products, only $0.03 is spent on imported food, but this is more than balanced by the sale of nonclassical exports of food to the other countries of the area.

III

Self-sufficiency as an economic goal is not necessarily an index of a sound economic organization. It is generally looked upon by modern economists as a utopia. Among a group of islands like those of the Caribbean, however, the case must be viewed under a different light. If these islands have been for centuries exporters of classical food products such as sugar, tobacco, coffee, and cacao to the mother countries, and they have to depend on considerable imports of other foods to subsist, the problem of food supply may at any time be magnified by another emergency such as the last world war. The Dominican Republic is satisfied with its present economy, based mainly on crop diversification. The economics of the other islands are too tied up with the historical and economic policies of the past to expect a change in them; furthermore, such change may in many cases not be desirable. But the fact that, within the Caribbean area, there is a country that has deviated from the paths of the others, sharply differentiates it from its neighbors. If adequately developed, with some help from the Food and Agricultural Organization of the United Nations, the Dominican Republic may play an ever-increasing role as food producer and supply, at least in part, the needs of some of the other islands

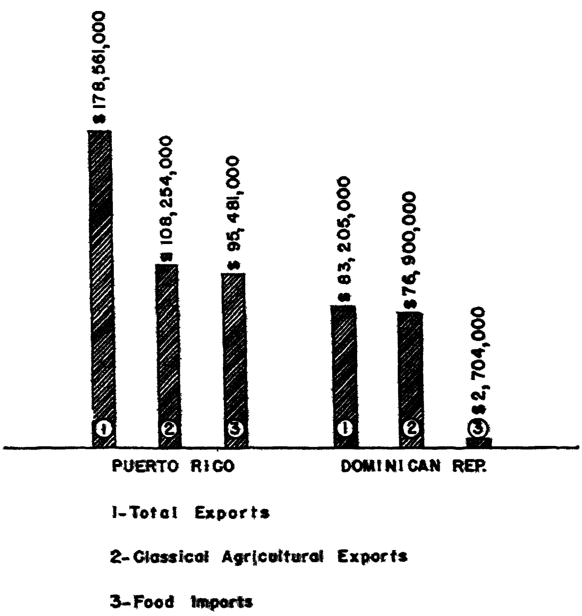


FIG. 2. Relation between export dollar and food imports.

What we have said about Puerto Rico in relation to the Dominican Republic applies to several other Caribbean islands where a sugar economy prevails. Underdeveloped countries are generally underfed countries. It is against the rules of sound economic collaboration to have underfed populations hampered by tariff barriers which preclude the purchase of food from near-by places that produce animal and vegetable products at relatively low costs. The Caribbean region, however, continues to be—with little hope of change—a political mosaic on the maps. There are seven nationalities represented within the archipelago. It is a political and economic rainbow which contrasts with the uniform blue ocean that surrounds it. We have paid dearly in the past for the lack of economic conscience among the economic leaders of the various countries and colonies which comprise the group. And the Caribbean Commission, as it is at present constituted, is destined to be by the very force of its limitations what may perhaps be called an "unfinished symphony" in the economics of the Caribbean.

REFERENCES

1. CHARDON, C. E. *Reconocimiento de los Recursos Naturales de la República Dominicana.* Univ. of Santo Domingo, 1937.
2. WILLIAMS, E. *The Economic Development of the Caribbean up to the Present in the Economic Future of the Caribbean.* Washington, D. C.: Howard University, 1944.
3. JONES, C. L. *The Caribbean since 1900.* New York: Prentice-Hall, 1936.
4. HILL, R. T. *Cuba and Porto Rico with other Islands of the West Indies.* New York: Century, 1898.
5. JENKS, L. H. *The Sugar Industry of the Caribbean in the Economic Future of the Caribbean.* Washington, D. C.: Howard University, 1944.
6. GAYER, A. D., HOMAN, P. T., and JAMES, E. K. *The Sugar Economy of Puerto Rico.* New York: Columbia University Press, 1938.

HIGH-SPEED AIRCRAFT AND AERODYNAMIC HEATING

VICTOR HALBMILLION

Victor Halbmillion was educated in Paris, France, studying theoretical engineering at the Ecole Centrale and law at Paris University. He then graduated in mechanical engineering from Columbia University, with a degree of Master of Science. Mr. Halbmillion is senior research associate in aerothermodynamics research at the Naval Ordnance Laboratory, White Oak, Maryland, and relaxes from his work in heat transfer and aerodynamics by indulging his love for winter sports, animal photography, and table tennis.

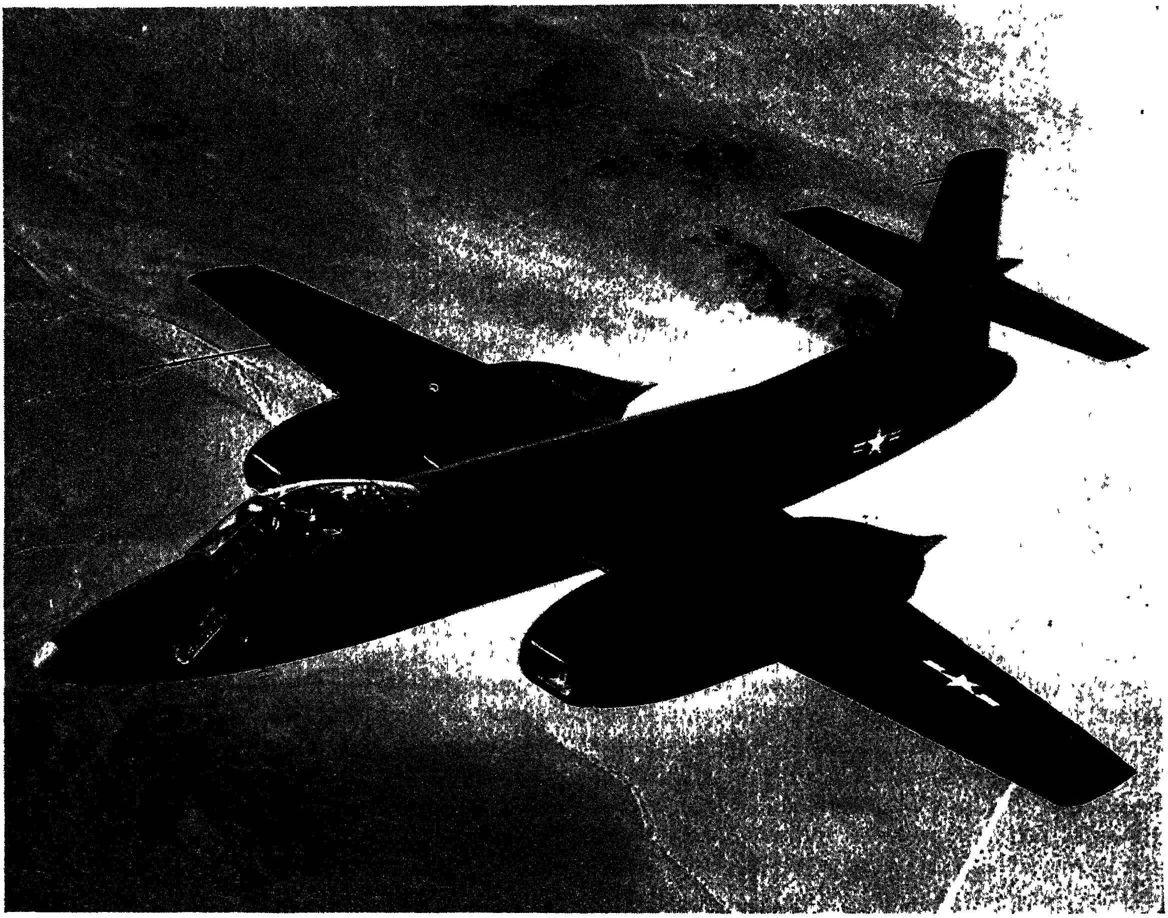
WHEN any object travels in the atmosphere at high speeds, its temperature increases owing to a phenomenon called aerodynamic heating. Until the last few years, this phenomenon was overlooked, mainly because at the speeds formerly reached by conventional aircraft it was still unimportant. With a steady increase in the speed of passenger-carrying airplanes —which have now invaded the supersonic range— and with the recent developments in the field of guided missiles—where speeds as high as several times the speed of sound are a commonplace occurrence—the problem of aerodynamic heating becomes of major importance and may prove to be the primary limiting factor to the speed of flight in the atmosphere.

The mechanism of aerodynamic heating is a combination of several complex effects. If we assume a sharp-nosed body moving in the atmosphere, the air in front of the body will be compressed and will suffer a considerable temperature rise. The temperature reached is called "stagnation" temperature, because relative to the body the air in that region remains practically still. The stagnation temperature can be fairly easily calculated by applying the classical formulae of gas dynamics, but the phenomena which take place along the side of the body are much more complicated.

If we consider a volume element of air at a very large distance from the body, it is obviously almost undisturbed by the passage of the body and has an absolute velocity zero (assuming no wind or other disturbance). On the other hand, a similar volume element in the immediate proximity of the body "sticks" to the body and is carried by it along its flight. Relative to the body, it will appear to have a velocity zero, and, as a first approximation, conditions apparently similar to stagnation are realized.

The air temperature in the immediate proximity of the skin is not, however, equal to the stagnation temperature, because of thermal effects in the boundary layer (which is simply the total volume of air around the body, at a given instant, which has been disturbed by the passage of the body). For practical purposes a volume element of air is considered disturbed if the motion of the body has impressed upon it a velocity in the direction of motion equal to at least 1 percent of the flight velocity of the body. Across the boundary layer the velocity of a volume element of air increases as a function of its distance to the wall of the body. The volume elements of air sliding in the boundary layer at different velocities produce shear stresses, and the work performed by these stresses increases the amount of heat produced. On the other hand, a certain amount of heat dissipation results from the fact that the temperature changes across the boundary layer, the peak temperature being reached in the immediate proximity of the body. Since there is always a heat flow between a point at a higher temperature and a neighboring point at a lower temperature, some of the heat produced in the inner layers flows across, and is finally transferred outside, the boundary layer.

The actual temperature resulting from the effects described above is called "boundary-layer temperature" (which is an abbreviation for the expression "boundary-layer temperature in the immediate proximity of the body") or "impressed-plate temperature." The relationship between the stagnation temperature and the boundary-layer temperature is mainly a function of a flow parameter called the Prandtl number, which is equal to the product of the specific heat of the fluid by its absolute viscosity divided by its thermal conductivity. When the Prandtl number is smaller than one, the amount of heat dissipated across the boundary layer is larger than the amount of excess



The Curtiss XF-87 in flight.

heat produced by the shearing stresses, the conditions being reversed for a Prandtl number larger than unity.

The Prandtl number depends to some extent on the temperature of the atmosphere, but mainly on its composition. For air, it is always smaller than unity, which is a favorable condition, and the boundary-layer temperature rise is usually only approximately 90 percent of the stagnation temperature rise (above the ambient temperature). A further favorable factor is that at extremely high stagnation temperatures, a certain amount of dissociation of the oxygen and nitrogen molecules takes place, which reduces to some extent the boundary-layer temperatures, but this is actually a very minor consideration, for the temperature-reducing effect of dissociation occurs only when the boundary layer is already very much above the practical allowable limits, as will be seen.

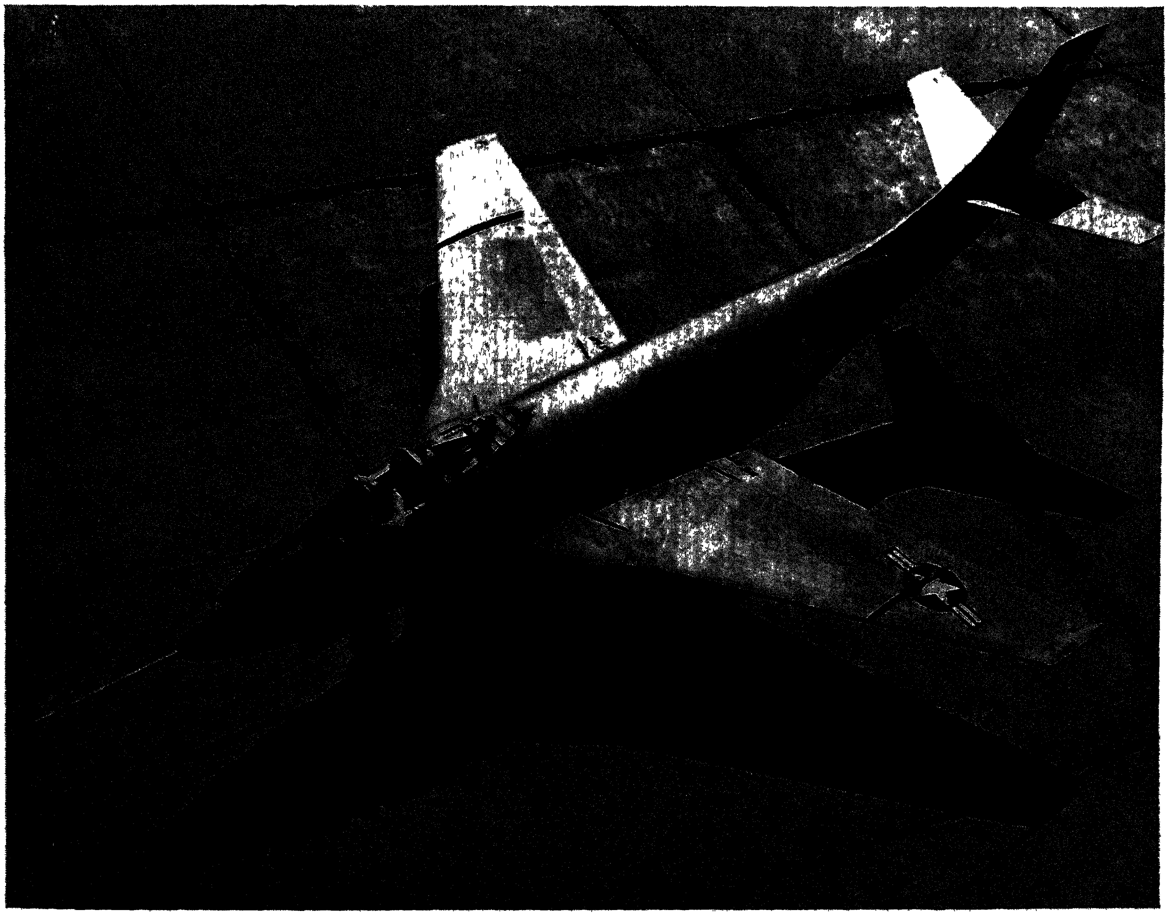
The boundary-layer temperature is a first approximation of what the temperature of the outer skin of the body would be should this temperature reach an equilibrium. The actual body temperature would be somewhat less, because some of the heat received by the body is reradiated to the outside (a relatively minor effect, however). Also, the body does not assume equilibrium conditions immediately, but passes through a transient period, the duration of which depends upon the heat-transfer coefficient between boundary layer and body skin, which is the amount of heat transferred per unit time and unit area of the body for each degree of temperature difference between boundary-layer and body temperature. This coefficient, usually denoted by the symbol h, increases with the speed of the body, but decreases when the density of the atmosphere, through which the flight takes place, decreases. The equilibrium temperature is therefore reached fastest during high-speed, low-level flight. At altitudes of the order of 100,000 feet and above, the reduction in air density is such that even at very high flight speeds the heat transfer into the body remains relatively very small.

In the discussion that follows, the equilibrium

conditions will be assumed to have been reached, and the heat transfer by radiation neglected. The body temperature rise is then approximately given by the boundary-layer temperature rise, which varies as the square of the speed of flight, and is shown in Figures 1, 2, and 3. The significance of such graphs is easy to interpret. Let us first assume a commercial-type, passenger-carrying airplane flying at altitudes between sea level and 35,000 feet. We shall assume the temperature of the atmosphere to vary according to the NACA tables for the Standard Atmosphere, the results of which are presented in Table 1.

By assuming that a passenger cabin temperature of 70° F. is required for comfort, the maximum boundary-layer temperature rise allowable, and therefore the maximum flight velocity attainable without artificial refrigeration of the cabin, may be calculated (Table 2).

It can be seen that the problem of aerodynamic heating is still of little concern to the designers of commercial planes. High-speed commercial flights will take place at relatively high altitudes, 20,000 feet and above, in order to take advantage of the reduced drag in a rarefied atmosphere, which would allow speeds as high as the speed of sound without any provision for refrigerating equipment. The same problem is already of importance to the designers of military fighters and interceptors, and planes equipped for attempting speed records. Speed-record flights are usually made at very low altitudes (only a few hundred feet) because of regulations set up by the Federation Aeronautique Internationale. They are attempted on hot days and in hot locations because, all other conditions being equal, the drag is lowest when the atmospheric temperature is highest. The allowable temperature rise for comfort is practically zero, whereas the boundary-layer temperature rise in present jet fighter records exceeds 60° F. Such conditions would of course be intolerable for sustained flight, but the duration of flight at record speeds is very short and the temperature in the cockpit has no time to reach equilibrium. Even



The McDonnell XF-88, a twin-jet fighter.

TABLE 1

Altitude, Feet	Temperature, °F.
5,000	41.2
10,000	23.4
15,000	5.5
20,000	-12.3
25,000	-30.1
30,000	-47.9
35,000	-65.7
Above 35,000 and up to 100,000	-67.0

TABLE 2

Altitude, Feet	Maximum Flight Speed, mph
5,000	430
10,000	540
15,000	640
20,000	705
25,000	795
30,000	865
35,000	925
Above 35,000 and up to 100,000	930

for flights of very short duration, however, the record pilot has to put up with a substantial amount of discomfort.

As airplane speeds are increased, a time will come when air conditioning by refrigeration will be essential, but the space, weight, and power requirements of such an air-conditioning system for the temperature control of a limited air volume represented by the cockpit and the passengers' cabin will probably offer no major difficulties in most cases. But as speeds increase still further, structural problems begin to appear. Present electrical and hydraulic equipment is not designed to operate at temperatures much above 180° F. At the same temperature, the strength of polysterene (which is one of the best heat-resisting plastics) is barely 50 percent of its strength at standard room temperature. A similar 50 percent strength loss occurs in commercial magnesium and aluminum alloys at temperatures of the order of 400° F. Even for parts that do not have to bear any stresses, the temperature limits for practical use are below 600° F. for magnesium and below 700° F. for aluminum.

This is serious because, whereas it is relatively easy to air-condition a passenger space properly, the problem of refrigerating a whole airplane, and particularly the outer skin, is considerably more difficult. With an atmospheric temperature of -67° F. around the airplane, the first thought that would

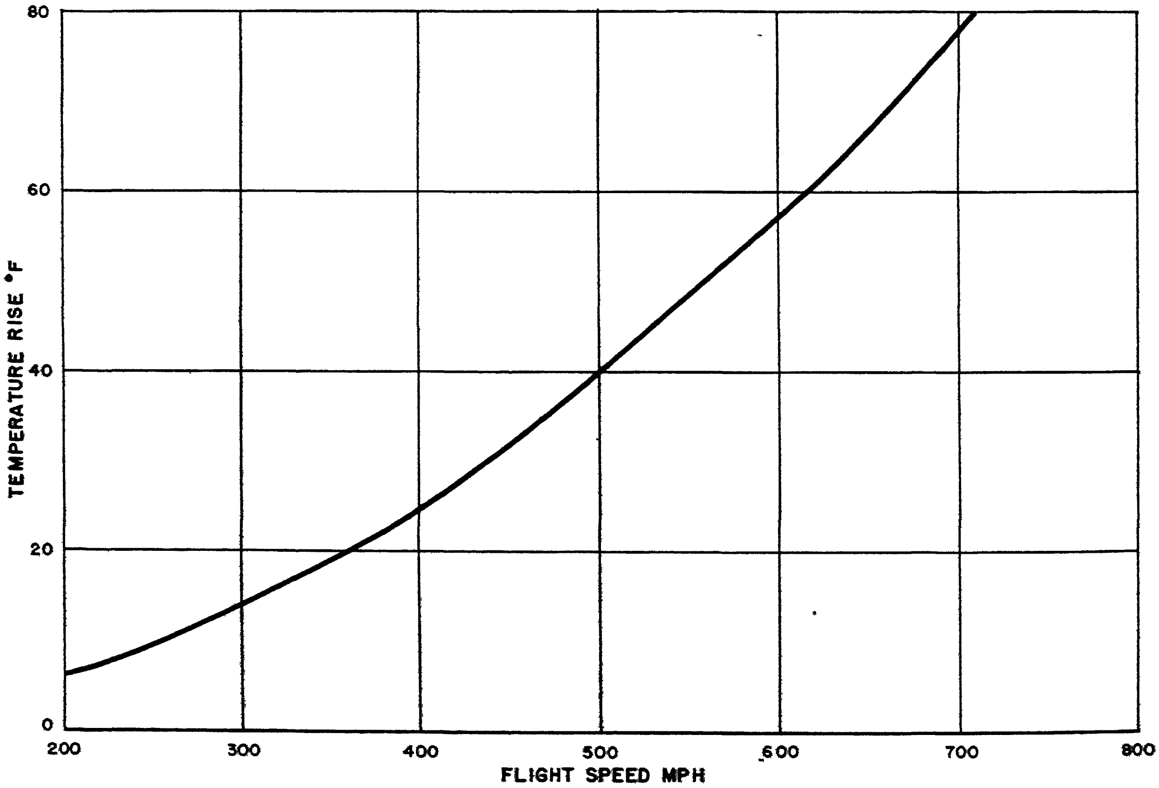


Fig. 1. Skin temperature increase for a typical aircraft.

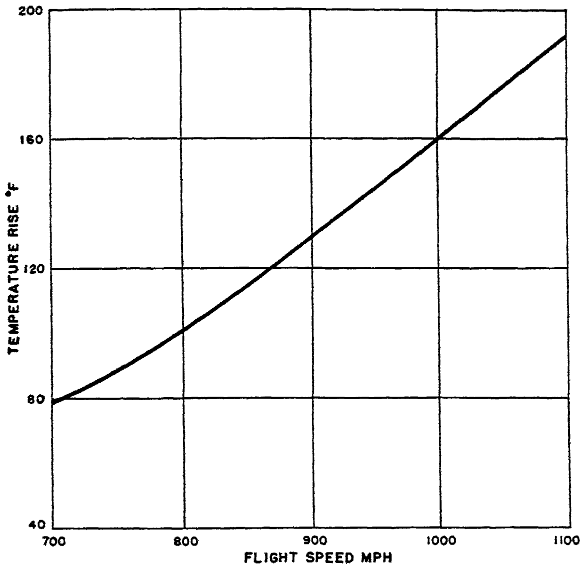


FIG. 2. Showing skin temperature increase for a supersonic aircraft.

come to the layman would be to try to suck in air from the outside to cool the structure from the inside. But, the cooling effect of air being proportional to its mass, because of the air rarefaction at high altitudes, the volume of air required for cooling would be extremely large, and the necessary pumps and pipes of tremendous size.

For flight in the supersonic region at speeds ranging between 1,000 and 2,000 mph in the coldest region of the atmosphere, which incidentally is reached at an altitude of the order of 35,000 feet, a substantial number of engineering problems would appear in specification changes and in the replacement of present standard materials by heavier or costlier, but better heat-resisting, materials. Above 2,500 mph, however, aerodynamic heating is so intense that new structural materials will have to be developed before long-duration flight under such conditions can be sustained. In the language of missile designers, speeds of 4,000 feet per second (2,728 mph) are conservative figures. Firing velocities of this order of magnitude have long been commonplace for naval artillery shells. For short-range missiles, the wall temperatures do not usually have the time to reach equilibrium conditions, and the point of failure of the material is avoided. Artificial skin cooling by evaporation may be taken advantage of, but its use is limited by the amount of liquid that may be evaporated. Considering only the thermal problem involved, increasing substantially the volume of the evaporating medium generally means increasing the over-all volume of the missile and therefore the amount of skin surface to be cooled. Protective refractory and insulating coatings may be considered, but an insulator can slow down the heat transfer only toward the inside, it does not stop it. The same equilibrium conditions will prevail as without an insulator, but the time duration to reach the equilibrium will be increased.

All the above seems to preclude the possibility of space travel, because the flight durations involved would be such as to allow the thermal equilibrium to be established. This, however, would be an incorrect conclusion. The results presented so far are valid only for atmospheric flight. At altitudes of the order of 400,000 feet, air can no longer be considered as a continuous medium, and the mechanism of aerodynamic heating begins to present an entirely different aspect. Heating is produced by bombardment of the moving body by individual molecules, which yield only a portion of their kinetic energy. Its evaluation is beyond the scope of this article, but it can be stated that mo-

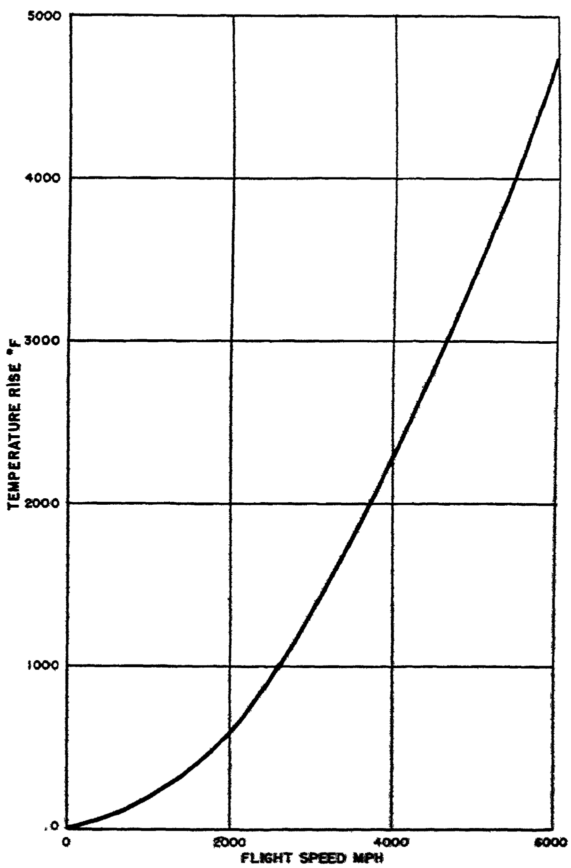
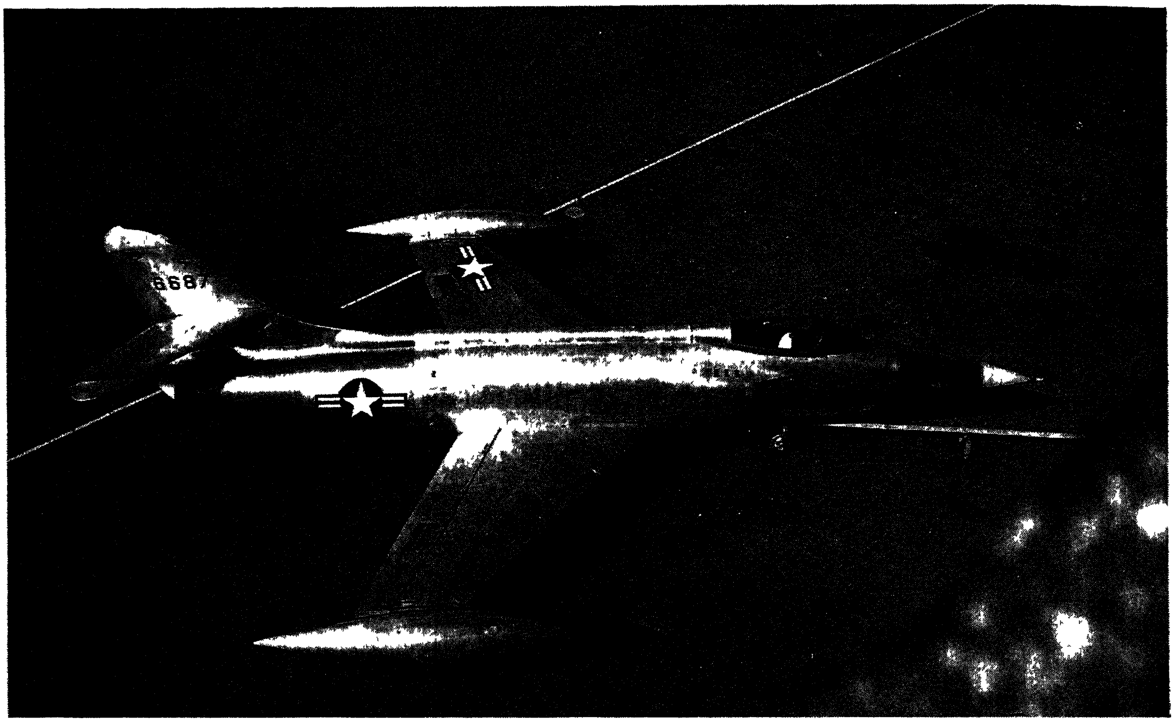


FIG. 3. Skin temperature increase for a high-speed guided missile.

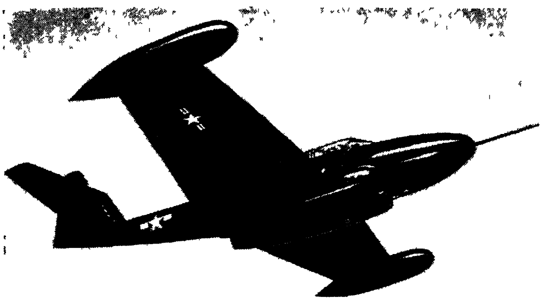


The Lockheed XF-90. A penetration fighter.

lecular heating at those extreme altitudes is very much smaller than the aerodynamic heating at similar speeds in the lower range of the atmosphere. As the altitude increases, the number of molecules per unit volume decreases; so does the number of collisions between the gas molecules and the moving body, and therefore the heating effect. The angle of attack—that is, the angle between the surface exposed to molecular heating and the direction of flight—is a very important parameter. To reduce heating, it is advantageous to have a very elongated shape, with the smallest possible angles of attack at all points; that is, a high "fineness ratio." To give a simple numerical illustration, at an altitude of 400,000 feet (or roughly 75 miles), the surface temperature of a thin flat-shaped body flying at zero angle of attack would actually be lower than the ambient air temperature prevailing at this altitude (which appears to be of the order of 200° F.) for all velocities up to 20,000 feet per second (or approximately 13,600 mph) if the heat radiation emitted by the body is taken into account. By simply shifting the position of the body so as to make an angle of 20 degrees with the flight path instead of being parallel to it, the body temperature would assume an almost immediate rise of the order of 600° F. at 13,600 mph.

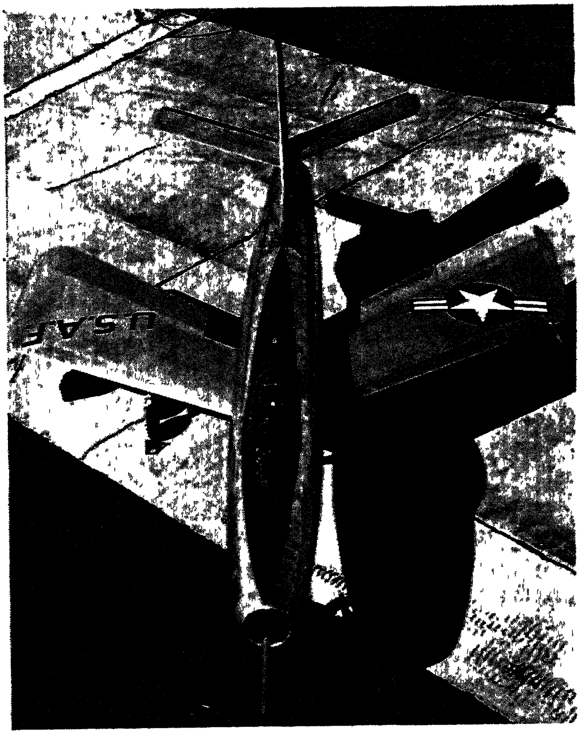
The heating by molecular bombardment decreases rapidly as the altitude increases further, and at an altitude of 800,000 feet, or roughly 150 miles, it becomes negligible, any heating due to or characteristic of the motion disappears for all practical purposes, and the surface temperature of the body is mainly a function of the intensity of the solar radiation. As it has been calculated that at 150 miles altitude the heating by solar radiation would raise the surface temperature of the body by approximately 500° F. (from a temperature of the order of −250° F.), its effect is of primary importance, but because of its static nature it is beyond the scope of the present discussion.

From all the above considerations, it appears that aerodynamic heating will not stand in the way of intercontinental or space travel. It will, however, impose certain conditions upon the trajectory, limiting the velocity as a function of the altitude, especially in the lower strata of the atmosphere. It may impose the use of refrigeration air conditioning, changes in material specifications, and require the development of new engineering materials. On the other hand, it may be a wonderfully cheap source of heat under conditions where heat may be required. Because the human body is sensitive to relatively very small differences in temperature, the effect of aerodynamic heating has



The Northrop XF-89. Night and all-weather jet fighter.

to be determined with great accuracy. It is a new field of research, open to both theoretical analysts and experimenters, which will probably grow rapidly as technological advancements in aircraft and guided missiles extend the ranges of flight speeds. To name a few of the research organizations involved, the National Advisory Committee for Aeronautics and the Naval Ordnance Laboratory have started an extensive research program to analyze the mechanism of, and collect experimental information on, the aerodynamic heating and the heat transfer between a body and a gas flow at high subsonic and supersonic speeds; and the University of California, under Navy contract, is investigating the same phenomena under conditions of highly evacuated flow. A thorough knowledge of the aerodynamic heating related to the trajectories under consideration would represent considerable progress along the path that will lead to the realization of the extremely high velocities of space travel in the not-too-distant future.



The Republic XF-91, an interceptor fighter. Equipped with a pilot-ejection seat, pressurized cabin, and refrigeration for the pilot at high speeds.

THE WATER ECONOMY OF DESERT MAMMALS *

BODIL and KNUT SCHMIDT-NIELSEN

Dr. Bodil Schmidt-Nielsen is a graduate of the School of Dentistry of the University of Copenhagen. During the war she was amanuensis and research associate at the School of Dentistry. She did research in microanalytical methods and saliva chemistry and also worked with her father, August Krogh. Dr. Knut Schmidt-Nielsen (Ph.D., Copenhagen, 1946) attended the University of Oslo in his native Norway before going to the University of Copenhagen for graduate work on ionic transport. From 1941 to 1947 he held Danish and Norwegian university fellowships. Dr. and Mrs. Schmidt-Nielsen are now assistant professor of histochemistry and research associate, respectively, at the University of Cincinnati.

THE most striking environmental factors in the desert are the heat and the scarcity of water. It is well known that man and many other mammals cannot survive for any length of time in a hot desert without large supplies of drinking water. Figure 1, taken from Adolph's recent book, *The Physiology of Man in the Desert,* illustrates this fact. The graph, which is somewhat simplified, gives the number of days a man will survive in North American deserts. The line running up along the coast gives the limit for a survival time of five days. There is a line for three days' and one for two days' survival; the little encircled area has a survival time of one day only.

In spite of the poor performance of man in the desert, the desert is inhabited by a large number of animals. The problem of how these animals get along without visible sources of water has excited interest for many years.

Dill, in his book *Life, Heat, and Altitude,* divides animals that can live under desert conditions into two groups. First are animals such as the camel, wild burrow, antelope, and several others that can endure the absence of water for a limited time. They must be able to go to sources of water periodically in order to restore their water content. These animals solve their water problem mainly by ability to withstand dehydration and to store water. In the second group are those animals that require no more water than that contained in their food. Some animals feed on succulent plants found in the desert, but certain other animals live on entirely dry food and must conserve water by every possible means.

Some rodents of the family Heteromyidae belong to the second category. The following study of these animals is based on field work in the summer of 1947 and in the late spring and early summer of 1948 at the Santa Rita Experimental Range, Arizona. This is right on the line of two days' survival for man, as indicated by an arrow on the map (Fig. 1). In this area a little pocket mouse (*Perognathus baileyi*), the bannertailed kangaroo rat (*Dipodomys spectabilis*), shown in Figure 2, and Merriam's kangaroo rat (*Dipodomys mer-*

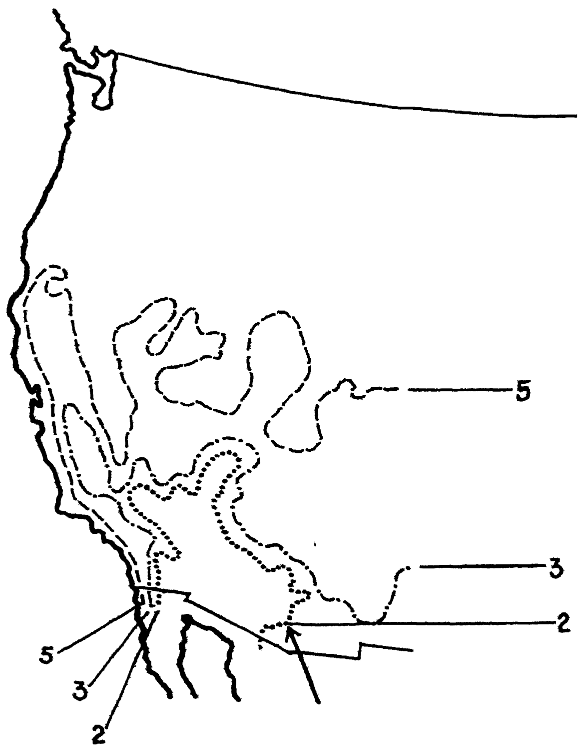


FIG. 1. The number of days man can survive without drinking water in American deserts (simplified from Adolph: *Physiology of Man in the Desert*). These areas are inhabited by numerous animals that live independently of drinking water.

* From a paper presented at a meeting of The Society of the Sigma Xi, Davis, California, April 7, 1949. The work was supported by Contract N7-ONR-380/T.O. 1 between the Office of Naval Research and Dr. L. Irving, Swarthmore College, Pennsylvania.

FIG. 2. Bannertailed kangaroo rat from the Santa Rita Range, Arizona, photographed in the laboratory.

riami) were abundant. These are small rodents, with fur-lined cheek pockets. The kangaroo rats look a little like kangaroos, with their elongated hind legs and short, weak forelegs; they also jump like kangaroos. They have large eyes and are nocturnal in their habits, which is of great importance in their water economy, as discussed later. They live in underground burrows.

The bannertailed kangaroo rats build large mounds, with many entrances and a complicated, labyrinthine net of runways. Their nest chamber is at the end of a blind tunnel about two feet under the surface. They keep large stores of food, mainly dry seeds, in their burrows.

The two other species occupy simpler dwellings, with only a few entrances and runways, and they store little or no food. Their nest chamber is about one foot underground. These animals are known to feed nearly exclusively on air-dried seeds and other dry plant material. They do not drink water because they have no access to it, and they do not seem to need it. They do not feed on cactus, as some other rodents do, and they seldom eat green leaves.

ABILITY TO LIVE WITHOUT WATER

In order to make sure that these rodents are able to live on the dry food in the dry desert climate, we kept a large number of kangaroo rats and pocket mice on a diet of dry rolled barley. At intervals of a few weeks, we sacrificed a number of them, weighed them, and determined body water content and serum concentration of urea and salts. The rats did not lose weight on the dry diet, except in the very beginning; after a period of eight weeks, nearly all of them had, in fact, gained weight. Some animals which we kept on the dry grain diet for two and a half months were in fine condition and had all gained weight at the end of the experiment.

The serum concentrations of urea and salt were normal in all the animals that had lived for different lengths of time on the dry diet, and the total body water content did not decrease. All this shows that the heteromyids are able to maintain a positive water balance under desert conditions when they live on a diet consisting exclusively of dry grain. This is a very unusual performance for a mammal, and it must involve specific physiological mechanisms.

If we set up an account for the water balance, we have:

Intake: Drinking water
Water content of food
Metabolic water

Output: Urine
Evaporation from lungs
Evaporation from skin
Feces

As mentioned before, the animals did not get any

drinking water, and the water content of the food was very low. The water content of grain at the humidity in the desert is about 5 percent. The metabolic water amounts to 0.6 g per gram carbohydrate combusted. The whole question of how much water the animals form by their metabolism and lose, simultaneously, by evaporation from their lungs, is very important, and we shall return to it later.

In order to get along on the little water it can derive from its food, the heteromyid will have to conserve water to a high degree. By concentrating the urine, the water loss can be decreased, but a limitation is imposed by the ability of the kidneys to concentrate the urine with respect to salts and urea. The higher the ability of the kidneys to concentrate the urine, the more water the animal will be able to save. We could, therefore, hope to find that the heteromyids are able to excrete a very concentrated urine. Analyses of the urine were, then, one of the important parts of the program when we set out to work on the water metabolism of desert animals.

Water is also lost by evaporation from the skin and the lungs. In some mammals, such as man and the horse, the evaporation through the skin is considerable. There are numerous sweat glands over most of the body surface, and these animals use a large amount of water in their heat regulation. Dogs and rats are supposed to have very few, or no, sweat glands, and the water loss from their skin is, consequently, much lower than in sweating animals. Dogs, on the other hand, regulate heat by hyperventilation and a resulting high evaporation from the respiratory tract.

The kangaroo rats and pocket mice apparently have practically no evaporation through the skin. They do not need water for heat regulation when they live their normal life, because they simply stay away from the heat. We noted in our desert laboratory that the animals died within a short time when the room temperature was around 37° C.

The evaporation from the lungs cannot be avoided. The air that comes into the alveoli will leave the lungs saturated with moisture slightly below body temperature. The amount of water lost from the lungs will depend on the ventilation of the lungs and the amount of moisture in the inspired air. There are two theoretical possibilities for decreasing the evaporation from the lungs: One is to breathe air with a relatively high moisture content, and another is to decrease the ventilation of the lungs. The heteromyids seem to utilize both these possibilities.

The water content of the feces is quite small, the feces of the heteromyid being very dry. From 100 grams of barley they produce about 5 grams of feces with less than 50 percent water content.

The two main sources of water loss from the heteromyids are, therefore, the urine and the evaporation from the lungs.

WATER EXPENDITURE FOR URINE FORMATION

The urine of the heteromyids is excreted in minute amounts. To analyze it, we used micromethods for the determination of total electrolyte content, urea concentration, and chloride concentration. All these determinations could be made on a total of 0.01 ml or less. We analyzed urine from heteromyids that had just been trapped and from animals that had lived for some time on the dry grain diet. The results immediately revealed that the heteromyids can concentrate their urine to an amazingly high degree.

The maximum urine concentration found in man is about 0.3 N, or 2 percent, of electrolytes, and 1 M, or 6 percent, of urea. The Norway rat has been supposed to be one of the mammals with the most efficient kidneys. It can excrete a urine that is 0.6 N, or 3.5 percent, with respect to electrolytes; and it can execrete urea in concentrations up to 2.5 M, or 15 percent. Maximum concentrations in the heteromyid urine, when the animals lived on the dry grain diet, were found to be 1.1 N for electrolytes and 3 8 M for urea (Table 1).

In order to get information regarding the physiological capacity in general, and the maximum performance of the renal execretory system in particular, we imposed extra loads of protein or salt on the animals by giving them diets containing an excessive amount of protein or sodium chloride, respectively. For a high-protein diet we used dry soy beans; for a high salt diet, barley soaked in a salt solution and dried. The grain thus treated contained 10 percent of sodium chloride by weight.

The animals could not survive on these diets; they lost weight and died within three weeks. But the urine they excreted was highly concentrated. We found values for urea concentration up to

TABLE 1

MAXIMUM CONCENTRATIONS OF ELECTROLYTES AND UREA IN URINE

	Electrolytes	Urea
Man	0.37 N (2.2%)	1.0 M (6%)
Norway rat	0.60 N (3.5%)	2.5 M (15%)
Heteromyids	1.2 N (7%)	3.8 M (23%)

3.8 M (23 percent). Electrolytes were also excreted in very high concentrations, up to 1.2 N. This is twice as concentrated as sea water and is nearly twice the maximum excretory ability known for other mammals. Furthermore, we found very high electrolyte concentrations simultaneously with very high urea concentrations. Making a rough estimate of the osmotic value of such a urine sample, we find that it is just about six osmolar, an amazing value compared with what is to be found in other mammals.

It is generally believed that mammals like man and the rat can only increase the urea concentration at the expense of salt concentration, and vice versa. It is difficult to understand how the kidneys of the heteromyids can perform such enormous osmotic work, but it is obvious that they have a very useful mechanism for water conservation. A heteromyid needs only half the amount of water a white rat needs for excreting its waste products.

With such high urine concentrations, heteromyids should actually be able to gain water when drinking sea water. In order to show that they really are able to do so, a special experiment was performed. A number of kangaroo rats were divided into three groups and given the following diets:

Group 1, dry soy beans
Group 2, dry soy beans and sea water
Group 3, dry soy beans and fresh water

The animals in the first group lost weight and died within three weeks. The animals in the second and third groups lost weight during the first two or three days, until they learned to drink. Then they all gained and increased to above their initial weights.

WATER LOSS THROUGH EVAPORATION

In order to determine the order of magnitude of the evaporation from the lungs, measurements of relative humidity and temperature in and around the burrows of the animals were necessary. Vorhies, during a whole year, recorded the temperature inside and outside the burrow of a *Dipodomys spectabilis*. He also recorded the humidity outside the burrow, but he did not have any suitable hygrometer with which he could record the humidity inside the burrow. For this purpose we used Krogh's microclimate recorders, which record temperature and relative humidity for a twelve-hour period. The record is made on a smoked circular glass disc, which can be read afterwards under the microscope. The humidity-sensitive element in the recorder is a hair hygrometer. It has, therefore, the same limitation as other hair hygrometers, in that it will not record humidities much below 20 percent with any accuracy. In the beginning, we did not believe that this recorder would be very useful in our work, because we expected relative humidities to be lower than 20 percent inside the burrows. But a brief survey showed that the humidity in the ground and in the burrows ranged much higher than we had anticipated, so the recorders turned out to be extremely useful.

In order to get the recorder down into the burrow, we tied it to the rat's tail, and released him in front of his own burrow. He would then drag the recorder down into the nest chamber. To prevent the animal from running away with the recorder, we had a thin wire tied to the instrument and fastened outside After twelve hours we excavated the burrow. Table 2 summarizes the results. We found some variation from burrow to burrow, but the humidity was always considerably higher inside the burrows than outside. The humidity outside the burrows was sometimes extremely low—humidities below 5 percent relative humidity were frequently measured. We see, therefore, that it is most advantageous for the animals to stay in their burrows, instead of running about in the dry desert.

TABLE 2

APPROXIMATE HUMIDITY OF *Dipodomys* BURROWS AS COMPARED WITH THE OUTSIDE AIR. EARLY SUMMER IN ARIZONA DESERTS

	Temperature, Degrees C	Relative Humidity, Percent	Absolute Humidity, mg H_2O/ Liter Air
Inside burrows	25–30	30–50	8–15
Outside burrows, day	20–45	(1)–15	1–5
Outside burrows, night	15–25	15–40	2–5

We can calculate how much water the animals save by staying in the burrows. We can assume that *Dipodomys*, like other mammals, expires air that is saturated with moisture slightly below body temperature. Normal body temperature for *Dipodomys merriami* is 36°–37° C, and the following calculations were made under the assumption that the expired air has a moisture content corresponding to saturation at 33° C (Dill, p. 26). Air saturated with water vapor at 33° C contains 35.3 mg of water per liter air. A *Dipodomys* that is breathing completely dry air will, therefore,

lose about 35 mg of water per liter of air expired. If the animal breathes desert air with a moisture content, as we measured it during the daytime, of about 2 mg water per liter air, it will lose approximately 33 mg of water per liter air expired. If, however, the animal stays in its burrow, the air it inspires will contain about 10 mg of water per liter, and the animal will lose only about 25 mg of water per liter expired air, or 24 percent less than in the first case.

It is obvious that a high moisture content of the inspired air results in a lower evaporative loss from the lungs, and that the absolute humidity and not the relative, is the determining factor. In this way a relatively dry and warm burrow may be more advantageous than a cold and relatively humid burrow, because the latter may have a lower absolute humidity.

In the water economy of the heteromyids, this saving is very important, because it determines whether the production of metabolic water will lead to an ultimate loss or gain. A common view is that metabolic water is of course useful, but not a very decisive factor, since the metabolism, in addition to formation of water, involves respiration, i.e., evaporation from the lungs.

Howell and Gersh (1935) are of the opinion that the animal will always lose more water by evaporation from the lungs than will be formed by the metabolism, so that an increased metabolism in order to form more water is too expensive to be practicable. We cannot quite agree with this viewpoint. The evaporation from the lungs is influenced by the humidity of the inspired air and the ventilation of the lungs, and whether the formation of metabolic water will lead to an ultimate loss or gain can easily be calculated.

The amount of water formed per gram of foodstuff combusted is shown in Table 3. In the same table is also shown the amount of oxygen used per gram foodstuff combusted; and, finally, the amount of water formed per ml of oxygen used is calculated.

TABLE 3

AMOUNTS OF WATER FORMED AND OXYGEN USED WHEN PURE FOODSTUFFS ARE METABOLIZED

	Carbo-hydrate	Fat	Protein
Grams of water formed per gram foodstuff combusted	0.556	1.071	0.396
Liters oxygen used per gram foodstuff combusted	0.80	2.01	0.95
Mg water formed per ml of oxygen used	0.69	0.53	0.42

Using the figures for the composition of grain from a nutritional chart, we can calculate the amount of water formed per ml of oxygen used when grain is metabolized (Table 4).

Next, the figures for the amount of water formed by the metabolism are compared with the figures for the amount of water lost by evaporation from the lungs. With 16 percent oxygen in the expired air (normal value for man and most other mammals), 50 ml oxygen are taken up per liter air expired. When corrected for temperature, vapor pressure, and barometric pressure, this gives 42.4 ml O_2. If the animal breathes completely dry air, it will lose 35.3 mg of water per 42.4 ml oxygen taken up, or 0.83 mg of water per ml oxygen. This figure is larger than the figures shown in Table 4 for the amount of water formed per ml oxygen used when grain is metabolized. This means that an animal breathing dry air will lose more water by evaporation from its lungs than is formed by its metabolism. In other words, for the conditions stated, the formation of metabolic water will lead to an ultimate loss of water.

TABLE 4

AMOUNT OF METABOLIC WATER FORMED FROM THE COMMON TYPES OF GRAIN, CALCULATED IN RELATION TO THE AMOUNT OF OXYGEN REQUIRED FOR THE COMBUSTION OF THE FOOD IN THE BODY

	mg H_2O/ml O_2
Barley	0.66
Oats	0.63
Wheat	0.62

If the animal breathes air containing 10 mg of water per liter (as is found in the burrows), the loss of water from the lungs will be 0.60 mg per ml oxygen. In this case, water is gained by the metabolism and an increase in the metabolism will lead to an increase in the water gained.

In the dry season, the moisture content of the air outside the burrows during the night was about 2–5 mg of water per liter air. In that air the water loss from the lungs will be about 0.7 mg per ml oxygen. The amount of water gained about equals the amount of water lost. As the animal spends less time outside the burrow than inside, the over-all picture will show a gain in water by the metabolism.

This calculation, however, was built upon the assumption that the ventilation of the lungs is the same as in man and that the expiratory air is

saturated with water. In order to find out whether the evaporation from the lungs can be decreased below this level, we measured the evaporation from the animals simultaneously with the oxygen uptake. We found that the kangaroo rats evaporate less water per ml of oxygen used than the ordinary white rats. Twelve heteromyids evaporated on the average 0.53 mg of water per ml oxygen taken up, whereas 10 white rats evaporated on the average 0.93 mg water per ml oxygen. If we compare these figures with the theoretical figure for the water loss from the lungs when completely dry air is inspired (0.83 mg H_2O per ml O_2), we see that the kangaroo rats show considerably lower values. In other words, the kangaroo rats are able to reduce the evaporative water loss from the lungs, probably by decreasing the ventilation of the lungs.

Summarizing the results of our studies, we can say that the desert rats, as Dill predicted, really do save water by every possible means. They spend no water for heat regulation. They excrete a highly concentrated urine. They have a lower evaporation of water than white rats. They further decrease the evaporation from the lungs by staying in their more humid underground burrows.

REFERENCES

ADOLPH. E. F., and associates. *Physiology of Man in the Desert.* New York: Interscience Publishers, 1947.

DILL, D. B. *Life, Heat, and Altitude.* Cambridge: Harvard Univ. Press, 1938.

VORHIES, C T. Water Requirements of Desert Animals in the Southwest. *Univ. Ariz. Techn. Bull. No. 107,* 1945, 487–525.

KROGH, A. A Micro-Climate Recorder. *Ecology,* 1940, **21,** 275–78.

HOWELL, A. B., and GERSH, I. Conservation of Water by the Rodent *Dipodomys J. Mammal.,* 1935, **16,** 1–9.

INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON

Judges in the THIRD ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON, which is sponsored by THE SCIENTIFIC MONTHLY in cooperation with the Smithsonian Institution, will be: Dr. Merle A. Tuve, of the Carnegie Institution, for the physical sciences; Dr. Ronald Bamford, of the Department of Botany, University of Maryland, for the biological sciences; A. A. Teeter, recently of Charles Pfizer & Co., New York City, for chemistry; Dr. Emanuel Krimsky, of Polyclinic Hospital, New York City, for the medical sciences; and Alexander J. Wedderburn, of the Graphic Arts Division, Smithsonian Institution, for photography.

Entries in the competition, which was established to encourage and extend the use of photography as a basic research tool, will be received by the Editor of THE SCIENTIFIC MONTHLY, 1515 Massachusetts Ave., N. W., Washington, D. C., from August 24 to September 14, 1949. Winners will be announced on or before October 1.

First, second, and third awards and five honorable mentions will be given in each of two divisions, black-and-white and color. The Judging Committee will consider the initiative, originality, and results obtained more than the composition and pictorial quality. All photographs must be taken for scientific purposes.

The prints selected for awards and display will be shown at the U. S. National Museum during October 1949, and at the New York Meeting of the AAAS, December 26–31, 1949. They will then go on a tour of important scientific institutions in this country and abroad.

THE FATE OF MORGAN'S BEAVER

RICHARD H. MANVILLE

Dr. Manville (Ph.D., Michigan, 1947) is assistant professor of zoology at Michigan State College and well acquainted with the area of which he writes.

FROM the time of Hearne's first account[1] in 1795 until the recent article by Gregg[2] in THE SCIENTIFIC MONTHLY, the beaver has had his full share of publicity in the literature of North American mammals. The largest of our native rodents, with many specializations for his peculiar type of aquatic existence, he is renowned alike for his works as a forester and landscape architect, for his engineering ability, and for his social organization. Indeed, an almost human degree of intelligence has frequently been imputed to him. His pelt lured the early trappers westward and was largely instrumental in opening up the continent. The beaver has figured prominently in folklore, legends, and children's tales; he is the subject of volumes by Dugmore,[3] Johnson,[4] Martin,[5] Mills,[6] and Warren,[7] and of innumerable shorter articles. Amidst this mass of fact and fiction, perhaps no more penetrating study of the beaver has been made than that by Morgan;[8] it is to this day a classic in its field.

The discovery of iron in the Marquette range of Michigan's Upper Peninsula, in 1844, was a tremendous boon to this backwoods country. The first iron ore was mined near Negaunee in 1846; the following year the first company for mining iron was organized. The Carp River forge was

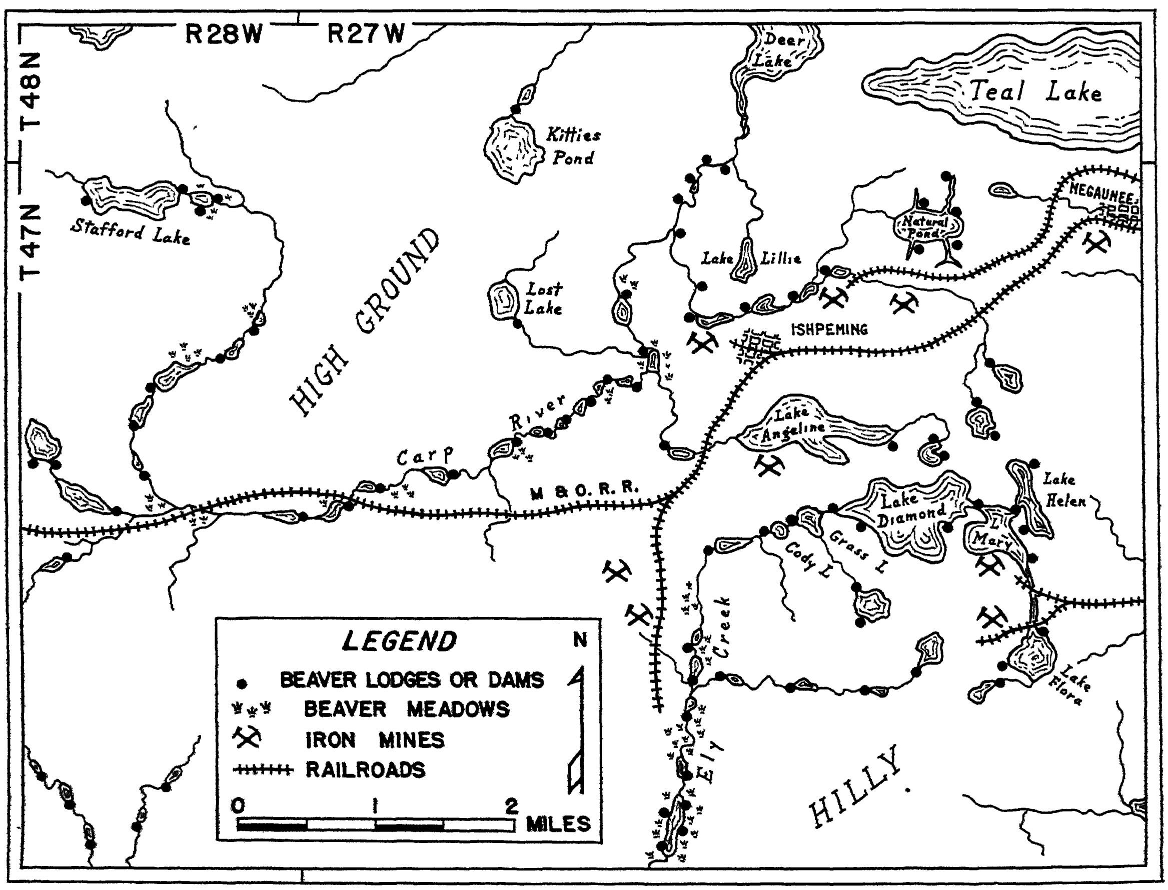


FIG. 1. The area of Morgan's observations in Marquette County, Michigan, about 1867. Modified from Morgan's map.

erected in 1847 and produced the first iron early in 1848. As a director of the recently developed Marquette and Ontonagon Railroad, Lewis H Morgan had occasion to visit the country while it was yet a wilderness. Attracted at first by the angling possibilities, then intrigued by his casual observations of beaver workings, he soon developed a deep interest in the animal—so much so that he devoted most of the summers from 1855 to 1867 to its study in this area. He was a keen observer, a careful recorder of what he saw, and he presented a picture of beaver life in a lucid, distinctive style. So clear were his maps and descriptions, in fact, that in some cases it is possible, even now, to locate the exact spots where his observations were made, as I did in August 1948. The present remarks are chiefly of historical value and may impress upon us the importance of man as an ecologic factor in the community where he dwells. An appraisal of the situation as regards the beaver, some ninety years after Morgan's observations, may be of general interest.

Morgan's studies were concentrated largely in an area of some forty-eight square miles (Fig. 1). Nine iron mines had been sunk within the area; the railroad had but recently pushed across the country, reaching Ishpeming in 1858; and the only settlements established in the wilderness were the hamlets of Ishpeming and Negaunee. Scattered lakes and ponds and many small streams, draining principally into the Carp and Escanaba rivers, made an ideal situation for the existence of beavers. The lands were heavily forested, with a plentiful supply of aspen, the beavers' favorite food, adjacent to the waters. The courses of Carp River and Ely Creek were studded with beaver meadows, the scenes of earlier beaver ponds that had filled with sediment and grown up to grassland. Throughout this region Morgan mapped the beaver dams, lodges, and meadows and located at least eighty sites of beaver activity.

The picture today is a vastly different one (Fig. 2). On careful scrutiny the country may be recognized as the same, despite changes in the names of many of the lakes and ponds and corrections of some of the minor inaccuracies in Morgan's map.

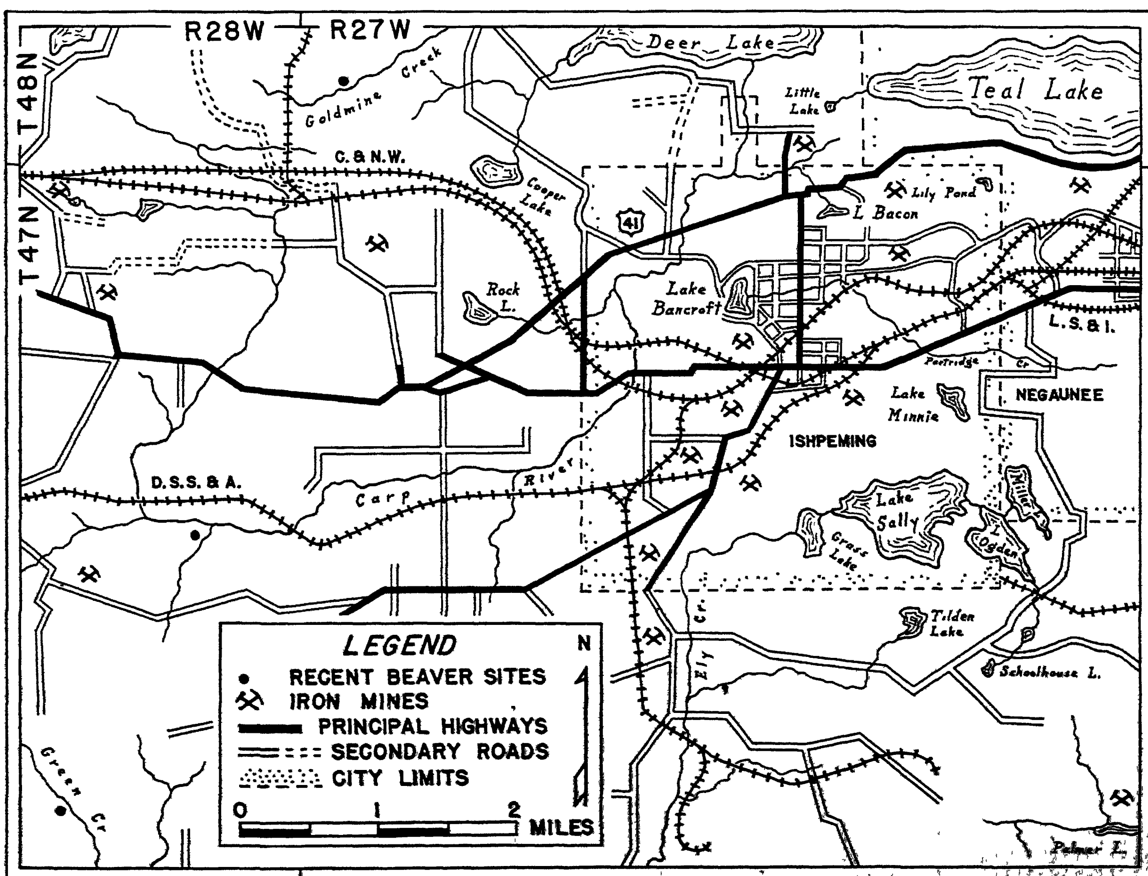


FIG. 2. The same area as shown in Figure 1, in 1948.

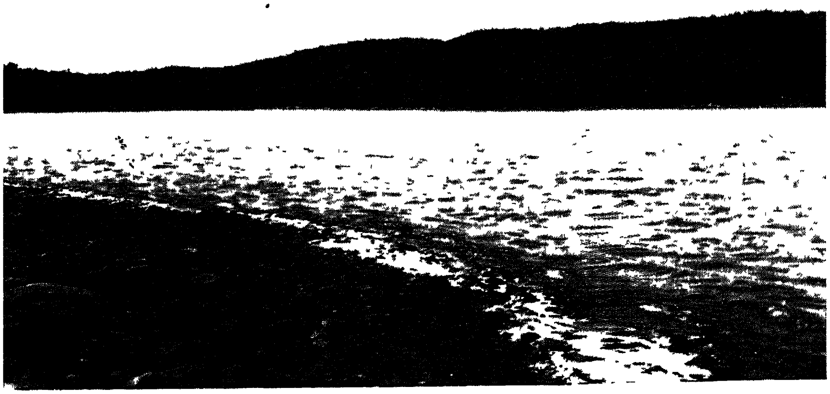


Fig. 3. The western end of Teal Lake, 1948, approximating the primeval conditions.

Fig. 4. The eastern end of Teal Lake, 1948, showing mining structures of the Cleveland-Cliffs Iron Company.

Fig. 5 The Carp River, 1948, as seen from highway U.S. 41 east of Teal Lake.

Fig. 6. A portion of the beaver dam at Grass Lake in August 1861, as illustrated by Morgan. (Used by permission of J. B. Lippincott Company.)

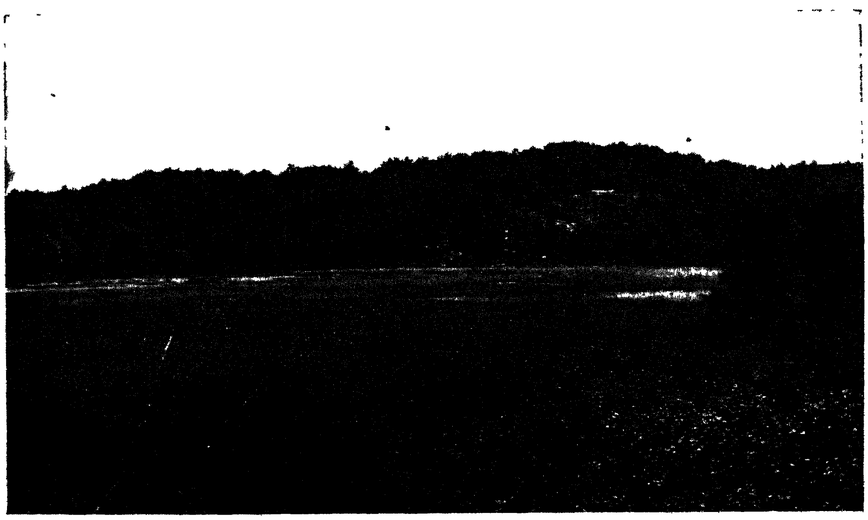


Fig. 7. The same site at Grass Lake in August 1948.

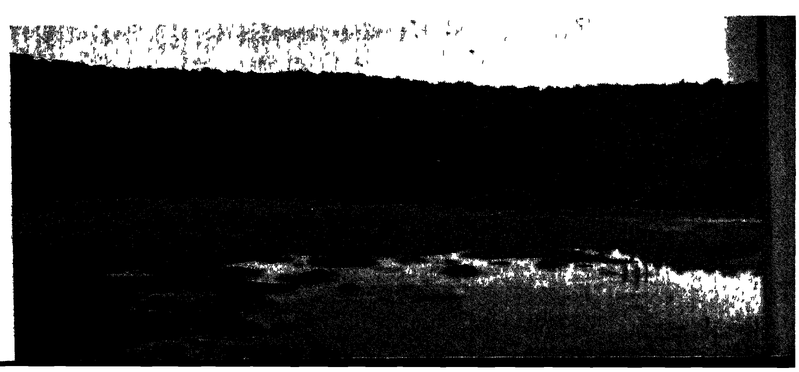


Fig. 8. A view across Grass Lake in 1948 from the bluff shown in Figure 7.

The beaver meadow succession has continued, to the point of forests in many cases, so that most of the ponds mapped by Morgan are no longer in existence. Others—for example, Stafford Lake, Lake Flora, and Kitties Pond—are much reduced in size. The configuration of Deer Lake has been greatly altered by impounding; Lake Angeline has been drained as a consequence of mining operations. Ishpeming and Negaunee (populations 9,491 and 6,813, respectively) have spread over much of the country. Three railroads, a Federal trunk highway, and a network of lesser roads penetrate to most parts of the region. Some of the mines are no longer in production, some are represented by huge areas of caved-in ground—but many are operating with renewed vigor. Forests have been cut, waters polluted, and the beavers themselves persecuted by trappers. The wonder is that, with conditions thus altered, the beavers persist at all in the more remote parts of this country where Morgan once found them so plentiful.

Teal Lake, although not studied in detail by Morgan, typifies the changes that have occurred. Today the western end of this beautiful lake (Fig. 3) is relatively unmodified, and deep forests still fringe its shores. A main highway, however, skirts its southern shore, and at the eastern end (Fig. 4) a settlement has grown up and mining structures rise above the horizon. Beavers have disappeared from Teal Lake.

Along the Carp River and its tributaries Morgan noted no less than forty sites of beavers' activity. In streams as large as this, bank burrows were frequently constructed in addition to the more orthodox lodges of sticks and soil. Dams, which were often built across small streams, were impractical here because of the threat of spring freshets and flash floods. Many canals were dug by the beavers along the Carp River, often at bends in the stream, to lessen the task of hauling branches for food and structural work. But beaver activity along the Carp River, once so prevalent, is now restricted to a few small tributaries at its headwaters. Elsewhere, although the forest cover is in many places undisturbed (Fig. 5), the river is polluted by wastes from the mines, and its waters are red with iron sediments.

Morgan's most detailed observations were made in the area about Lake Diamond (now called Lake Sally), which has been raised some ten feet by a concrete dam and serves as a water supply for the city of Ishpeming. No longer do beavers occupy this lake or its outlet, Ely Creek, where Morgan found them so numerous. Not far below Lake Diamond was Grass Lake, a pond of about sixty acres, great beaver dam, described by Morgan (pp. 86, 99) as "the most remarkable of all the structures . . . not surpassed in magnitude by any other beaver dam in North America," measured more than 260 feet in length along its crest and over 6 feet in vertical height at the center of its great curve. Morgan estimated that it contained upwards of 7,000 cubic feet of solid materials. At considerable pains he and his party photographed this dam in August 1861; from the photograph, the engraving (Fig. 6) published in his book as Plate VII was made. Beavers are absent today from this locality and the great dam has completely disintegrated. The marsh succession, evident even when Morgan viewed the scene, has progressed far. The changed nature of the area is shown in Figure 7, a present view from practically the same site as Morgan's illustration. The contour of the hill and the clump of trees at the right are recognizable features. The former location of the dam is marked only by the junction of the terrestrial willows and alders with the lower-growing, more aquatic, vegetation of leatherleaf, bog rosemary, and pale laurel. An elevated view from the opposite side of the pond (Fig. 8) shows clearly how much of Grass Lake is now filled with emergent aquatic vegetation.

Morgan discussed at some length the manner in which beavers fell trees and section them into logs. Usually this is done in a stereotyped fashion, but occasionally unusual types of cutting are observed. One of these, a stump with two deep incisions

FIG. 9. An unusual beaver cutting in the near-by Huron

around it, and severed at a third spot above, was interpreted by Morgan as an indication of the beaver's inclination to eat wood as well as bark, probably in the winter or early spring. Gregg[2] illustrated a spirally gnawed tree in which a stout wire wound about the tree had caused this peculiar type of cutting. Not many miles north of the area of Morgan's study, in the Huron Mountains, I observed, some years ago, a similar example of beavers' work (Fig. 9). This was an aspen stump of about ten inches basal diameter, with two successive cuts encircling it, but only the third severing it. It is my belief that this stump represents winter cuttings of beaver on several different nights, with sufficient snow falling in between to necessitate starting a new cut the next night. At the third attempt, the tree was either felled in one night or snow ceased falling until the operation was completed.

From the evidence of beaver meadows, fallen trees, lodges, dams, and excavated burrows and canals, Morgan concluded that beavers had lived in this country for countless centuries. He went so far as to state (p. 84) that "these dams have existed in the same places for hundreds and thousands of years, and . . . have been maintained by a system of continuous repairs." The Indians of the region, practicing a type of wildlife management of their own, had apparently not been inimical to the beaver colonies. But Morgan foresaw their diminution as a consequence of the establishment of human settlements near by. He wrote (p. 123) that "these dams begin to decay as soon as they are deserted by the beavers, and quickly thereafter disappear; and . . . in no case do the latter remain in any district long after the establishment of the first settlements in their vicinity."

We now know that beavers, if supplied with their normal needs and not unduly persecuted by man, are somewhat tolerant of civilization. Even when greatly reduced, a beaver population exhibits a high degree of resilience if afforded proper protection, as has been demonstrated in Maine, New York, California, and elsewhere. But Morgan's predictions were, on the whole, remarkably astute. Beavers probably now number near a dozen, where Morgan once estimated them by the hundreds. Throughout the entire area, I found recent evidence of their presence only along upper Goldmine Creek, in the northwestern corner of the tract; even here they were absent in 1948, but cuttings indicated that they had not been gone for many years. A summer camper along Goldmine Creek told me of a few living there four or five years before, and stated that in recent years two beaver dams on this creek were blown out. It is possible that a few [illegible] may still persist along Green Creek, in the extreme southwestern part of the area, and there may even be a few inhabiting bank houses along the upper waters of the Carp River. Elsewhere, as foretold by Morgan, their structures have fallen into decay and they themselves have departed.

Advancing civilization has spelled the doom of much of our native fauna, and it is a sad commentary on the white man's culture that he is less able to coexist with the native animals than were his aboriginal predecessors. In the conflict the beaver has fared better than some, and yet the results of human dominance are all too evident. Given a modicum of consideration, this remarkable rodent will thrive in close proximity to man. Much of the area here under consideration is still virtual wilderness, with many streams suitable for beavers. Aspen is plentiful; many trees in the Grass Lake area measure more than twelve inches in diameter. The supposed desirability of the area for beavers is affirmed by the introduction here in 1948, by the state Conservation Department, of several nuisance beavers from country to the east. Their fate I do not know. Apparently it is not simply such features of encroaching civilization as railroads, highways, mines, and towns that are fatal to the beaver population. Rather it is the intentional destruction of the animals by trappers, legally or otherwise, or of their dams by fishermen who consider such dams a menace to their interests. Or, again, it may be the unintentional destruction of beavers' habitats by logging the forests, damming the lakes, or polluting the streams, which the beavers are unable to tolerate. These factors, many of which are subject to man's control if he so desires, hold the key to the future of the beaver. A proper appreciation of the aesthetic appeal of this rodent should enlist the aid of those in a position to assure his future existence in the country where Morgan first observed him so carefully.

REFERENCES

1. HEARNE, S. *A Journey from Prince of Wales's Fort, in Hudson's Bay, to the Northern Ocean.* London: Strahan & Cadell, 1795. Pp. 458. (Beaver, pp. 226–46).
2. GREGG, H. The Magnificent Rodent. *Sci. Mon.*, 1948, 67, (2), 73–82.
3. DUGMORE, A. R. *The Romance of the Beaver.* London: W. Heinemann, 1914. Pp. 225.
4. JOHNSON, C. E. The Beaver in the Adirondacks: Its Economics and Natural History. *Roosevelt Wild Life Bull.*, 1927, 4, (4), 495–641.
5. MARTIN, H. T. *Castorologia, or the History and Traditions of the Canadian Beaver.* Montreal: Wm. Drysdale & Co., 1892. Pp. 238.
6. MILLS, E. A. *In Beaver World.* New York: Houghton Mifflin, 1913. Pp. 240.
7. WARREN, E. R. *The Beaver. Its Works and Its Ways.* Baltimore: Williams & Wilkins, 1927. Pp. 177.
8. MORGAN, L. H. *The American Beaver and His Works*

SCIENCE ON THE MARCH

MAGNETIC RECORDING

AMONG the different methods of sound recording, magnetic recording has always occupied a unique position. It is the only method in which no permanent changes in the physical characteristics of the recording medium are brought about by the recording process.

In mechanical recording—for example, in disc recording—the surface of the recording medium is permanently changed by producing a modulated groove. Once surface portions of the recording medium have been removed by cutting, or deformed by embossing, the recording material cannot easily be restored to its original physical condition.

In optical recording, as in sound film, a light-sensitive layer is exposed to a light source of varying width or intensity. This results in a change in transparency of the film after it has been developed. Even more than in mechanical recording, it is difficult to recondition the light-sensitive layer so that it can again be used.

In magnetic recording, on the other hand, the recording medium can be used again and again. A magnetic record is made by impressing a varying magnetic field upon a moving magnetizable material. The magnetic recording medium, like any other magnetic material, does not experience any permanent physical changes after it has been subjected to magnetic fields. There may be temporary effects, such as expansion and contraction, caused by the magnetostriction of the material, but these dimensional changes can always be eliminated by bringing the material back to its magnetically neutral condition. In magnetic recording, because of the nature of the recording method, mechanical vibrations, temperature variations, and light have very little influence on the recording characteristics.

The fact that the physical characteristics of the recording medium are modified owing to magnetization implies that this method imposes basically only such frequency limitations as are usually inherent in electromagnetic processes. In order to record high frequencies of any predetermined value, it is only necessary to create appropriate operating conditions by taking into account the fact that storage of signals always requires space and that, thus, the process of recording or reproducing always requires relative motion of proper magnitude between the recording and playback device on the one hand and the recording medium on the other. Because of such motion, to each frequency a wave length can be assigned, and this wave length λ—that is, the length of the medium required to record one cycle—is given by

$$\lambda = \frac{v}{f},$$

where v is the velocity of the medium and f is the frequency of the signal.

As long as the magnetic recording medium moves fast enough so that the recorded wave length will never be shorter than a predetermined quantity in the direction of the motion of the recording medium, it is at least theoretically as easy to cover a frequency band up to one megacycle as it is to record frequencies falling within the audio spectrum. It has been found that in any practical recording system a wave length of about 0.001 inch is the minimum limit whenever reliable operation is expected. This limitation can also be expressed by stating that for each additional 1,000 cycles per second response versus frequency, the speed of the medium has to be increased one inch per second. If, for example, a 50-kc response is wanted, the medium would have to move with a velocity of 50 inches per second.

What makes it difficult to impress upon the recording medium and to reproduce from it a wave length shorter than 0.001 inch? The attempt to answer this question leads into the fundamental problems of recording. In general, there are three factors that determine the capability of a system with regard to the shortest wave length.

1) The ability of the recording device to affect only an infinitesimal length along the path of the sound track.

2) The degree of resolution of the playback device.

3) The ability of the recording medium to maintain an impression of predictable magnitude at any one point along the path of the sound track.

The closer these three requirements can be met, the more information can be stored within any given length of the sound track. It is therefore important to analyze these three points.

The recording device, usually called the "re-

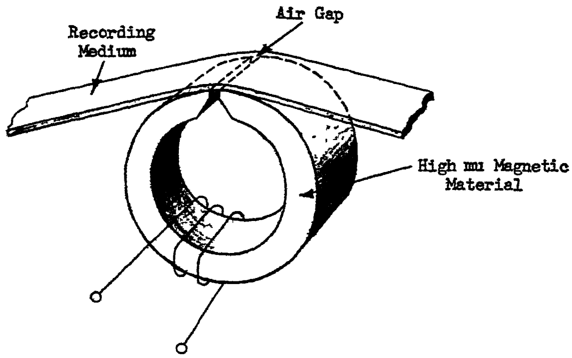


FIG. 1. Typical ring head as used for magnetic recording and reproducing.

cording head," must be so constructed that it will focus the magnetic flux so as to affect only a minimum length along the motional direction of the recording medium. Unfortunately, there is no magnetic insulator available to eliminate a magnetic field where it is unwanted. The best approach so far has been that of using a structure of high permeability material to confine the magnetic field and form the smallest possible air gap within the structure for creating a concentrated magnetic flux where it is needed.

Magnetic heads of a variety of configurations have been built. Lately the trend has been to use ring heads similar to that shown schematically in Figure 1. The length of the air gap over which the recording medium travels cannot, for practical considerations, be made much shorter than 0.0005 inch. The leakage field which is developed around the gap is not limited to the physical gap length but usually extends beyond it.

The effective gap length is not always the factor which controls the smallest wave length that can be recorded. The zone of magnetic influence of the recording head and the "biasing" process have to be simultaneously taken into consideration.

The playback device, usually called the "reproducing head," must be capable of scanning the sound track with the highest degree of resolution. Thus the function of the reproducing head is to detect the magnetic conditions of the recording medium as they exist within an infinitesimal length of the sound track. It can be shown that the flux emanating from such a short portion of the sound track is related to the flux retained within the medium at that "point." The reproducing head is frequently so designed as to guide all external flux lines which are generated over a relatively extensive length of the sound track through an appropriate path, but to evaluate only those flux lines which specify the condition at the point under consideration. This is exactly what is done by the previously described ring head, its effective gap length setting a limit to its resolving power. The reproducing head gap length in magnetic recording has, then, the same effect as the slit width in optical recording.

When the recorded wave length approaches the dimensions of the gap length, the reproducing head cannot continue to perform its function properly. In Figure 2 the loss in the playback process is plotted against the ratio of the effective gap length to wave length. When this ratio equals 1, 2, 3, etc., no response can be expected from the scanning device. With a gap length of 0.0005 inch and an effective gap length somewhat larger, it becomes obvious that wave lengths of approximately 0.001 inch are about the shortest that can be reasonably well handled.

The recording medium must be capable of retaining a magnetic impression in such a manner that the remanent magnetization of one point does not affect the remanent magnetization of another point. This means that the magnetic material must be highly resistive to any change of its magnetic condition. Such resistance to change is essentially given by the coercivity value of the material. Even if it were possible to choose an arbitrarily high coercivity for the recording medium, compromises have to be made for practical considerations. The requirement of being able to alter the magnetic status only under the influence of substantial forces during recording implies that even more substantial forces are needed to restore the recording medium to its original condition, since, as a part of such a process, the material has to be at least temporarily magnetically saturated. Restoring, or as it is more frequently called, "erasing," is performed by the erasing head. It is difficult to design

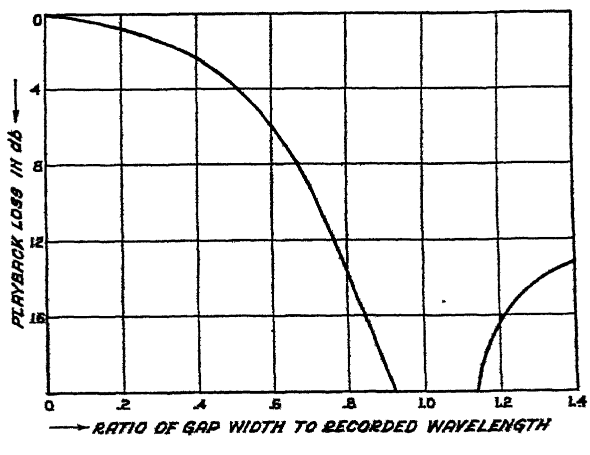


FIGURE 2.

an erasing head generating magnetizing forces in excess of 1,500 oersteds. The strength of the magnetic field of the erasing head sets the limit for the coercivity of the recording medium. A coercivity of 200–300 oersteds is usually considered a satisfactory compromise.

So far, nothing has been said about the relationship of the value of remanent magnetization left in the recording medium and the magnetizing forces of the recording head which are the cause of the remanent magnetization. Although not important for all applications of magnetic recording, for many it is essential that these two quantities be linearly related. Considering the nature of permanent magnetic materials, such linearity cannot easily be obtained. If one attempts to record on a magnetically neutral medium without the use of any other auxiliary means, the remanent magnetization left on the sound track will be badly distorted compared to the magnetizing forces which produced the remanent magnetization (Fig. 3).

A special treatment, which in the lingo of magnetic recording terminology is called "biasing," must therefore be applied. Two methods of biasing are known. The d.-c. method takes advantage of the fact that the up-and-down branch of the hysteresis curve has a long, relatively straight portion of which advantage can be taken in the recording process. A hysteresis curve of a typical magnetic recording material is shown in Figure 4. Prior to

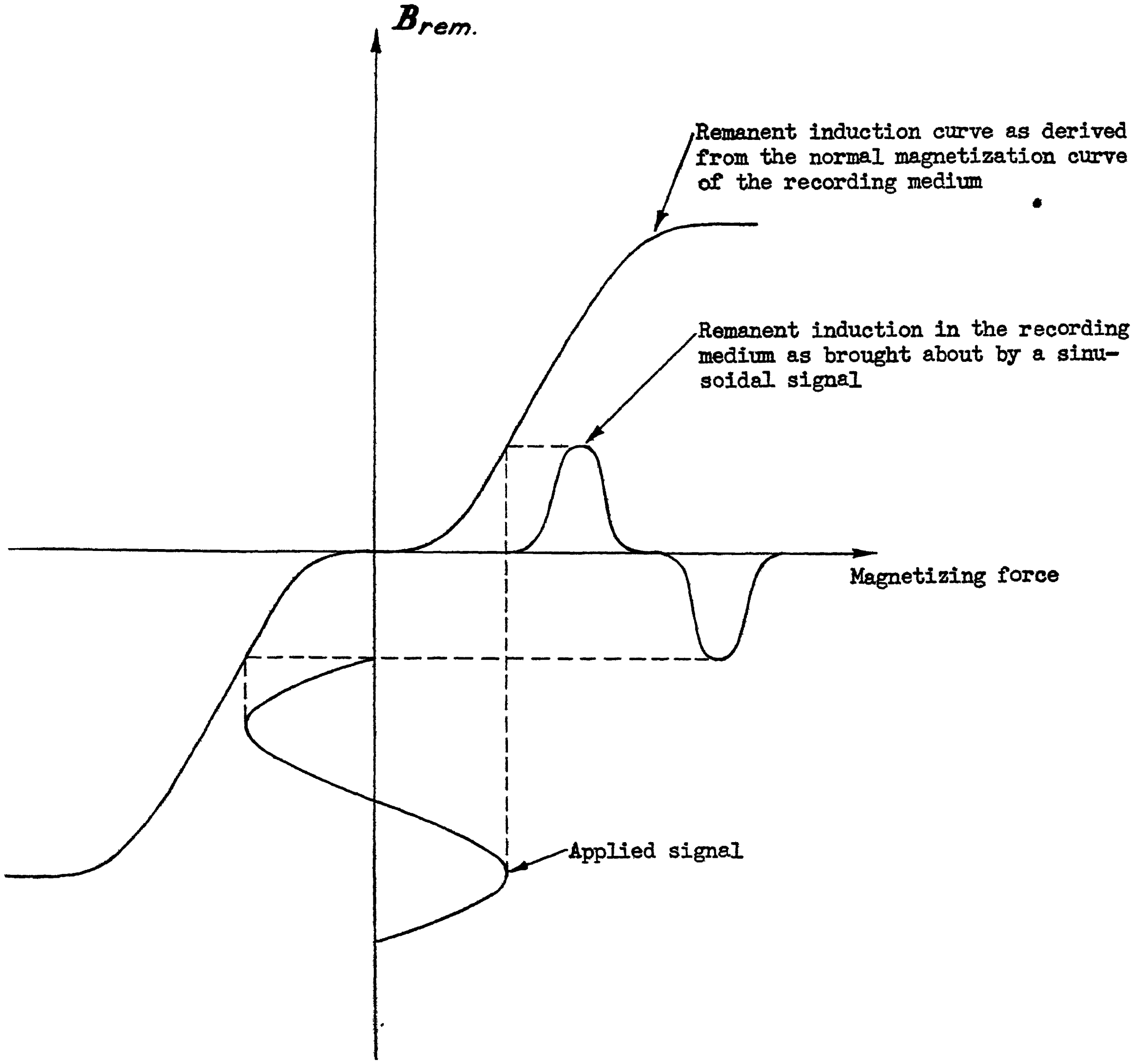


FIG. 3. Without the use of biasing, the remanent induction values left in a previously neutral magnetic recording medium are distorted when compared to the signal that was applied to the recording head.

the recording process, the medium is magnetically saturated, a process which incidentally erases any previously recorded material. During recording, a d.-c. magnetizing force of opposite direction to the saturating field is superimposed upon the signal in such a manner that the combined magnetizing forces produce an induction which, depending upon the strength of the signal, falls between the extreme points A and B of either one of the straight portions of the hysteresis curve. When the different elements of the recording medium leave the recording head, a remanent induction is left in the recording medium which corresponds to the varying induction values of the straight portion of the hysteresis loop but differs from them to the extent that now the magnetizing force is removed and, therefore, a change of magnetization takes place, which is given for the two points A and B by points A' and B', respectively.

This presentation of the d.-c. biasing process is greatly simplified and makes no allowance for demagnetization, which in any practical case has to be taken into account. Demagnetization is a reduction of the retained magnetism, and the degree of demagnetization is a function of the rate of change of the impressed magnetization along the sound track. When the effect of demagnetization is considered, it is found that only one half the straight portion of the hysteresis loop (A–C in Fig. 4) can be used.

In a.-c. biasing, a magnetically neutral material is subjected to the simultaneous action of two fields, one being a high-frequency field of constant amplitude, the other being proportional to the instantaneous signal strength. If again a simplification of the rather complex process of a.-c. biasing is permissible, the field produced by the high-frequency current may be considered as supplying the energy to collapse the hysteresis loop otherwise associated with the signal field, thus bringing about a linear relationship between the magnetizing field of the signal and the remanent induction left in the medium. The picture of a collapsing hysteresis loop implies that each incremental element of the recording medium is subjected to a series of cycles from the high-frequency source while the element passes through the effective magnetizing zone of the recording head gap. Diagrams have been plotted to establish the remanent magnetization in the recording medium as a function of the instantaneous values of the signal strength, taking the complex BH relationship during the magnetizing process into account. These graphical illustrations confirm the experimental evidence that there is a good proportionality between the strength of the signal at any instant and the remanent induction left in the recording medium. To prepare a recording medium for a.-c. biasing, it is necessary that the erasing process return the medium to its neutral condition.

It has been previously stated that the shortest wave length which can be impressed in recording is not only dependent upon the effective gap length of the recording head but is also determined by the biasing process. In accordance with theory and practice, the effect of the gap length in a.-c. biasing is much less important than in d.-c. biasing as long as the field intensity beyond the gap of the recording head decreases rapidly.

As in d.-c. biasing, demagnetization must be considered in a.-c. biasing, but in the latter case the range of linearity is not restricted to half the magnetization curve as it is in d.-c. biasing.

A review of magnetic recording would not be complete without a discussion of one of its most unique applications. Magnetic recording, like any other method of recording, can and has been widely used for temporary and permanent storage of audio signals. But magnetic recording, in its broader aspects, is not limited to this restricted field. For use in a general memory device—for example, in an electrical computer—the principle of magnetic recording has particular appeal because of the high speed with which the signal can be impressed on, and, at will, removed from, the recording medium.

During recent years, the development of suitable recording mediums in particular has provided the practical means for fast storing and quickly locating recorded data. In the early years, subsequent to Poulsen's invention of magnetic recording in 1899, it was the general concept that steel wire and steel tape were the appropriate recording mediums. In fact, steel wire and, to a minor degree, steel tape are still used in sound-recording equipment. In 1929, Pfleumer in Germany produced a magnetically coated paper tape. In coated tape, small particles of permanent magnetic material are imbedded in a binder and serve to provide the required magnetic retentivity.

Pfleumer's idea started a great many development activities—first in Germany, and, during the second world war and thereafter, in this country. The search, which was mostly directed toward obtaining more suitable sound-recording mediums, was extraordinarily successful. The signal-to-noise ratio, heretofore always limited by the surface roughness and lack of magnetic uniformity of steel wires and tapes, has been so improved, par-

ticularly by the use of plastic base materials, that it can be made to exceed 60 db, a value generally considered adequate for high-fidelity sound reproduction. Coated recording mediums, whether paper, plastic, or other materials are employed as base, can be given a configuration which inherently provides the desirable characteristic of a rapidly acting memory device. The plating of magnetic alloys on a nonmagnetic metal surface, a process also developed during the war, has proved equally useful for such applications. It has only to be kept in mind that the recording medium of a memory instrument must expose to the reproducing and playback heads a substantial surface area so that a great many data can be recorded and located with a minimum of time.

The potentialities of magnetic recording for a memory device can best be illustrated with reference to a typical example. Let us assume that the surface of a rotating cylinder is chosen to store one million items. Let us further assume that the existence or absence of a pulse at any one designated

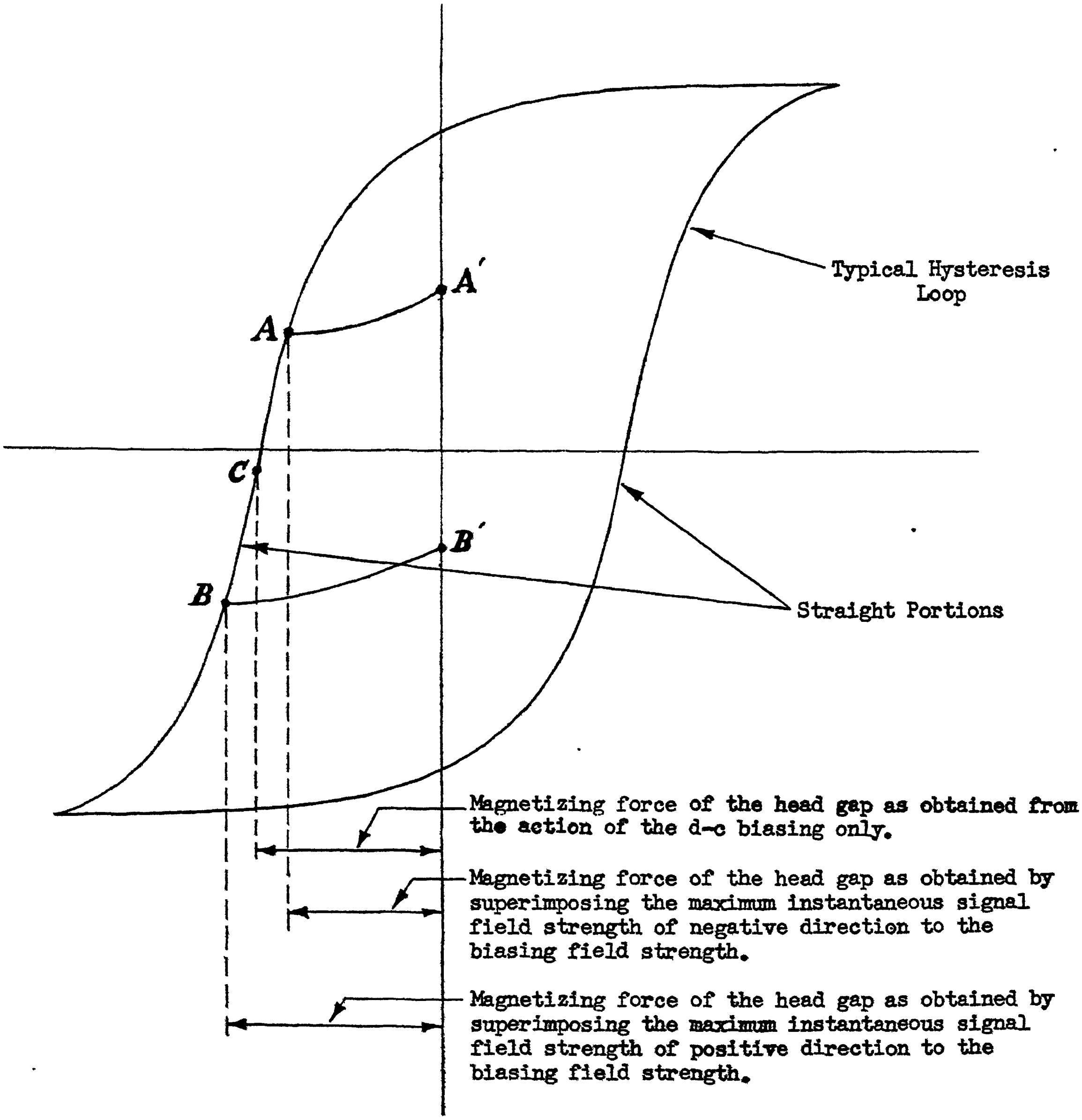


FIG. 4. In d.-c. biasing, elements of a previously saturated magnetic recording medium are subjected to the combined magnetizing force of the signal and d.-c. biasing field, and have remanent induction values along the sound track which are essentially linearly related to the instantaneous values of the signal.

point of the surface provides the yes-or-no information with regard to the one million items. Under these conditions the storage area can be easily calculated. Since each pulse should be impressed or removed without interference of any adjacent one, it is necessary to provide sufficient distance between them to assure trouble-free operation. By spacing the centers of individual pulses 0.005 inch apart along the sound track, adequate separation is obtained. This means that 200 items can be recorded for each inch of sound-track length. If a width of 0.01 inch is allowed for the sound track, and if the sound tracks are spaced 0.005 inch apart, about 66 sound tracks can be provided side by side on a recording medium one inch wide. A set of approximately 13,000 items can therefore be stored on one square inch, and a drum with a surface area of approximately 78 square inches will be able to handle one million information items. This surface area is obtained with a cylinder 5 inches in diameter and only 5 inches long.

Should there be a recording and reproducing head associated with each sound track, the maximum time required to locate any information would be that needed for the cylinder to complete one revolution. The rotational speed of the cylinder is limited only by the mechanical design. Magnetic recording itself does not, for all practical purposes, impose any limitations.

The electronic and switching equipment required to make full use of a memory device of this kind is complex by the very nature of a computer and need not be treated here. But one point is worth mentioning. In a system with only yes-or-no information, no attention has to be given to linearity between the signal-magnetizing force of the recording head and the remanent induction left in the recording medium. Under these conditions no biasing is required. If the amplitude of the recorded pulse is to be used to provide more than a yes-or-no answer, a.-c. or d.-c. biasing becomes a necessity.

S. J. Begun

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SCIENTIFIC PROCESSES FOR THE IMPROVEMENT OF FARM ANIMALS

IT IS estimated that in 1948 the total national income from the sale of livestock and livestock products was about 17 billion dollars. These products, when processed, amounted to about 24 billion dollars. Of even greater value is the benefit to the population of having at least an adequate supply of animal products for food and clothing.

Farm animals as we know them today have come a long way from the wild boar, wild sheep, and wild ox of prehistoric times. Changes in these animals were a part of the general process of evolution, occurring in response to environmental changes and to the directive action of man. The latter is particularly true for advances made during the past three hundred years or so. Man's interference with nature has come about so gradually that it would be easy to disagree as to the exact length of time that he has taken a major part in directing these changes. Regardless of whether it is three hundred, two hundred, or four hundred years, the changes have been slow, although they have been both progressive and constructive from man's viewpoint.

The first change man probably made in the environment of animals was to begin herding them together rather than hunting them. Subsequently, he must have taken some protective measures. During the hunting period, the wildest and fleetest animals were the ones most likely to survive and propagate their kind. With herding, and the provision of some protection against both wild animals and starvation, a somewhat more docile, and perhaps larger, type became favored, and gradually changes brought about by natural selection began to occur.

As man moved about in his search for a more suitable environment, and as a consequence of his wars with other tribes, he carried his animals with him. This led naturally to the crossing of types. Crossing was followed by breeding from "within," and that brought about segregation, with further opportunity for the selection of types better suited to the environment. Later, man introduced new types into his own stock with the object of improving it, and he also began selecting toward definite objectives. Henry VIII used these methods to improve the race horses of his day. He also promoted the race track, which provided a proving ground for his horses.

During the eighteenth century leading farmers of England and Western Europe settled down to the real business of breeding more useful farm animals, and the rate of improvement was increased. They deliberately searched for superior stocks to introduce into their own herds; they then bred from within their own herds and carefully selected progeny toward definite ends. More or less unwittingly, they used the only tools we have

A Minnesota No. 1 gilt. This type of pig was produced in this new breed in ten years from the time of the first cross.

today for livestock improvement—crossbreeding, inbreeding, and selection. It is noteworthy that several of the English breeders used inbreeding very deliberately at a time when the best biologists of the day condemned the practice. Fundamental biological knowledge had not yet advanced far enough for biologists or breeders to know what was involved in either crossing or inbreeding. The breeders, however, had discovered that they achieved certain objectives by the use of these methods. We must admit that they did a remarkable job when we consider that they violated the edicts of the leading scientists of the time and that there was little past experience to guide them. At the same time, it should be remembered that, for the most part, it took them fifty to one hundred years to make a breed, and that their breeds, when finished, did not possess a high degree of purity.

It should also be recognized that not all the breeds that are prominent today were developed by the orderly process outlined. Several came into being by a hit-or-miss series of events, mostly the result of a community endeavor to produce more useful market animals. If, however, we analyze the methods used we find that crossbreeding, inbreeding, and selection were always used, though with little precision or understanding.

Gregor Mendel, who laid the foundations for that branch of biology known as genetics, at the same time laid a foundation for improved and scientific methods of breeding for both plants and animals. In the period immediately following the rediscovery of Mendel's laws, too much was expected of them by many plant and animal breeders. In general, plant breeders did a better job than animal breeders of continuing to try to adapt Mendel's laws to their work of improvement. The result was that the plant breeder ushered in a whole series of new and modified techniques for plant improvement, techniques which embodied the three fundamental basic principles: crossbreeding, inbreeding, and selection. Plant breeders during this century have made many contributions to society, but the development of hybrid corn was perhaps of most economic importance, and it caught the public fancy. My own work, and that of my contemporaries, in making new breeds stemmed from the successes of the plant breeder, particularly his success with hybrid corn.

The fundamental task of the animal and plant breeder alike is to regroup the genes already in existence so that new gene complexes are produced, which in turn produce more favorable phenotypes. This is largely ignoring mutations and chromosome aberrations, which science has not yet brought under control to any practical extent. Since every mating in farm animals involves several thousand genes, which are grouped on about 40–60 chromosomes, the opportunities for regrouping the genes are almost limitless. Our comprehension of the application of genetics to practical animal breeding is very different today from the ideas that prevailed after the rediscovery of Mendel's laws. At that time the concept was of a small number of genes and of each gene, or at least most genes, being solely responsible for a given end result. Today it is estimated that the number of genes involved in producing a zygote runs into several thousand and that most character expressions are the result of the interaction of many genes. Work today has therefore come to focus more and more on gene complexes in animal breeding.

Crossbreeding is used in constructive breeding for two entirely different purposes: One is to introduce new genes into a population suspected of being deficient in some one or several respects; the other is to induce hybrid vigor through pairing of unlike genes for the production of commercial livestock.

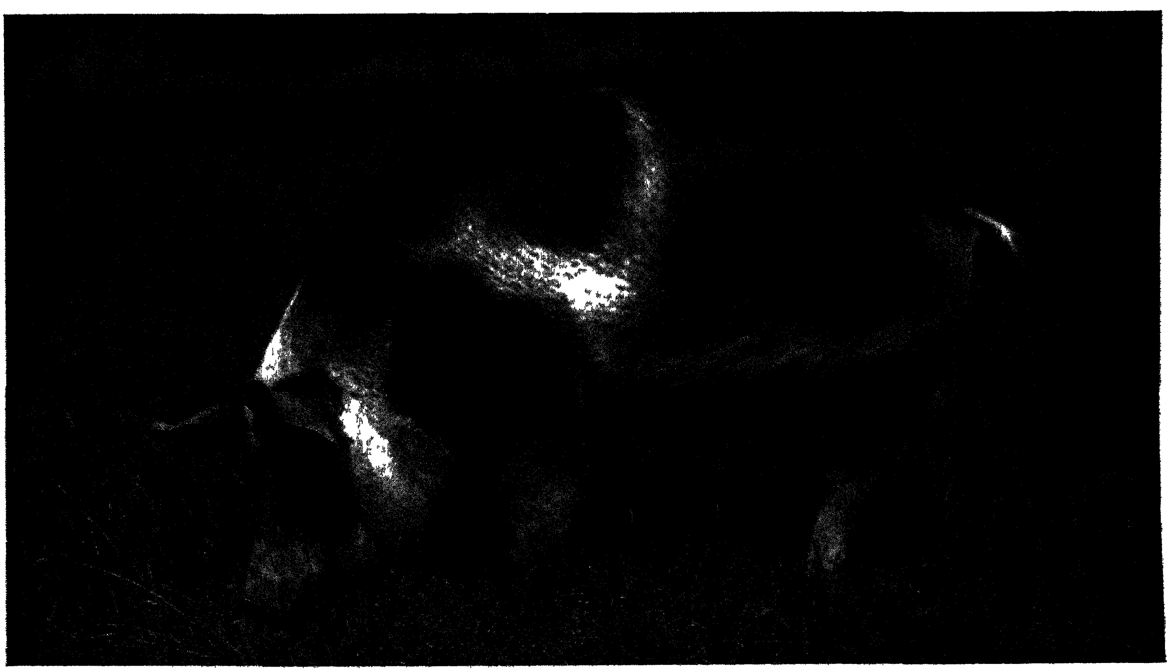
For the most part the older breeds yield some hybrid vigor on crossing. The amount varies between breeds and species; breeds of swine give an increased performance of about 6–7 percent for

a single cross and about 12 percent for a three-way cross. The breeds of sheep when crossed appear to yield more hybrid vigor; it is estimated that it will run from 12 to 20 percent. The Bureau of Dairy Industry, USDA, has released data on the crossing of dairy cattle which show an increased production of from 25 to 32 percent for the crossbreds. The beef breeds of cattle also cross to advantage. The evidence from crossing shows clearly that even though the process by which the older breeds were developed was slow and indefinite, yet considerable separation of genetic material did occur; otherwise there would be no increased vigor from crossing.

Since the existing breeds did cross to advantage, the obvious next step was to develop breeds or lines that would cross to greater advantage. In 1937 a concentrated attack on this problem in swine was initiated through the establishment of the Regional Swine Breeding Laboratory. This is a Federal institution that functions through cooperation with a number of state agricultural experiment stations. The modes of attack on the problem were somewhat varied. All, however, were directed toward the development of inbred lines that would cross to advantage, and all were planned to utilize in some way the three basic tools.

Inbred lines were started from within the existent breeds and from crossbred foundations. Superior lines have been developed from both types of foundations, but the evidence favors those started from the crossbred foundation. This is quite logical because, within reason, the broader the genetic base the greater the opportunity for effective selection after the process of segregation has been started by the interbreeding of the crossbred population. Three new worth-while breeds of swine have been developed during the past few years from crossbred populations; they are designated as Montana No. 1, Minnesota No. 1, and Minnesota No. 2. The Montana No. 1 was developed by the USDA in cooperation with the Montana State Agricultural Experiment Station, twelve years being taken to bring the animals to the point where they were recognized as a new breed. The Minnesota No. 1 and No. 2 were released ten and seven years, respectively, after they were started. This is in contrast to the fifty to one hundred years required to develop the older breeds. Furthermore, as measured by the coefficient of breeding, these new strains carry about three times the amount of genetic purification of the older breeds.

In a sense these new breeds are "tailor-made." This does not imply that everything desired has been bred in them; it does mean, however, that they were developed according to plan and that at least certain definite qualities were bred in them. What is more, the Minnesota No. 1 and No. 2 were planned to make a superior crossing combination,



A Minnesota No. 2 boar. This breed was made in seven years by the application of modern scientific principles.

and they do cross well. The crosses are yielding a strong 20 percent superiority in performance. Carcass quality is one of the factors included in the appraisal of performance; the carcasses of these two breeds yield an 18 percent reduction in fat over standard breeds reared under similar conditions.

The development of new breeds of inbred stocks to be used in crosses for market animal production offers the professional animal breeder an almost unlimited opportunity for the improvement of our farm animals. The whole principle rests on the sorting and regrouping of the genes. The process rests on the science of genetics and, finally, on utilizing the fundamentals of hybrid vigor.

Modern selection embodies the basic principles of statistics. In order to use statistics satisfactorily, it was first necessary to develop adequate techniques of measurement; the performance factors of farm animals are not measured by eye appraisal. The development of satisfactory techniques for measuring performance is still in the embryonic stage, but enough has been accomplished to indicate plainly that more progress can be expected through further refinement of measurements. A large portion of the refinement must come through a greater control of environmental influences. It is estimated that for most economic characters used as a basis for selection about 60–80 percent of the variance is due to such influences. In other words, the breeder at present cannot hope to advance the genetics of a population more than 20–40 percent of the selection differential.

Scientific processes have invaded the field of animal improvement. They involve a knowledge of basic genetics; for the most part the identity and manipulation of individual genes plays but little part in this process, since quantitative characters are generally dependent upon the interaction of many genes. An understanding of heterosis and an appreciation of its applications are fundamental. Inbreeding, crossbreeding, and selection are the tools for shifting genes into different combinations. Proper selection involves at least a partial control of environment, the development of more precise methods of measurement, and the utilization of statistics.

What has been done is barely a beginning. The work can be speeded up, and animals can and will be developed that are far better suited to specific purposes and to our ever-changing demands.

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Type of bacon produced from a cross of the Minnesota No. 1 and No. 2. Note the high percentage of lean meat.

BOOK REVIEWS

UP FROM THE APE

A New Theory of Evolution. Sir Arthur Keith. x + 451 pp. $4.75. Philosophical Library. New York.

IN THE sixty years since he began his comparative studies of monkeys and apes, Sir Arthur Keith, the *doyen* of physical anthropology, has added much to the literature on human origins. His previous studies have been mainly concerned with possible stages in physical evolution, and he has not shown, at any rate in his writings, much interest in what might be called the living context of physical change. Three years ago, however, he published *Essays on Evolution,* a volume in which, among other themes, he elaborated the view that nations, although political units, should also be regarded biologically as evolutionary units. His new book, we are told in the preface, is an exposition of the fundamentals on which this earlier volume of *Essays* is based. It deals with Sir Arthur's new "theory of man's evolution," with the demarcation of mankind into its major divisions or varieties, with the role played by "race" in evolution, and with the rise of nations.

Like those that have gone before it, the book is eminently readable. Its essential thesis is that human evolution has taken place by the physical transformation of individuals within small social groups, first of apes and then of proto-men, and that larger social groupings have been derived from these smaller groups. All this is readily acceptable, and indeed there is nothing very novel about Sir Arthur's proposals. Evolution is not something that occurs among the skulls on the shelves of a museum. It has always shown itself by the emergence of new physical and behavioral characteristics within the context of a social life. Scientific readers will, however, find that Sir Arthur's re-emphasis of this fact is associated with much strange and special pleading.

The book begins with a consideration of the social organization of primitive man and of the subhuman primates, and Sir Arthur speculates convincingly about the factors which hold small groups together—about such matters as territorial associations, kinship, patriotism, cooperation, and so on. "A group to survive," he writes, "must have amity and unity at home, and a will to resist attack from without." Isolation in groups, according to Sir Arthur, is essential for evolution. But obviously it is not the whole story, for otherwise, he asks, why have the apes, with the same group organization as primitive man, failed to evolve? This question is answered in the middle section of the book. Some group or groups of anthropoids became possessed of genes which led to an upright posture, and so man began his march toward humanity. The particular proto-men who embarked on this adventure were the African fossil anthropoids, and here Sir Arthur reverses his earlier and scientific caution, and swallows the speculations of Drs. Broom and Dart in their entirety. The African ape men spread, and gave rise to the five main divisions of mankind, whose diversification into races and national groups—biological units of evolution—takes up the final third or so of the volume.

It would be idle to underline the many shortcomings in Sir Arthur's treatment of the well-known doctrine of evolution in small groups, or in his discussions of the interaction of gene changes and selection in evolution. It is also unnecessary to comment on the many tautologous statements and contradictions—for example, about the force of "human nature" which on one page "takes the place of instincts," and on another is itself possessed of "instinctive actions." But it is necessary to show why the book fails as a serious scientific contribution. The reason is fairly clear. In spite of its title, Sir Arthur's new book completely begs the question of the problem of human evolution. He asks, for example, what were the mental gifts that gave ape men evolutionary ascendancy in the world of living things? And he answers, quite simply, the same qualities of leadership as characterize such modern human leaders as Stalin and Archbishop Temple. What, he asks, is the mechanism which underlies the evolution of the specific characters of the human body? And again the answer is there—changes in the system of hormones. What, again, was the first essential change that transformed an ape into man? Once more we have a ready solution—the sudden possession of genes which assured an upright posture. And, last, how difficult to the anatomist is the problem of the transformation of an ape to a man? To Sir Arthur, not difficult at all. "Such a transformation implies merely an increase in the organization and in the size of the brain, with a reduction in strength of jaw and teeth."

It is that "merely" which makes the new "theory" less a serious work of science than an example of how words can be used to by-pass problems. Writing this is hard for a reviewer who has learned much from Sir Arthur's earlier works. But it needs to be said. Physical anthropology is at present sick from a plethora of writings by a number of its older disciples who find composition and ex-cathedra statement easier than biometric study. Years of hard scientific work may be needed to reverse the setback it has suffered recently from proponents who seem prepared to go to any lengths of special pleading in order to further what Sir Arthur calls "an evolutionary credo."

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PROBING PSYCHODYNAMICS

Psychodiagnosis Saul Rosenzweig. xii + 380 pp. Illus. $5.00. Grune & Stratton. New York.

ONE of the promising signs of our increasingly complex and specialized age is an awakened self-criticism and a growing insight into some of our own and society's adjustment problems. An era of real individual maturity and enlightenment is still in the future, but a wider acceptance by the public of the need for modification or intensive restructuring of the personality with the help of skilled psychiatric and psychological therapists is evident.

The relatively new field of clinical psychology has emerged to help meet this challenge of reconstructing the sick and troubled personality: and, as a first step in the therapeutic process, these clinicians are being entrusted with the responsibility of charting the dynamic factors underlying the present status and functioning of the patient. In *Psychodiagnosis*, Dr. Rosenzweig, with the help of Dr. Kate L. Kogan, takes the reader into this field of exploring, describing and predicting the key factors underlying human behavior by means of psychological tests.

In succession there are presented fairly brief but instructive descriptions, followed by discussions of major instruments currently in clinical use for measurement and evaluation of intelligence, of intellectual impairment, of vocational aptitudes and interests, and for appraisal of personality both by means of questionnaires and inventories, as well as by the richer projective techniques. The material on each of the instruments is followed by illustrative test protocols which bring into sharper focus the particular diagnostic values and limitations of the tools in real-life situations. Then, after stressing the importance of the "battery approach"—by means of which the clinician seeks to obtain a well-rounded picture of the individual by employing a number of complementary procedures plus additional clues—the author demonstrates how thereby an integrated view of the patient may be obtained. The last half of the book is in essence a series of examples of the fused science and art of the psychological clinician.

Of course it is possible to question the inclusion of some of the materials selected for presentation and the omission of others. For instance, among the projective techniques, the Szondi and the Bender Gestalt tests, on which considerable research material became available sufficiently prior to publication, would seem to warrant attention, along with more ample treatment of the personality implications of human figure drawings. Similarly, graphology, the "action techniques," and the revealing play in "control-situations," "frustration techniques," expressive finger painting, plastic and balloon games, might well have been included within the scope of the book as implements both of diagnosis and of the larger therapeutic process.

Nevertheless, although the really advanced clinical psychologist will gain relatively little in new or refined skills by study of the work, even for him it has value as a convenient compilation, which offers as well good illustrative case material. Most significantly, though, in providing students, younger people in the field, and professionals with a scientific interest in psychodynamics with an up-to-dâte, readable, compact yet authoritative review of techniques employed by clinical psychologists today to gain a better understanding of the functioning, endowment, interests, drives, conflicts, and problems of their patients, Rosenzweig has made an important contribution.

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A VISIT WITH THE MACGINITIES

Natural History of Marine Animals. George E. MacGinitie and Nettie MacGinitie. ix + 473 pp. $6.00. McGraw-Hill. New York.

THE high light of any trip to the West Coast is a visit with the MacGinities—husband and wife —if you are so fortunate as to know them or go to see them after having heard about them. One is always welcome, and aside from the great pleasure found in their company on such occasions, you learn a lot of real biology, of the lives of the animals that live in, or draw their sustenance from, the sea, their kinds and classification, their habits and their habitats, their behavior and its variation, the relationships they bear to one another and to the world about them, succession in animal communities, what animals do, what they eat, how long they live, the evidences of their existence in life, and also after death—things for the paleontologist as well as the recent zoologist to ponder over. Before the spell of your visit is over, whether it be a matter of minutes, hours, or days, and whether it sounds appropriate or trite, you invariably end up with "You ought to write all this down." They have written it down, much of it, in this book. Moreover, they have dedicated it to the friends and visitors who thought it should be done.

It is couched in simple, easy-to-understand language, a rarity in these days of extreme specialization in science and concomitant specialized vocabularies. Where special terminology had to be employed because of the subject matter, all such terms are carefully defined in appropriate places in the text. The pronunciation and derivation of the animal group names are included along with the scientific names of the animals, so necessary for further reference to them in literature, laboratory, or field. By these means, the authors, both highly regarded ecologists, have achieved the happy result of writing informatively for the layman and the student without offending the more catholic tastes of the professor or the scientific authority, and so give this impressive journal of personal experience and observation the wide appeal it justly merits.

It is a marvel that two people working together

could have seen and done, read and written, so much, have done it so well, and embroidered it so entertainingly with factual animal anecdote and other pertinent comment on the ways of animals, and on man, and how at least some biological work should be conducted and what it takes to do it.

Among the animal-interest stories there is the one of their "pet" hermit crab. Through the shell a small hole was drilled at about the place where the crab fastened on with the tip of its abdomen. A little prodding through this aperture would cause the crab to drop out of its shell immediately. The MacGinities used to pick this crab up and prod it out of its shell for the entertainment of visitors. So often was this performance repeated that finally the crab would drop out of its shell whenever it was lifted out of the water. It would then rest on the bottom of the aquarium until its shell was dropped, whereupon it would quickly grasp the shell and re-enter it.

Where the field is so large it is not to be expected that all scientific names will be in accordance with the lastest findings of the systematists; or that all the pertinent literature could be satisfactorily or adequately covered. For example, the name long applied to one of the commoner California pistol-crabs—more properly a shrimp, *Crangon dentipes* (p. 277, Figure 128)—has been replaced by the very appropriate one under which it was originally described, *C. clamator*. In view of the reliability of the MacGinities' own personal observations, it is to be regretted that it was not possible for them to check or verify the material gleaned by them from the published work of others. In a few instances, bits of misinformation have crept into their otherwise excellent text.

In their reference to the tree-climbing coconut crab of the Indo-Pacific (p. 301), which feeds on other and smaller palm fruits often so obtained, the MacGinities unwittingly repeat the long-popular idea that this crab is able by its own unaided efforts to open ripe coconuts. The reviewer himself feels in a measure responsible for the perpetuation of this assumption handed down by a long series of authors, most of whom copied earlier, unverified statements or merely reproduced hearsay. Within the past decade, another author, in a carefully documented survey of earlier literature, called attention to the fact that no naturalist has yet witnessed a coconut crab opening an undamaged nut, and that, so far, all feeding experiments with mature whole coconuts have proved unsuccessful. More recent experiments repeated on Christmas Island in the Indian Ocean likewise failed.

The many half tones that illustrate this natural history may have necessitated the high-gloss paper used throughout, but it is neither well adapted to marginal notes, which many a reader will be intrigued to make, nor suitable for use in laboratories —where it undoubtedly will be in constant demand— for the inevitable wetting will damage the pages, if it does not cause them to stick together. The generally unique and intimate photographs of marine habitats and animals deserved better reproduction than the publisher seems to have been able to render.

Altogether too often has it happened that a real naturalist, in every sense of the word, found the observing, the experimenting, and the telling far more fascinating than the recording and writing. Virtually everyone can recall at least one fine naturalist of this sort, who possessed a wealth of firsthand information regarding marine animals but got little or none of it into print. Happily, the MacGinities have proved an exception to the rule. The general reader as well as the researcher will ever be grateful to them for writing down so much of what they learned in so inspiring a book, suggestive as it is of a thousand theses and biological investigations; to the publisher for having undertaken this fine effort for the MacGinities; to Thomas Hunt Morgan for setting them up in Cal Tech's Kerckoff Marine Laboratory at Corona del Mar, where so much of their original work was carried on (this reviewer thinks it was the best thing he ever did); and to the professors of their Stanford days at the Hopkins Marine Station at Pacific Grove, notably Harold Heath and Walter K. Fisher, for having turned out two such well-grounded and accomplished students as Nettie and George MacGinitie. The latter, by the way, is the newly appointed scientific director of the Arctic Research Laboratory at Point Barrow, where Nettie will serve as his research associate.

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A DIP INTO THE FUTURE

Must We Hide? R. E. Lapp. x + 182 pp. $3.00. Addison-Wesley Press. Cambridge, Mass.

ATOMIC bombs, the way in which they might be used, and the damage which might result are the subjects of *Must We Hide?* These are old subjects (nearly four years!), and most of the facts presented are already available somewhere in the literature, but the contribution of this book lies in the assembly and interpretation of these facts in simple language, and in the deduction from them of some important conclusions.

The immediate and lingering hazards of radiation, the vulnerability of skyscraper cities, the possible methods of bomb delivery and of defense against them are analyzed in a deliberately cool manner. A variety of background data is offered, including some interesting detail from the U. S. Strategic Bombing Survey.

Dr. Lapp is a well-trained physicist who happens also to be well informed about atomic bombs. He has had an extensive association with atomic energy matters, first in the Manhattan Engineer District, and later in the National Military Establishment. His serious consideration of unpleasant facts is therefore a matter of some importance. He is to be commended for avoiding sensationalism, but the reader will be

a little disappointed to find that the author's writing style is not sufficiently buoyant to prevent the book from becoming a bit of a chore instead of a pleasure.

The most interesting part is probably the author's plea for the dispersal of great cities. He insists on its desirability apart from the possibility of further war, and that this possibility simply clinches the argument. His methods for achieving such a large objective may or may not seem feasible to the reader, but his analysis of the problem deserves attention and his challenge to face the issue must be accepted.

In choosing a title, Dr. Lapp obviously played upon the words of Dr. Bradley's *No Place to Hide,* but he gave the words a different meaning. Whereas the earlier title can be interpreted literally in a physical sense, the theme of this book is that "We must not hide from the facts." It seems likely that Dr. Lapp's reason for this choice of title was to emphasize his commendable effort to place in broader perspective atomic bomb hazards in general and radiation dangers in particular.

Further efforts to meet the need for a level-headed appraisal of atomic warfare potentials will be welcome. As long as the possibility of another war persists, the problems and the facts discussed in this book must be widely faced and widely understood.

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FOR ANTHROPOLOGISTS AND OTHERS

Most of the World. The Peoples of Africa, Latin America, and the East Today. Ralph Linton, Ed. 917 pp. 19 maps. $5.50. Columbia University Press. New York.

AT THE mid-point of the twentieth (so far mankind's most terrifying) century, anthropology has become one of the significant voices speaking in the wilderness. And, one might also point out, there has been less crying and beating of breasts in accompaniment to its utterances than has been the case with most other professional sages that are on the scene today. The cross-cultural point of view contains a high element of objectivity. Its descriptive-comparative findings are presented dispassionately. The book *Most of the World* follows in this anthropological tradition. No Cassandra-cry, it is not even a call to arms. It is simply a statement of: "Here are the facts. Do with them what you will. But do not forget that in this world there are contemporaries of ours who outnumber us at least 10 to 1 and who have lived their entire lives under motivations and with values that are completely alien to the European-American patterns of behavior that seem so natural to us." The anthropologists, sociologists, and economists who have contributed the essays for this symposium offer no pat solution for "saving the world." They are, however, interested in realities and in the examination and understanding of all aspects of human cultural behavior. They are aware that the emphasis of any one segment of such behavior, if done at the expense of other segments, lessens the reality of even the particular aspect overemphasized. International credit is indeed a very real thing; but those who see the destiny of the Near East or Indonesia only in those terms will eventually become just as remote from the totality of the problem as a high-caste Hindu, whose quite different vision of a sane and well-ordered world also appears to him as indisputably correct.

Like Linton's earlier compilation, *The Science of Man in the World Crisis,* the present collection of essays is an attempt to make available to the nonspecialist the findings of anthropology. Each of the twelve articles is written by an outstanding regional or topical specialist: Howard A. Meyerhoff, "Natural Resources in Most of the World;" Stephen W. Reed, "World Population Trends;" John Gillin, "Mestizo America;" Charles Wagley, "Brazil;" H. J. Simons, "Race Relations and Policies in Southern and Eastern Africa;" William R. Bascom, "West and Central Africa;" Carleton S. Coon, "North Africa;" F. L. W. Richardson, Jr., with James Batal, "The Near East;" Daniel and Alice Thorner, "India and Pakistan;" Raymond Kennedy, "Southeast Asia and Indonesia;" Francis L. K. Hsu, "China;" and Douglas G. Haring, "Japan and the Japanese, 1868–1945."

A critical analysis of all these papers would require several pages and is, moreover, beyond the experience or capabilities of the reviewer. Each writer is steeped in his own material and possesses a great wealth of detail as well as insight into his subject. All articles are well organized and written. Meyerhoff and Reed have offered information, respectively, on the topics of natural resources and world population. Both Coon and Kennedy give fascinating accounts of Colonial policy and history. Hsu, the only non-Western author, combines an "insider's" knowledge of culture with modern anthropological and sociological training and techniques. In brief, each areal contribution is a "vest-pocket" monograph covering both overt and covert culture in the territory selected.

Will this book and others like it be used by those who are guiding our social, political, and economic destinies? A review of past history does not offer much hope on this score, but on the other hand cultural objectivity is a recent concept. We can only wait and see. Linton, in his excellent introduction, speaking of these vast areas and peoples of the world with whom we must work out together some design for living, closes with:

> It is hard for Americans, reared in the traditions of European ethnocentrism. to appreciate the importance of these emergent powers. In population and natural resources they represent most of the world and they are moving toward technological equality with the West at a startling rate. The purpose of this book is to give an accurate picture of the conditions which exist in most of

the world today in the hope that this may assist in the formation of public opinion and may provide a basis of sound knowledge for future planning. The task which confronts us now is that of trying to reconstitute one world on the basis of collaboration rather than domination We must devise techniques to conserve the advantages of the former European hegemony as far as possible while rectifying its injustices. Any realist must recognize that the chances of accomplishing this are far from good at present. However, we must find what consolation we can in the knowledge that if we fail there will be others to try again, and again, until world unity is achieved.

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ALL KINDS OF DATA

Human Behavior and the Principle of Least Effort. George Kingsley Zipf. x + 573 pp. $6.50. Addison-Wesley Press. Cambridge, Mass.

IF WE consider the population of the major cities in the United States, from the largest to the smallest, we notice that the second largest city is only about half the size of the largest, the third largest is only about a third the size of the largest, etc. This holds true not only at the present time, but throughout the whole history of the United States. In addition, this relationship holds true for practically all other major countries. We may also go to a vastly different area of human behavior, the use of language, and find that a similar type of relationship holds. If we compare the most frequently used word with the next most frequently used word, we discover that the second is used only about half as often as the first, and further that the third is used only about one third as often as the first, and so on. If we plot a frequency distribution of data of this sort, with the base line ordered according to rank frequency, we obtain a descending function which approximates very closely a hyperbolic curve. Or, if we plot this hyperbolic curve double-logarithmically, we obtain a straight line of negative slope. Scientists have long been familiar with the near universality of the Gaussian curve, in which the base line represents some quantitative variable, and the ordinate the frequency of occurrence. It appears, however, that those kinds of data that cannot be ordered except in rank order of size have not been studied extensively. When frequency distributions of this sort are made, it turns out that there are many, many areas of human behavior in which this type of distribution approximates a simple hyperbolic function. The best examples are the two given above.

This book consists of a vast array of such data drawn from such varied fields as linguistics, psychology, economics, sociology, population, music, politics, and even taxonomy. Most of the examples are ordered in such a way that the frequency distributions approximate straight-line functions when plotted double-logarithmically. The amount of work involved in collecting all this varied data was prodigious.

The main thesis of the book is that the regularity of this material may be explained, in large part, by the principle of least effort: "An organism will expend the *least average probable rate of work* (as estimated by itself)."

The first third of the book deals with linguistic data, with examples of language samples drawn from practically the whole range of human language: American newspapers, English novels, primitive languages, Chinese, Gothic, Old English, Old High German, vocabulary of a young girl at different ages, and samples of schizophrenic speech. Most of this material is presented graphically, and the empirical data fitted to straight lines. The ordered regularity of such a vast amount of varied material is surprising. The middle several chapters of the book deal with language as a psychological process, with the origin of the "ego," with the economy of the symbolic process, and with other psychological processes, including dreams. In practically all cases, these processes are viewed as instances of the principle of least effort. The last several chapters of the book deal with social phenomena, all of this material viewed as "a case of intraspecies balance." Here is presented a large number of hyperbolic functions, including size of metropolitan districts, manufacturing establishments, retail stores, occupations, movement of freight, distances of long-distance telephone messages, distances separating applicants for marriage licenses, incomes, and many others. This book, if for no other reason than the sheer array of data presented, represents a signal contribution to the behavior sciences.

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STUDY OF THE ORIGIN OF CANCER

Cancer Radiations, Virus, Environment. Vol. II. J. Maisin. 306 pp. 120 fr. Casterman, Tournai. Paris.

IN THE periodical *Science* (1949, **109**, 21), we commented on the first volume of this book. There, Maisin covered the extensive fields of heredity, hormones, and carcinogenic substances; in this second volume, the Belgian worker discusses the equally large, or still larger, fields of radiation, viruses, and environment. Now that the whole book is finished, one cannot help but feel a genuine admiration for the task accomplished of giving a coherent expression to the almost chaotic knowledge we have of cancer.

With increasing frequency in our times, books are written on various subjects by many persons and edited either anonymously or by one person with a well-known name. In either case the reader is given just the factual information or, at best, some unilateral views on the problems, and from these data—in most cases conflicting—and from an inhibited discussion—generally a mere formality—he is supposed to draw

his own conclusions. This procedure may be acceptable if the conception one has of a book is that of a more or less useful compilation of facts put together according to some pattern; but it is quite inadequate if one thinks of a scientific book as a conceptual endeavor, in which each concept is the synthesis of many facts.

The latter is what Maisin has done with his monograph, which is why the book is not only useful but enlightening. The sensation of unity the reader experiences is that conveyed only by those who are familiar with all the facts of a complex biological problem, both in the clinic and in the laboratory. If, on the one hand, Maisin's book reflects a considerable effort, on the other it gives that specific, indefinable impression of ease only achieved by the few who in their maturity can write about what has obsessed them for a very long time.

Precisely because of its unity, the book cannot be analyzed separately in its different sections, since these sections really complement one another. We should like to single out, however, the section on viruses, in which these agents as factors in the genesis of cancer are given their due importance, although one could hardly say that Maisin is a follower of the so-called virus theory of cancer. In the last eight pages of his book he lucidly summarizes the few fundamental concepts at which he has arrived, and on this basis tries to formulate an explanation for the cause of the disease. This he does by attempting to reach a compromise between the indubitable exogenous viral nature of some cancers, and the effects of endogenous inanimate carcinogens. Briefly, the initial carcinogenic effect would be on the nucleus which, in turn, acts on some components of the cytoplasm, which may become autonomous and then behave as viruses.

Maisin is not the only worker belonging to what can be called the "reconciling school," which attempts to find a logical common denominator for the conflicting views on the origin of cancer. Whether the logic of the school is valid or whether it represents an understandable but fruitless effort to reconcile what is scientifically unreconcilable must be left both to the judgment of the reader and to the advent of more discoveries.

We want to end by strongly recommending that this work be translated into English.

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BACTERIOLOGY UPSIDE DOWN

Peace or Pestilence. Theodor Rosebury. 218 pp. $2.75. Whittlesey House. New York.

IT IS of interest to the layman to learn that biological warfare, or "bacteriology upside down," the synthesis of epidemics, will be directed largely against civilians; also that either incapacitation or psychological implications may prove more important than actual killing. There is, however, no background of actual experience. "B.W.," unlike atomic warfare, could be conducted by small nations at low cost and would be hard to control by international authority.

As indicated by the author's long list of sources, there has been much written on the potentialities of biological warfare. Authoritative publications, such as the official statement of Mr. Forrestal and the earlier Merck report, have provided sufficient sound background for those working in fields of bacteriology, epidemiology, and the like. The foregoing official and other unofficial sources have regrettably provided much for exploitation by sensationalists, who are inclined to employ a technical discussion as a springboard from which to leap into political and philosophic considerations.

Such portions of *Peace or Pestilence* as deal with bacteriology and the appreciation of biological warfare are technically sound and deviate in no significant respect from the factual content of official publications. The political and philosophic chapters 11 and 13 merely emphasize the point made by the author on page 155 in speaking of scientists working during the last war: "Necessity lifted them out of their own familiar laboratories and transplanted them into quite unfamiliar soil."

Dr. Rosebury may believe that the only defense against weapons is the establishment of a permanent peace. To assume that such is likely to be accomplished or to be brought about by emphasis on the horrors and possibilities of lethal agents is to ignore the pattern of history.

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THE FAMILY CULICIDAE

The Natural History of Mosquitoes. Marston Bates, xv + 379 pp. Illus. $5.00. Macmillan. New York.

THE introduction to this book offers three major reasons why an animal group receives intensive study. These are: The economic importance of the group, the ease with which its species may be manipulated in the laboratory, and its attractiveness to collectors. In all, the family Culicidae ranks high, and the result has been the accumulation of a vast quantity of facts about mosquitoes: as living organisms in nature, inhabiting its quiet waters, plaguing man and animals, and bearing many serious diseases; as laboratory subjects for students of insect physiology, behavior, development, and disease transmission; and as a fascinating occupation for those whose greatest joy is to note and classify the great diversity exhibited by a group whose basic pattern is notably uniform.

Because of this, mosquitoes are one of the leading families of insects from the standpoint of hours devoted to their study, but in spite of it, a great deal of the knowledge is scattered and unorganized. Whenever the facts on any phase of the work are gathered

together, many gaps are found. Dr. Bates has brought together the salient facts on all phases of mosquito study dealing with the living organism, both in the field and in the laboratory. The 48 pages of bibliography attest to the quantity of literature that was studied to compile the work. It is far more than a compilation, however, since it embodies the important ideas of the author on numerous fundamental problems of animal biology and evolution. He has had many years of close contact with mosquitoes, from the marshes of Albania to the treetops of Colombian jungles, supplemented by long hours of study in the laboratory.

This reviewer can find no aspect of the relationship of the living mosquito to its environment, as exemplified by different species and groups, that is not well covered. As a taxonomist, he is particularly pleased with the treatment of mosquito classification and speciation, with its emphasis on the importance of nonmorphological characters for specific differentiation. The scope of the book is so wide that any zoologist would find it profitable reading, and this includes the physiologist, behaviorist, and the medical man dealing with insect-borne diseases. Dr. Bates has shown ably what we now know about mosquitoes and the relation of this knowledge to biology as a whole, and he points out clearly the most profitable paths for further research in the field.

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CONJURING UP TOMORROW

Nineteen Eighty-Four. George Orwell. 314 pp. $3.00. Harcourt, Brace. New York.

IT IS evident now that *Animal Farm* was a trial kite in political science fiction, sent up to determine the direction of the wind for this, the major flight. The former novel was satirical humor. This one dispenses with humor. The former novel seemed to stem only from the Soviet system. This one adds, to the clearly Russian impact, a picture of the ultimate end of English socialism ("Ingsoc") as it appears to be tending. It is magnificent and deadly satire, humorous only as is the bitter, sardonic laughter of man's utmost extremity. According to the *Saturday Review of Literature,* it "is the most compelling novel to appear this year." Critics on every book-review page hail it as a mighty "what-can-be" and "fantastic-horror" novel.

The story is of an average man, Winston Smith, who lives in London, the "chief city of Airstrip One, the third most populous of the provinces of Oceania," only thirty-five years hence. On the wall of his room is a "telescreen." Before his window hovers a helicopter of the Police Patrol, snooping. He lives with the knowledge that every act is seen, every sound is heard. He is a member of the Party, a worker in the Ministry of Truth, one who rewrites the past so that "Big Brother's" predictions always fit the eventualities. Posters of Big Brother's face—much like Stalin's—are everywhere. Slogans are omnipresent: "War is Peace," "Freedom is Slavery," "Ignorance is Strength."

Other workers are rewriting the language into "Newspeak," to reduce everyone's vocabulary so that only authorized thoughts can be voiced. To "crimethink" is to invite vaporization. Science has ceased to exist except as a study in what "another human being is thinking" or in how "to kill millions of people in a few seconds." War is ever-present; it is a way to limit production, to keep the "proles" underfed, to encourage ignorance, to increase the power of the Inner Party.

For power is the aim, power as an end in itself. Romance, family relationships, and individual personality must be in subjection to it. Early in the book, Winston Smith is a rebel and a seeker for the underground movement called the Brotherhood. Before the end, he—and the reader—knows that there can be no such group, that resistance is impossible. Pages of torture prove it.

It is a frightening book because of its very timeliness. Plainly, the seeds for future horror are being sown now. Totalitarianism is a first symptom, state socialism follows, and after that total destruction of our civilization. Mr. Orwell, "indignant and prophetic," calls upon man to beware, to stop, to look where he is heading. It is an emphatic warning and a challenge.

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BEGINNING OF THE INDUSTRIAL REVOLUTION

James Watt and the History of Steam Power. Ivor B. Hart. vii + 250 pp. Illus. $4.00. Schuman. New York.

THE title of this book has been carefully worded so as to avoid a common misconception of Watt as the inventor of the steam engine. As a matter of fact, the development of steam power was a gradual growth, covering several centuries. The book under review describes this development, not neglecting the scientific research that produced a body of laws and facts of thermodynamics which made possible the design of the steam engine.

The earlier pages of the book give an interesting picture of the dawn of human control over nature; and, coming down to the England and Europe of the time of Watt, the book describes the domestic industrial system then in existence in England, when the weavers and spinners labored under their own roofs and were not yet concentrated in factories.

The first steam engines were designed to act as pumps to remove water from mines and deep wells. The most important predecessors of Watt were Savery and Newcomen. In the pumping engines

designed by these inventors the cylinder acted also as condenser. The time necessary for heating and cooling the cylinder made these engines rather slow-acting. Newcomen's engine of 1712 operated at ten strokes per minute, but even at that rate it was much more efficient than manual labor or animal power.

Watt contributed two important improvements to the design of the steam engine. He introduced a separate condenser, which greatly accelerated the rate of working, and also saved steam and coal necessary for heating the cylinder before each stroke. He also introduced the double-action principle, by which steam pressure acts in both the forward and backward motion of the piston. The steam engine as thus developed by Watt remained unaltered in fundamentals for a century, until the introduction of the steam turbine, about 1884.

Hart's book gives an excellent picture of the whole subject, pointing out that it was Watt's engine that *produced* the factory system by bringing the textile workers from their separate homes to a common workshop. There is also an interesting aftermath which discusses, among other things, the ethical aspects of the industrial revolution. The book as a whole can be recommended to nonprofessional readers.

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USEFUL COMPENDIUM

Animal Encyclopaedia. Mammals. Leo Wender. 266 pp. $4.50. Oxford University Press. New York.

THIS is the kind of book that many of us of the older group of zoologists wish we might have had when we were much younger.

It is an alphabetical arrangement of the vernacular names of mammals, with short comments touching upon the range, number of species, size, classification, weight, anatomy, economic importance, and other pertinent facts. About 1,798 vernacular names are listed, and under most of the vernacular names the generic name and sometimes the specific name are mentioned. The "Latin index" lists the scientific names alphabetically, and opposite each appears the vernacular name; these are arranged alphabetically in the main text. Three pages are devoted to "Gestation Periods and Numbers of Young at a Birth." This lists about 150 names, with the gestation period and number of young for such as the author apparently had available. The question marks at several places in the column marked "gestation" and in the column marked "young" are valuable in that they emphasize the need for obtaining and publishing more information regarding these important phases of the development of animals. Four pages are devoted to "Classification of Mammals," a tabular arrangement which shows one of the recognized groupings in the scientific scheme of classification down to the families.

One hundred and forty-six small pen-and-ink sketches and half tones, most of which are good, picture both little-known and well-known mammals mentioned in the text.

This book will undoubtedly prove to be so useful that it should go through several editions, which will give an opportunity for adding other names and descriptions and making such minor changes and corrections as may be found desirable in the light of very critical use of the book. It is definitely a worth-while publication and brings to mind the very useful *Dictionary of Birds*, by Newton, although this work is much smaller.

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MICROBIOLOGY

Evaluation of Chemotherapeutic Agents. Colin M. MacLeod, Ed. xii + 205 pp. $4.00. Columbia University Press. New York.

FOR the past three years the Section on Microbiology of the New York Academy of Medicine has been annually holding symposia on subjects of current interest in the field of microbiology. In 1948, in the symposium of that year, the "Evaluation of Chemotherapeutic Agents" was discussed by fourteen outstanding authorities in the field; these discussions comprise the book under review.

Without doubt, the papers collected together in this volume represent carefully studied and authoritative opinion in a field that is rapidly expanding. Among the subjects critically discussed are the significance of drug concentration, blood levels on renal clearances, the binding effect of proteins, the problem of microbial resistance, defense mechanisms, localized infections, the nature of local lesions, chemoprophylaxis antimalarial drugs, rickettsial diseases, viral infections, and the chemotherapy of cancer. The whole field of the evaluation of chemo- and antibiotic therapy is brought into focus in this volume. Every student in this field and every clinician who employs chemo- and antibiotic agents in his practice would profit by reading these discourses.

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CORRESPONDENCE

IN DEFENSE OF LYSENKO

Robert C. Cook's article, "Walpurgis Week in the Soviet Union," in the June issue, is a remarkable performance. In his defense of democracy, Mr. Cook denounces all who dare disagree with him as givers of aid and comfort to the enemy, and makes it clear that anyone so unwise as to reply is to be considered in advance an enemy of democracy. Despite this danger—and it is a serious one—it would be spineless to permit his article to go unanswered. There is no question here of any unfairness to the Soviet Union, but merely of the fact that his emotional discussion is a distinct disservice to science.

Mr. Cook, with considerable support from the hysteria-provoking headlines of our press, evokes a picture of terror against scientists in the Soviet Union. He is a little careless, however, about details. On page 368, for instance, Vavilov, Lysenko's opponent, "died mysteriously in Siberia in 1942." On page 369, research has added to our knowledge, and it is stated flatly that Vavilov was "liquidated in 1942."

Mr. Cook can give no references to confirmed reports of Vavilov's "liquidation." He can refer only to rumors, made venerable by continuous repetition. Back in the thirties, Vavilov was so often killed or imprisoned—in American newspapers—that he sent an indignant denial to the American press, which he accused of using his pretended persecution merely as a stick with which to beat the Soviet Union. In 1942, when his death was reported, he was supposed to have been liquidated mysteriously in Leningrad; the site of liquidation was later shifted to Siberia, in the minds of American readers a much more sinister locale.

Mr. Cook does not trouble to explain why Vavilov should be done away with in 1942, a year which saw the Soviet Union ready to grasp the hand of any temporary ally who offered the slightest help—even of so determined a long-time enemy as Winston Churchill. Nor does it strike Mr. Cook as odd that N. Vavilov should be liquidated and his brother, who shared many of his views, should be elected head of the Academy of Sciences.

Mr. Cook notes a report that "Schmalhausen, Dubinin, and Orbeli have been liquidated." He adds, "This has no confirmation." But once the rumor is spread, the damage is done, the picture of terror evoked. (It is well to remember, however, that such liquidations are reversible. After many liquidations, Litvinov returned from the grave to become wartime ambassador to the United States. And on September 1, 1947, in different pages of the Washington *Evening Star,* General Zhukov was given an important assignment and liquidated on the same day.)

The fact that a discussion of "liquidation" occupies so large a portion of Mr. Cook's article is no accident. Such a discussion serves the purpose of shifting the ground, of preventing judgment of scientific principles and theories on their merits, of persuading American scientists that Russians do not hold the opinions they express, but have been terrorized into accepting them—and therefore that the opinions are not worthy of serious consideration.

Most Soviet biologists once shared Mr. Cook's low estimate of Lysenko. Like him, they ridiculed Lysenko's views, called them unscientific, and refused, taking advantage of prestige and official positions, to appoint Lysenko's supporters to research and teaching posts.

The Soviet government did not, for a long time, intervene in Lysenko's behalf. It adopted, instead, the position of mediator, and in 1939 arranged a conference on genetics and selection at which all the participants were allowed complete freedom of discussion. At the conference, the philosopher M. Mitin, who more than anyone else expressed the attitude of the government, praised both Vavilov and Lysenko for their achievements (several years after Vavilov's original "liquidation") and criticized them both for their shortcomings. Vavilov, he pointed out, tended to separate his theory from practice; Lysenko and his followers, he said, were dogmatic and made exaggerated claims. The conference ended on the note that both sides were working for the advancement of biology, and that actual results would decide between them.

Lysenko himself obtained products which orthodox genetics could not explain and, in fact, claimed could not be obtained by his methods. Practical methods he recommended were tremendously useful to Soviet agriculture, before, during, and after the war, all through a period when agricultural production was a matter of life and death.

The Soviet government was impressed by these facts; Mr. Cook is not. He sneers at Lysenko as only "a practical plant breeder," just as he sneers at Burbank as "capitalism's Michurin," who had "an inflated ego, a flair for weird statements, and a contempt for plodding experiments." That Burbank also obtained results that his more orthodox rivals, with their uninflated egos and their unweird statements, could *not* obtain, strikes him as merely a matter of " 'a green thumb'."

The orthodox geneticists were, on the whole, in control of the universities and the more academic scientific institutes. Lysenko's supporters, on the other hand, were recruited mainly from the agricultural institutes. The struggle between the two groups came to a head when the latter found their experiments limited by the authority of the orthodox geneticists.

In discussing the resulting conference, Mr. Cook notes that "the minority of Mendel-Morganists were frequently interrupted and ruthlessly heckled." This is true, but it hardly has the sinister significance Mr. Cook implies. Both sides were liable to interruption and heckling. During the 1939 conference, the shoe was mostly on the other foot, and Lysenko and his supporters were frequently interrupted by denials and shouts of contradiction from the audience. From our point of view this may be bad manners, but it is hardly sinister.

The government and the Communist Party did not interfere in the controversy. For those who are naive enough to conclude from this that the government wanted to encourage freedom of expression, Mr. Cook has a more terrifying interpretation: the government wanted the assembled biologists to *commit themselves,* and then, after their words were recorded, Lysenko was given a

chance to "explode the bomb" by announcing that the Central Committee of the Communist Party had examined his report and approved it.

How the Soviet government can possibly escape from the dilemma in which Mr. Cook has placed it, I cannot see. If it announces its views beforehand, it is guilty of repressing freedom of speech; if it reserves its views, it is for the purpose of liquidating dissenters. Mr. Cook has it coming and going.

Mr. Cook is so far carried away by his emotions as to sneer at the "futility" of Soviet work in the Arctic (despite the very different opinions of such men as Stefansson). He even plunges rashly into the dangerous field of economics to support Varga, who had expressed the opinion that the United States might stave off an economic crisis for several years. Varga has changed his mind, and so of late have many of our home-grown economists. Mr. Cook has picked a bad time to defend Varga's opinions.

It is impossible to discuss every peculiar detail in Mr. Cook's report. But whatever American biologists think of Lysenko's views, they have a right to accept or reject them without being confused by tales of Soviet liquidation, or intimidated by the threat of their own liquidation from the faculties of American universities They would do well to remember that even such American experimenters as Tracy Sonneborn have indicated that nuclear genes may not be the sole carriers of heredity (without, however, associating themselves with Lysenko's views).

In the December 1948 SCIENTIFIC MONTHLY, Jacob W. Gruber points out that the Cuviers, the Mayers, and the Virchows—among the most eminent men of science of their day—had "intellects imprisoned and imaginations shackled by hypotheses of their own making." Mayer, for instance, concluded that the Neanderthal skeleton was that of a "rickety Mongolian Cossack." And the author wonders "how many 'rickety Mongolian Cossacks' exist in the controversies and conclusions, the debates and deductions of today."

Our own eminent biologists may be as much prisoners of hypotheses of their own making as were Cuvier, Mayer, and Virchow. They would do well to acccept or reject Lysenko's views on their merits, and to guard against the invention of a liquidating Ukrainian peasant.

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New York City

MR. COOK REPLIES

In my forthcoming article in the *Journal of Heredity* I am citing the sources of statements made in THE SCIENTIFIC MONTHLY article. That Vavilov died late in 1942 or early in 1943 in the town of Magadan, in Eastern Siberia, was learned from reliable confidential sources. That his brother Sergei's presidency of the Soviet Academy of Science did not save him from some sinister fate is evidenced by one official Soviet datum. The invitations sent in June 1945 to attend the 220th Anniversary Celebration of the "Soviet" Academy of Sciences (founded by Peter the Great!) contained a list of living and of recently deceased members of the Soviet Academy of Sciences. The name of Nicolai Vavilov appears neither on the roster of the living nor of the dead. Hence it is a reasonable deduction that in 1945 he was not even a dead member in good standing. Ergo, something had happened to him. If Nicolai Vavilov is actually still on this earth, an interview with him by a person or persons who knew him prior to 1942 would cause Dr. Dobzhansky and me very cheerfully to eat every word we have said to the contrary.

That other distinguished Russian biologists have either lost their lives or been banished into an outer limbo of utter silence was a matter of common knowledge when Dr. Muller was in Russia before 1938. Since many recent firings have been officially recorded in *Pravda*, Samachson's demand that this part of the record be ignored as irrelevant is obviously absurd. Anyone who reads the recantations that followed the August 1948 meeting, and who followed the events recorded in *Pravda* during the succeeding weeks, knows the question of the *scientific validity* of Lysenko's views is not an issue. The biologist in Russia takes leave of all personal opinion and follows the party line, or he takes the consequences.

All the apologists for the Lysenko cult attempt to picture the conflict between Michurinism and Mendelism as a valid scientific controversy. This definitely and emphatically is not true. Lysenko's sacred "views" are not valid scientific hypotheses. His queer "Michurin teaching" *simply denies, and makes no attempt to account for, any of the abundantly demonstrated basic phenomena of Mendelism.* The position of the Lysenkoist is essentially that of a physicist who, boldly denying that stones fall if tossed from a window, builds on this brash allegation his own "private world" of views of physical reality.

Geneticists are not "prisoners of hypotheses" in their rejection of Michurinism. It is rather the Michurinists who are *prisoners of the facts.* Their false allegations ring them around with an impassable barrier of continued and repeated observation which prove their assumptions false or which permit of a simpler explanation. Saint Luther with his wheat "with the same specific gravity as granite," and Saint Trofim with his "thousands of graft hybrids" cannot outtalk reality. If Lysenko could account for the basic one-two-three's of genetics he might have a case, but that he has never done. It is significant that competent biologists seem unanimously agreed on this point. They are surprised that chemists like Samachson and Spitzer seem so intent on interpreting the basic facts of life for them.

ROBERT C. COOK

Journal of Heredity
Washington, D. C.

TO A YELLOWJACKET

I've learned a bit about you, in your gleaming black and gold
And of your fellow workers, whose multitude untold
Engage in every industry within your social mold.
I know your every synonym, your type and allotype.
I've watched you garner nectar from the fruit that is just ripe,
And malaxate your wood pulp, and tyloids gently wipe.
Hail to you, Vespine Beauty, with your eyes emarginate,
Yellow clypeus extending to your mandible dentate,
And your three ocelli shining from the vertex of your pate.
Hind angle of pronotum extending to the wing;
Your diploid gyne and ergates, your short-lived, haploid "king,"
But I've learned enough about you, through your carboxylic sting.

ALBRO T. GAUL

Brooklyn, New York

THE

SCIENTIFIC MONTHLY

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FOOT-AND-MOUTH DISEASE—A HAZARD TO THE WORLD'S FOOD SUPPLY

H. W. SCHOENING

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IN DECEMBER 1946 an announcement was made from Mexico City which had far-reaching implications and which resulted in a gigantic international cooperative battle against one of the most feared of the world's livestock plagues. This announcement was that foot-and-mouth disease had been found in the Republic of Mexico. It was a matter of grave concern to the livestock industry of both the United States and the Republic of Mexico, since this disease had not appeared on the North American continent since 1929.

Foot-and-mouth disease, caused by a filtrable virus, gets its name from the fact that vesicles, or blisters, form in the mouth parts, particularly the tongue, and between the claws on the feet. The disease produces severe lesions in the mouth and on the feet. It has a general effect on the animals with a rise in temperature, since the blood stream is invaded temporarily by the virus. Milk flow is decreased, and the animals lose weight. The disease is complicated in many instances by secondary infections involving the feet and rendering the animals more or less helpless. The average mortality is about 3–5 percent, but in some years, in countries where the disease is epizootic, the mortality may reach as high as 50 percent of some herds. The aftereffects on the animals are sometimes long-standing, and in dairy animals the milk supply is depressed for many months. Abortions and mastitis, an inflammation of the udder, frequently follow an attack of foot-and-mouth disease.

The disease is one of the scourges of the livestock industry. It has existed in many countries for years, despite strenuous efforts on the part of the livestock sanitary officials of those countries to bring about its control. Cattle, swine, sheep, and goats are primarily affected, although all ruminants and cloven-footed animals are susceptible. Man is quite resistant, and authentic reports of cases in the human family are very rare. The damage caused by the disease, in the reduction of supplies of meat and meat food products and milk, when once it becomes established in a country, runs into high figures. The United States, through its Federal Bureau of Animal Industry and its state livestock sanitary services, has been and is on guard against the introduction of the disease into this country. Since the turn of the century, however, the disease has been introduced into the United States five times—in 1902, 1908, 1914, 1924, and 1929. In each instance, it was eradicated within a comparatively short time through the stamping-out, or slaughter, policy, which is predicated on the early recognition of the disease, the slaughter of all infected and exposed animals, disinfection, and quarantine measures.

The proximity of the disease in Mexico, where its existence was established through the investigations of veterinarians from the Mexico Depart-

ment of Agriculture and the Bureau of Animal Industry, United States Department of Agriculture, immediately brought realization of the danger to the livestock industry of the United States. It was apparent that unless active steps were taken to control and eradicate the disease in Mexico, it would soon spread northward and eventually cross the border into the United States. As soon as the presence of the disease in Mexico was determined, a quarantine was immediately placed by the United States on the United States-Mexico border. This prohibited the movement of susceptible species of livestock from Mexico into this country. Since the cattle industry in the northern part of Mexico annually exported to the United States about 500,000 animals, this embargo was a severe blow to the economy of the northern part of Mexico. The Mexican officials were cognizant of the effects of foot-and-mouth disease upon the livestock industry of the country and were anxious to control and eradicate it.

Conferences were held between the officials of Mexico and the United States to bring about cooperative efforts to control and eradicate the disease in Mexico. The Secretary of Agriculture asked Congress for authority to assist the Mexican government in the eradication of the disease through the stamping-out policy. Legislation to this effect was passed by Congress. Funds were provided whereby a joint effort to stamp out the disease was undertaken by the two governments. A Mexican-United States Commission for the eradication of foot-and-mouth disease was established, with headquarters in Mexico City. Forces including veterinarians, technicians, business administration clerks, etc. from the United States and Mexico were recruited to start the campaign of eradication by the stamping-out, or slaughter, method, which has been so successfully used in the United States and England. It took considerable time to develop the organization and to provide equipment necessary for the slaughter of the infected and exposed animals and for their disposal through deep burial. The extent of the disease was determined, and quarantine lines were established to prevent the spread of the disease beyond the quarantined area. Efforts were then directed to eradication within the area through the slaughter of infected and exposed animals.

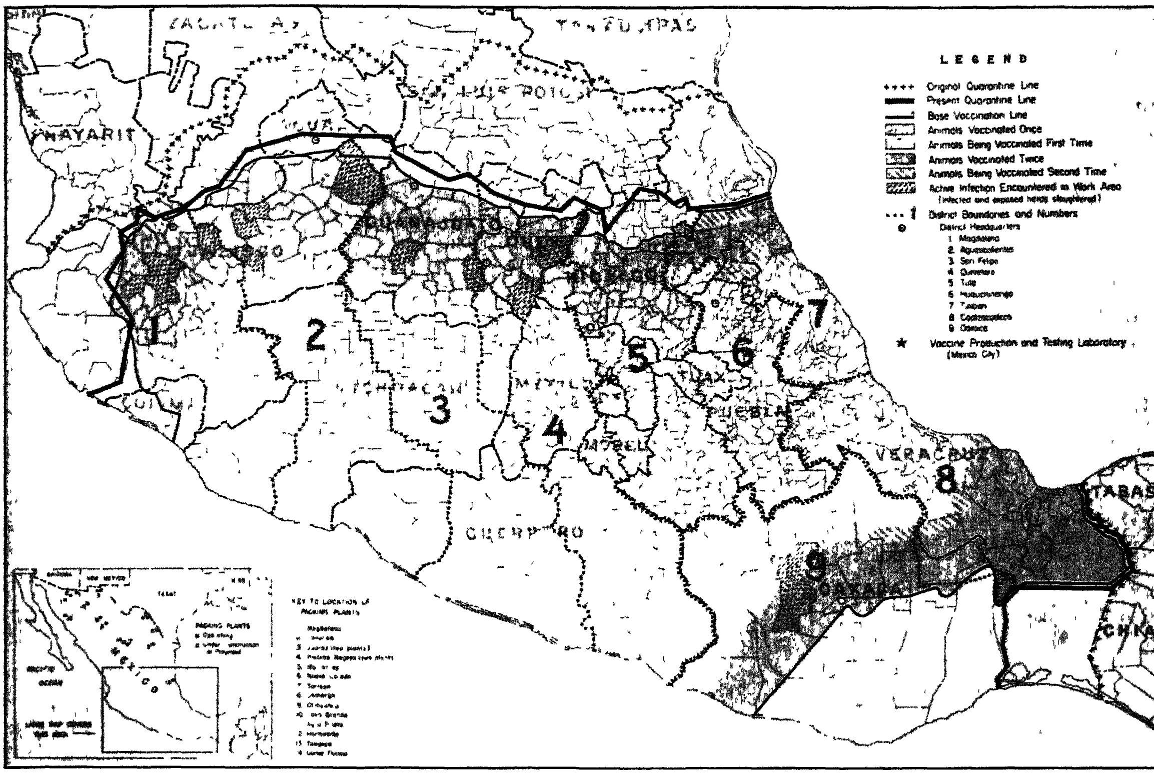
The disease was definitely diagnosed in December of 1946, but there are indications that it had existed for perhaps two months previously. The infection was first diagnosed in the vicinity of Puebla, about fifty miles from Mexico City, where it had spread from the Gulf Coast in the State of Vera Cruz. The accompanying map shows the infected area with the northern and southern quarantine zones.

The task of stamping out the disease was a tremendous one. Many difficulties were encountered in carrying out the program, despite the best of cooperation between the officials of the two governments. There was opposition on the part of cattle owners, since they were not familiar with the disease and the necessity of stringent measures for its eradication. Information forces worked hard through newspapers, motion pictures, radio, and direct contacts to promote understanding of the program. The Mexican National Army is responsible for enforcement of quarantine and other orders and for protection of the control forces. The disease had gained much headway before the stamping-out program could get into full speed.

The impact of the extensive slaughtering on Mexican economy was a matter for serious consideration by the Mexican officials, and in November of 1947 the Mexican government insisted that it could no longer continue the program because of the damaging effect upon the economy of the country. It requested the United States to revaluate the program of vaccination, slaughter, and quarantine. A new program was instituted by mutual consent of the two governments in November 1947, with procedures based on maintenance of the quarantine lines, repeated inspections, and vaccination of all susceptible animals in the quarantined area as quickly as possible, together with a modified slaughter program.

The development of an effective vaccine against foot-and-mouth disease has been based on extensive research done over the years in various countries of the world where foot-and-mouth disease is enzootic. The first indication that an active immunity could be established in cattle through the use of an inactivated vaccine was reported by Vallée of France in 1924. He showed that it was possible at that time to produce immunity under certain conditions with a vaccine in which the virus of foot-and-mouth disease was inactivated by formalin. This type of vaccine was irregular in its results but furnished a lead for other workers to build upon.

In 1934 Schmidt of Denmark showed that it was possible to produce immunity with a vaccine in which the virus of foot-and-mouth disease was adsorbed on aluminum hydroxide. This method was further studied by Waldmann of Germany and his co-workers and resulted in the development of the present-day vaccine which utilizes



Infected area and quarantine zones

the virus of foot-and-mouth disease adsorbed on aluminum hydroxide rendered inactive by the addition of formalin. This is known as the Schmidt-Waldmann type of vaccine and is the one that has had extensive use in Europe.

Modifications of this type of vaccine have been made in South America and Mexico by utilizing the principle of adsorbing the virus on aluminum hydroxide and the addition of formalin. The Schmidt-Waldmann type of vaccine as prepared in Europe is injected into animals subcutaneously in doses of 30 cc, whereas the modification of the vaccine in Mexico calls for the injection of 2 cc intradermally.

Briefly, the vaccine is made as follows: A number of cattle are inoculated on the tongue with the virus of foot-and-mouth disease in a very active form. At the end of twenty-four hours there is a marked multiplication of the virus at the sites of inoculation, the so-called primary vesicle of the disease. The animals are slaughtered at this stage of the disease when a multiplication of the virus has occurred only at the site of inoculation and blood infection has not yet begun. After the animals are slaughtered, the tongue is removed, the epithelium containing vesicles filled with fluid is stripped off, and this material is processed into the final vaccine. The animals are dressed in the abattoir in the usual way, and the meat is sold for food after inspection. The previously harvested virus is then ground through very fine mills and proper diluents added to extract the virus from the tissues. The material is finally centrifuged and the virus collected in solution. The appropriate amounts of aluminum hydroxide are added and, finally, 0.05 percent formalin to kill the virus. The material is kept at a temperature of 25° C for 72 hours and then is bottled as the final step before it is ready for use. Tests of the finished product are made for innocuity or safety; that is to say, the vaccine is tested on cattle for the presence of active virus. Each lot of virus is also subjected to potency tests. A group of twenty animals is vaccinated intradermally with 2 cc of the virus. Fourteen days later half of these animals are exposed to the virus by inoculation on the tongue, the other half are exposed by contact to infected animals. An appropriate number of control animals checks the activity of the virus used in exposure. Only those vaccines which protect animals completely against generalization of the disease are considered to be satisfactory. These vaccines have previously been determined to contain no active virus. It has been possible to grow the virus of

foot-and-mouth disease in tissue culture using the epithelial tissue of guinea pigs and calf fetuses.

In more recent years Dr. Frenkel of Holland has developed a technique whereby it is possible to grow the virus of foot-and-mouth disease in tissue culture, using tongue epithelium. This latter method has been used in the production of small experimental lots of vaccine with considerable success and is a project which has a promising future as a means of developing a virus sufficient for vaccine production in the test tube rather than in the animal as is now the case.

Until very recently attempts to grow the virus of foot-and-mouth disease in the embryonated hen egg have not been successful. A recent report from Germany indicates, however, that a strain of virus had been adapted to growth in the embryonated hen egg as a result of considerable experimentation during the war. No confirmation of this work has yet been forthcoming, but it opens a new lead in attack upon the problem of the artificial propagation of the virus of foot-and-mouth disease.

After the outbreak of the disease in Mexico, arrangements were made to have the European laboratories conduct necessary typing tests of the virus prevailing in Mexico and to furnish vaccine for the Mexican program until such time as facilities could be made available for the production of a similar product in Mexico.

The problem of preparing sufficient quantities of vaccine in Mexico was a big undertaking, since it required not only facilities for inoculating large numbers of animals but also extensive facilities for harvesting and processing the virus into vaccine and, finally, the testing of all lots of vaccine for safety and potency before use in the field. Facilities were finally made available in Mexico for the production and testing of vaccine. This work is carried out through the joint Commission by Mexican and American personnel. Starting in May of 1948 small quantities of vaccines were produced. As the facilities increased, the production and testing of the vaccines increased, and at the same time a field organization was established for the vaccination of cattle in the field. Great strides have been made in the vaccination program, and at the present time more than 2 million doses of vaccine are being prepared monthly. Vaccination of cattle, swine, sheep, and goats in the quarantined area is proceeding at a comparable rate. Since it has been found that in some instances vaccination produces an immunity for approximately four months only, the interval between vaccinations is routinely set at four months. Active infection as it is encountered is eliminated through slaughter.

The task of organizing this program so that it functions smoothly is an enormous one. Inspectors in the field in certain areas have a mountainous terrain to work, and others are in a tropical jungle country. The vaccines used must be kept at low temperatures at all times, and the proper transportation of the vaccine under these conditions is a major problem. Airplanes, trucks, wagons, pack animals—all are utilized to move the vaccine to the "front lines." The collection of all animals for inspection and vaccination, the delivery of vaccine at the proper place at the proper time in sufficient quantities, the task of preparing sufficient quantities of potent vaccine—these are factors which must be highly coordinated and integrated. In order to carry out the over-all plan of operations, which is aimed at progressive constriction of the quarantined area, it is necessary to vaccinate the largest number of animals in the shortest possible time. So far the program of vaccination has been favored by the fact that only Type A virus has been found in Mexico. Three types of virus, designated A, O, and C, are recognized in other countries. If other types of virus are found in Mexico, it will be necessary to prepare vaccines against these types, since vaccine prepared from one type does not immunize against others.

The program in Mexico is the largest of its kind that has been attempted in any part of the world. The officials of the Mexican-United States Commission are encouraged with the progress made so far, although it is too early to evaluate the results critically. The outbreak of the disease in Mexico has focused attention on the need for research work on foot-and-mouth disease by the United States in order that we may be better prepared in the event of an outbreak in this country. No research work, as such, on foot-and-mouth disease has been done in the United States in the past, although the Bureau of Animal Industry has been cognizant of the research work done in other countries, and in 1925 formed a commission of three scientists to study the disease in European countries and laboratories. The information that has been accumulated in the past twenty years has been limited, owing primarily to the fact that, in general, large animals (cattle) are needed in experimental work. This is extremely expensive, since many animals are needed, and it is imperative that these be housed in strict isolation for the proper conduct and evaluation of experimental work. Much information is necessary before the

whole problem of foot-and-mouth disease control can be evaluated.

After careful consideration, the Bureau of Animal Industry has taken the stand, which has been supported by the livestock industry of the country, that research on foot-and-mouth disease should be undertaken by the United States Department of Agriculture. Legislation has been enacted authorizing the conduct of such research on a coastal island of the United States. Funds for this work have not yet been appropriated. In the meantime, however, a temporary research program on foot-and-mouth disease is being carried out by the Bureau in cooperation with several of the institutes in European countries, namely, in the Netherlands, Denmark, and England. The Bureau has representatives actively engaged in research work at these institutes. Thus the Department is training personnel in studying foot-and-mouth disease and the various techniques used in experimentation with it. The control of foot-and-mouth disease and research on the problem are receiving attention on an international basis. They have been the subjects of consideration at several meetings of the International Office of Epizootics in Paris, as well as the Food and Agriculture Organization of the United Nations. The accompanying map shows the present infected territory in Mexico and some details of the vaccination program.

The infectiousness of the disease, the damage to livestock, the rapidity with which it spreads, the difficulty of controlling it despite quarantine measures, which in themselves are restrictive and costly, make it a disease of the greatest economic importance. It is estimated that, should foot-and-mouth disease become established in the United States, the average annual monetary loss would be 200 million dollars. Much information has accumulated over the years giving help in the control of the disease. Much has been accomplished through the development of present-day vaccines. A comprehensive program of research will considerably improve this vaccine and the methods of control. The knowledge obtainable will be to a great extent dependent on the amount of research that can be done. The research laboratory proposed by the United States will be the largest of its kind in the world, and here large-scale experiments could be conducted with safety and efficiency. This should give, within a reasonable period of time, many of the answers now needed on the foot-and-mouth disease problem.

AUTUMN IS THE LISTENING TIME

The air is clear
Save for the counterpoint—
The south-winged whooshing
Of the cranes,
The chant of chickadees,
The velvet pat of doves,
The myriad hum of pollen-sowers.
But that's not all;
Within the hollow of the hill
The tractors throat a score
To puffs of powdered earth;
A migrant train
Adds plaint and argument.
But there is gladness in lament.
The spires in the valley's cup
Wear bells
Which now and then
Discuss with merriment and humor
All the virtues on the earth
And in the heavens too.

JOSEPH HIRSH

SMOG AND SONICS

VINCENT SALMON

Born in Jamaica, B.W.I., Dr. Salmon moved with his parents to the United States shortly before World War I. He became a naturalized citizen and was reared and educated in this country (Ph.D., MIT, 1938). In 1946 he received the biennial award of the Acoustical Society of America for his invention of a new family of acoustical horns Dr. Salmon is head of the Sonics Section of the Stanford Research Institute's Physics Research Department.

THE history of man's attempts to subjugate nature is replete with instances of her retaliation for any upsetting of the delicate balance of natural processes. An example of this, often much too familiar to those living near large concentrations of industry, is "smog." Originally the word "smog" was coined to describe a mixture of smoke and fog, such as, for example, that formerly present in a severe form near Pittsburgh and St. Louis; now, however, its meaning has been extended by popular usage to apply to any set of atmospheric conditions in which smoke, fumes, and fog, alone or in any combination, have reached a nuisance level evinced by excessively reduced visibility, with perhaps undue eye, nose, and throat irritation.

Severe smog usually occurs when certain natural meteorological conditions combine with a continuous outpouring of excessively contaminated gases from such processes as combustion of fuels, smelting and refining of metals, production of petroleum, and general chemical operations. The contamination consists of both particulate and gaseous components; the former includes vapors and liquid droplets as well as solid particles. In what follows we shall be concerned with the conditions leading to the formation of smog and the role played by the particulate components. Finally, the removal of these components by sonic means will be considered in some detail.

FORMATION OF SMOG

Normal weather is variable weather—that is, if conditions of temperature, humidity, wind velocity, etc. at any one location remain steady for any length of time, we consider that unusual. There are, however, some steady patterns of meteorological behavior which attain a quasi-stable state for considerable periods, and constitute more or less normal behavior. The example pertinent to our discussion is the predominantly seasonal formation of a semipermanent temperature inversion layer over the western shores of most continents, and occasionally in the interior. Normally, air temperatures should decrease (lapse) as one recedes from the ground, but in approaching an inversion the initial decrease, or lapse, rate becomes slower, finally reversing sharply, then again finally decreasing. This is portrayed in Figure 1; the boundaries of the inversion layer are marked by the changes in slope, which often are as sudden as indicated.

Within the inversion layer the motion of the air is remarkably uniform in magnitude and direction; hence the discharge from a stack that pierces the layer will stretch out as a long plume, which mixes but slowly with the surrounding air. If the smoke is given an appreciable initial upward component of velocity from the stack, it will continue to rise as long as its temperature is above that of the ambient air. As it rises, however, it expands and therefore cools, and thus at the most can go little higher than the top of the inversion layer, when the lapse rate again becomes normal.

Under conditions of excessive atmospheric contamination plus an inversion, material may be collected in the inversion layer and be discernible as a thick haze, particularly if the geography is such as to wall off the escape of incoming winds. Suppose that for various reasons the inversion layer gradually approaches the ground. In doing so it encounters the turbulent surface winds, which thoroughly mix the contamination layers to form the thick blanket we call smog. Conditions are worsened if in addition there is a fairly long period of low wind, low atmospheric moisture content (mixing ratio), and above-normal temperatures. It was probably an acute combination of such conditions that led to the "death smogs" of 1930 in the Meuse Valley in Belgium and of 1948 in

Donora, Pennsylvania. The deaths traceable to these smogs numbered around 63 and 20, respectively, a rather high price to pay for preventable tragedies.

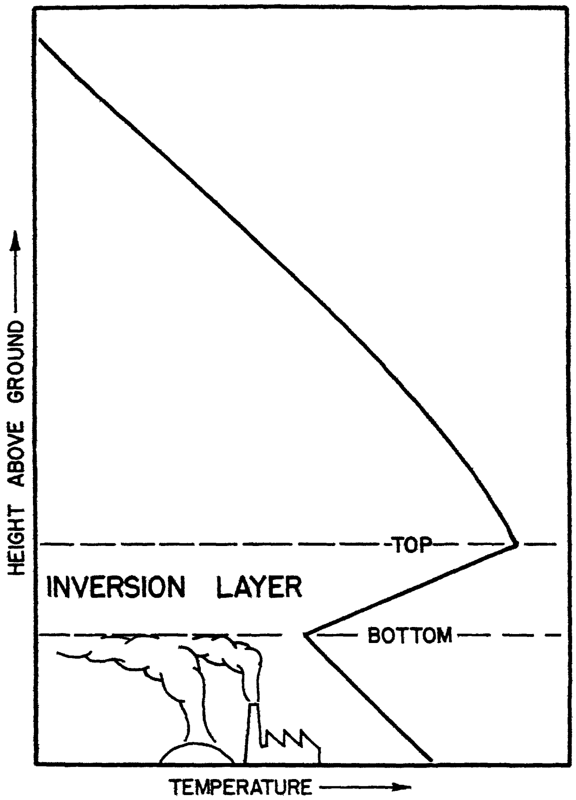


FIG. 1. Temperature-altitude relation with inversion present.

SOURCES OF AIR POLLUTION

Much of our air pollution is due to the smoke arising from the incomplete combustion of soft coal, among such users as householders, power plants, and railroads. Since smoke represents wasted fuel, many economy-minded industrial users have installed more efficient combustion equipment, whose discharge is barely noticeable. Also, the University of Illinois has been instrumental in developing a downdraft soft-coal heater for household use which bids fair to effect considerable reduction of contamination from this source. The best means of preventing combustion-caused pollution is simply the use of better combustion equipment, a fact that is now well recognized.

In the ore-refining, metals-smelting, and heavy-chemical industries, some processes normally result in aerosols comprising fumes and dusts of such fine particles that removal by conventional means is difficult. Often the solid component is the semifinal product, and inexpensive means of collecting the fumes (which are finer than dusts) would not only reduce contamination of the atmosphere, but would also furnish additional income. Considerable thought and effort have been put into this problem for many years, but no universal means of precipitating all types of particles has been found. Rather, each problem must be considered individually, and the optimum collection means found in each case may well turn out to be a combination of several simpler ones. Recent and widespread efforts to alleviate air pollution have led to the search for new methods of removing very fine particulate matter of submicron size from such hot aerosols as stack gases, and have resulted in the rapidly increasing industrial use of sonic methods of glomeration. In this process, intense sound is used to increase the average size of a clump of particles so that they may readily be removed by other means.

COLLECTING MEANS

Let us digress at this point to indicate the generally used methods of clearing gases of particulate components. Classification of the means of separation at present available may be set forth as follows:

1. Separation by a field of force which acts differently on the gas molecules and on the particulate matter.
   - Centrifugal (cyclones)
   - Electrostatic (Cottrell process)
   - Gravitational (sedimentation chambers)
2. Separation by relative motion between gas molecules, suspended particles, and some obstruction with selective action.
   - Oscillatory motion
     - Ordered motion (sonic glomerators)
     - Random motion (thermal precipitators)
   - Unidirectional motion
     - Impingement (impingement scrubbers, packed towers, mechanical washers)
     - Filtering (textile, fiber, ceramic, metal)
     - Bernouilli mixing (Venturi scrubber)

All the means of separation outlined above also involve some glomerating action. Whether by the action of a field of force or by velocity differences, the particles to be removed will experience increased numbers of collisions with one another. Theoretical studies have confirmed the observed fact that colliding particles naturally tend to cohere to form larger ones. This is particularly true in sonic glomeration. As ordinarily used, sonic

glomerators are employed to increase the size of clumps of particles until the minimum size is well within the operating range of another device, whose main function is that of separating and collecting the suspended "flocs," as the groups of primary particles are called.

SONICGLOMERATION

In a discussion of some of the elementary notions about sound, we may first note that the word "sound" refers to both the sensation and to the physical cause. Physically, the presence of a sound wave is shown by the more or less ordered vibration of the particles of the medium through which the wave passes. In most cases of interest to sonic glomeration, we deal with waves that are approximately plane and are of closely sinusoidal wave form. By this is meant that the time variation of gas molecule displacement from equilibrium is a sine curve, and that points having the same phase of motion lie in a plane. We may measure the strength of such a wave by its displacement, velocity, acceleration, temperature change, pressure change, or density change, depending on the work it has to do. Here let us use the maximum displacement, or displacement amplitude, as a convenient measure of the strength. Then if the plane wave exists in a medium with absorption, a plot of amplitude versus distance along the direction of propagation will show a gradual exponential decrease of amplitude, provided that no reflected wave is present. This slow change is shown schematically in Figure 2*a*, and characterizes what is called a traveling wave.

If we now place perpendicular to the wave a large plane piece of material which is an imperfect reflector—that is, it has finite stiffness, mass, and internal dissipation—the reflected wave will then interfere with the direct incident one to produce the amplitude variation pattern of Figure 2*b*, which characterizes a standing wave. Note that the maximum amplitude is not obtained at the reflector, but some distance in front of it; this is due to its finite stiffness and mass. Also, the minimum amplitudes are not zero; this arises from dissipation of energy in the reflector. The gradual change in the minima arises, as in Figure 2*a*, from absorption in the medium. When the reflector is perfectly rigid and the medium still has some absorption, the pattern changes to that of Figure 2*c*, which most closely approaches conditions in a sonic glomerator stack. Here the maxima have been increased, whereas the minima are still dependent on absorption in the medium. The maxima are also affected by absorption, but this is a second-order effect that is not noticeable unless the absorption is quite high.

When a generator of sound works into a column of air in a rigid pipe, the maximum displacement amplitudes are attained when, in electrical engineering parlance, the reaction back on the ultimate source is entirely resistive. This condition is called resonance, and merely increases the amplitude in some regions at the expense of lessened amplitudes elsewhere. In terms of the standing wave pattern of Figure 2*c*, the source must be operating at the correct position and frequency to obtain resonance.

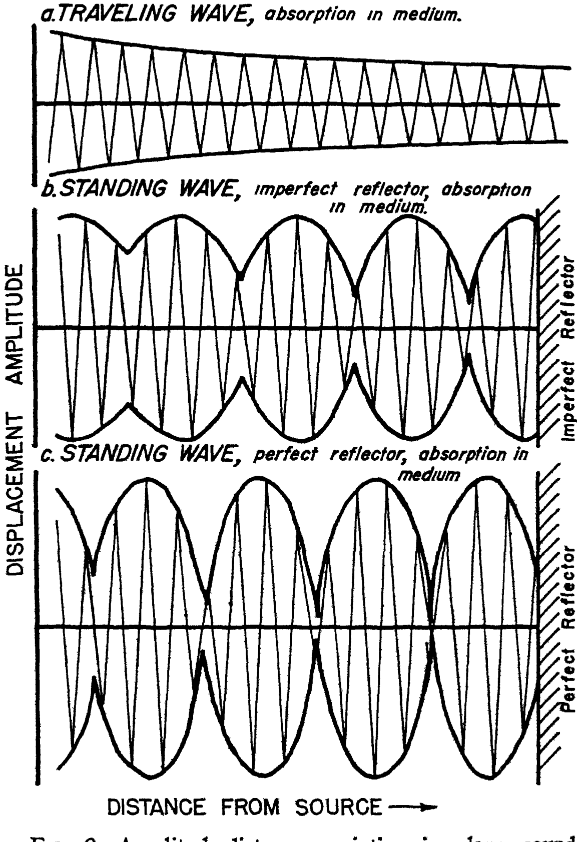


FIG. 2. Amplitude-distance variation in plane sound waves under various reflecting conditions.

It was observed in 1866 by the German physicist Kundt that a fine dust of lycopodium powder lying in a horizontal glass tube would collect and pile up at the regions of maximum gas molecule vibration in an intense standing wave set up in the air within the tube, using a perfect reflector. The effect remained a scientific toy until the 1930s, when

there was renewed interest in the phenomenon. It was known that if an aerosol were introduced into a strongly resonant air column the suspended small particles would glomerate into larger particles, or flocs, which collected at the positions of maximum amplitude. These flocs were found to grow to be so large and heavy that they easily settled out in the absence of any body motion of the gas.

Much work has gone into making this phenomenon the growing industrial tool it is in modern sonic glomerators. An intense source of sound is necessary for the rapid and effective glomeration of aerosols, and is usually a special siren which can interrupt a stream of air at rates from 1,000 to 30,000 times per second. One commercially available model is capable of producing at its mouth a sound intensity of more than 6 watts per square inch. The acoustical engineer will perhaps understand this better as a sound pressure level of about 160 dbp (dbp is the unit of sound pressure level in decibels above 0.0002 dyne/sq. cm). As this is more than 1,000 times the power at which the ear first perceives pain, one of the problems in such installations is adequate sound insulation, especially if the siren is operated in the audible range. Although sound will not be heard if its frequency is above this range, which ends between 15,000 and 20,000 cycles per second, some laboratory workers have experienced fatigue, headaches, and even nausea after considerable and extreme exposure. It must be emphasized, however, that there is no evidence that moderate exposure is permanently harmful; it is known that many susceptible individuals will develop the symptoms simply by suggestion. In any event, it is an easy matter to provide adequate insulation at the frequencies used.

Investigators have observed that as the acoustic power to a glomerating column is increased, there is little effect until a threshold power is attained; thereafter the glomeration rate increases rapidly. For many initial applications this threshold has been found to be near 0.5 watt per square inch. Since the aerosol is usually passed continuously through the glomerating column, the large flocs that form have no opportunity to settle out. Their final removal may be accomplished by utilizing additional means, such as electrostatic precipitators, centrifugal separators, bag houses (cloth bag filters), etc. The maximum acoustic power that may be employed is dictated by reasons of economy; there is no need to make the flocs any larger than necessary for efficient removal by the final separating device.

In Figure 3 are shown the essential parts of a sonic collector in the form at present used. The sound field is set up in a cylindrical stack, all of whose walls are rigid and highly reflecting toward sound. The bottom opens into the final separator, here a cyclone, and the top is terminated by the gas inlet and by the source of sound, usually a siren; these relative positions avoid fouling of the generator. The frequency of the siren is set to provide a fully developed standing wave (resonance) so that the maximum oscillatory velocity of gas molecules is attained. As indicated in Figure 3, this occurs in regions which are half a wave length apart in the uniform portion of the stack; the maximum nearest the bottom will be close to a quarter wave length from the plate closing the bottom. These exact relations are disturbed not only by inhomogeneities in the composition of the aerosol, but also by the presence of the inlet, outlet, and sound generator facilities.

Since absorption increases markedly with frequency, most glomerators work at lower frequencies, well within the audible range. If the aerosol is principally air at 900° F. and atmospheric pressure, then at a frequency of 2,700 cycles per second the regions of maximum particle velocity are about 4 inches apart. Under these conditions a 15-foot stack would have about 40 useful maxima, affording ample opportunity for glomeration. Thus, when the aerosol is finally passed on to the collecting device which follows, the particles are larger and fewer, constituting a favorable condition for precipitation by cyclones and electrostatic collectors. If the stack is 4 feet in diameter, with a sound intensity of 6 watts/sq. in., 12 kilowatts of acoustical energy will be required.

One may ask, Why not employ the final separators (as cyclones) for the whole process? The answer lies in the difficulty of using them economically when the size of the suspended particles is much below 5 microns (0.0002 inch); also, at high aerosol temperatures, the ions necessary to the functioning of electrostatic precipitators may sometimes be discharged prematurely. It turns out that sonic glomeration is most useful for the higher temperatures and the smaller particles of micron range and below. In these categories lie appreciable components of such aerosols as stack gases from carbon-black plants, petroleum refineries, metals-smelting plants, and cement factories; sulfuric acid mists from contact-process acid plants; smoke

from burning soft coal and wood; and many types of fly-ash laden gases. In most cases sonic glomeration supplements rather than supplants the other processes of separation; in each application one must weigh the cost against the necessity and desirability of removing the solid particles. With anti-air-pollution legislation receiving considerably more attention, all means of glomeration have to be considered, and the special uses of the sonic type must be kept in mind.

and density, and particle radius; this increased velocity makes for more collisions per second between particles of different sizes. From theoretical considerations it is known that a very small particle will cohere to a large one on impact, even if it will not cohere to one of its own size. Thus the colliding particles will glomerate until the resulting floc has reached the size at which it is disrupted by the motion of particles past it. The concept of gas molecules dragging along the suspended particles has proved quite useful in initial calculations for relatively low-frequency glomeration.

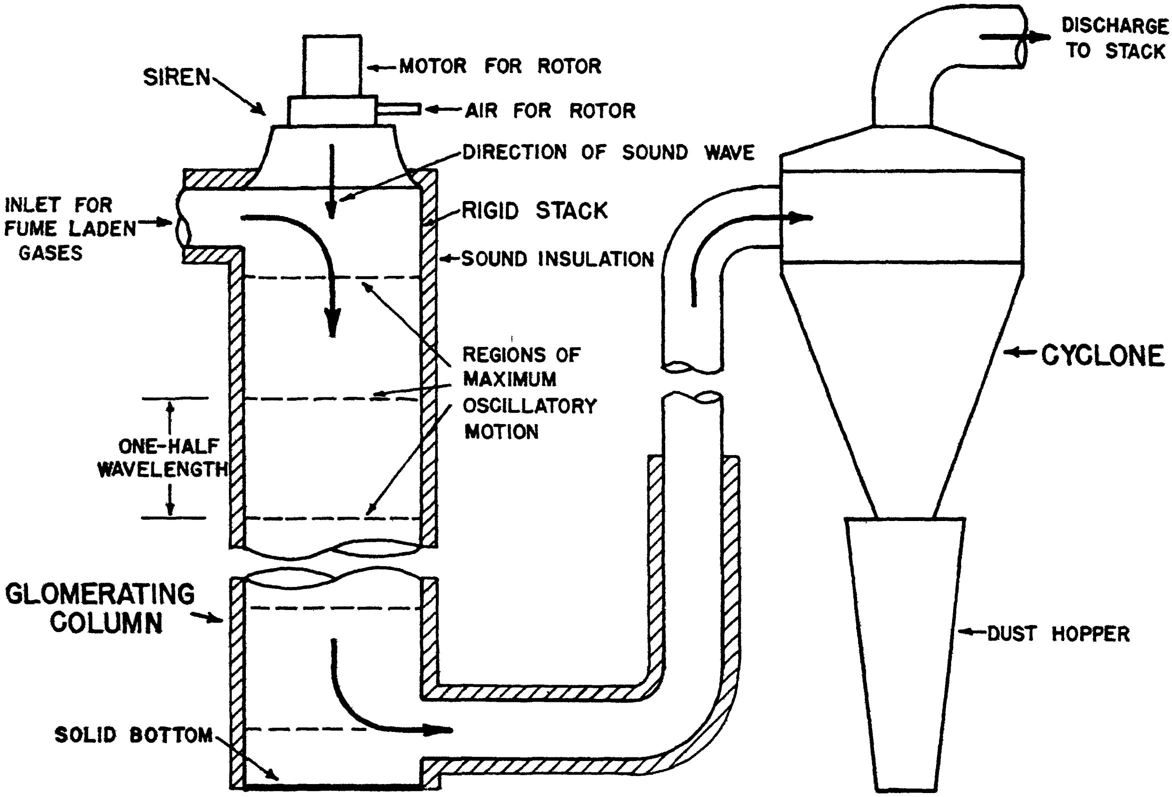


FIG. 3. Essential portions of a sonic dust fume collection system using a cyclone for the final separation.

MECHANISMS OF GLOMERATION

For all its initial successes in industry, we still do not have enough basic information about the sonic glomeration process to predict the performance with a given aerosol, frequency, and acoustic power. Many explanations have been set forth, and it is likely that all the mechanisms play some part. One of the most obvious explanations is that the suspended particles are carried along by the vibrating gas molecules due to the viscous drag between them. Thus the contaminating particles partake of the motion of the gas molecules to a degree depending on the frequency, gas viscosity

To explain the fact that sonic glomeration of extremely fine particle aerosols can take place at relatively high frequencies, recourse is had to three other possible mechanisms. The most plausible is based on the fact that a reflecting surface placed in the path of the wave experiences a pressure known as radiation pressure. This is not to be confused with "sound pressure," which arises from the gas molecules being alternately closer together and farther apart. Rather it is a steady push (with a superimposed "ripple") which tends to urge sus-

pended particles toward regions of maximum motion of the gas molecules. This pressure varies with the frequency, and hence may be expected to be operative mainly at the higher frequencies.

Another mechanism proposed for explaining sonic glomeration calls on Bernouilli forces. These may be simply illustrated by blowing between two sheets of paper held about two inches apart. The far ends of the paper will tend to come together, for they enclose a region in which the air velocity is high compared to that outside, and the Bernouilli law indicates that the pressure in this region will thus be lowered. Similarly, in a resonant glomerating column there will be a force tending to move suspended particles toward the regions of maximum gas molecule motion. The forces evolved are rather too small to be effective, however.

A final suggested explanation for sonic glomeration introduces the concept of vortices, or whirlpools. These may be observed near intense sources of sound; calculations reveal that the vortices should attract one another. This mechanism gives forces that are much too small to explain the observed rapid glomeration, and hence is little used.

All these explanations are still too crude to permit useful calculations of the performance of sonic glomerating columns; they do not explain the threshold effect of a minimum power needed for glomeration, nor is the time factor correctly predicted under all conditions. Much of the discrepancy is due to the assumption that the aerosol contains spherical particles of but one radius; any complete explanation must recognize this as being at variance with the facts. It is likely that no analysis of sonic glomeration will be satisfactory unless the aerosol is considered to be what it actually is—a cloud of gas molecules and solid (or liquid) particles, with a distribution of velocities, sizes, shapes, and cohesiveness specified for each.

The absence of a satisfactory analytical basis for the design of glomerators has not prevented their production and use. Once research has determined the basic relations, however, broadened fields of application and increased efficiency will surely result. Again a scientific plaything is going through the familiar cycle of discovery, research, engineering, and more research, to become a most useful addition to industry's supply of processing tools.

EDISON AND THE INCANDESCENT LAMP

Just seventy years ago this month Thomas A. Edison placed on test an incandescent lamp with a filament of carbonized sewing thread. For the next forty hours, he and his assistants took turns anxiously watching the light's performance. On October 21, when the lamp continued to burn with a clear, steady glow, Edison triumphantly declared it a success.

The lamp was just the beginning, however—a complete system of electric generation and distribution was necessary to make the invention commercially successful. Edison invented and devised the dynamos, conduits, insulators, fuses, sockets, and meters necessary for the system. One of his most important contributions, according to electrical engineers, was the development of the multiple arc system of current distribution, which made efficient and economical distribution possible. Until this was evolved, it was believed that lighting with electricity would be impractical because all lights on a circuit would have to be either lighted or turned off at the same time.

In his search for a satisfactory filament material, Edison tested hundreds of domestic materials, and his agents scoured the world for strange fibers that might serve the purpose. He carbonized everything—paper, threads, and woods, many of them soaked with tar and other substances. In addition, he experimented with thousands of varieties of plants, canes, and grasses. Finally, he discovered that a very long strip cut from the outer edge of bamboo cane had almost parallel fibers and practically no pith, and by July 1880 carbonized bamboo filaments had replaced the paper carbons that succeeded sewing thread.

In 1882, Edison's Harrison, New Jersey, plant employed about 150 men and had a production capacity of 1,200 lamps a day. Today, the annual sales of incandescent lamps amount to $864,800,000 (according to a government survey of 1947 production), and the three major lamp manufacturers employ approximately 30,000 people in the production of incandescent lamps.

Of the 1,097 patents issued to Edison, 356 deal with the incandescent lamp and the distribution of light and power.

HELIUM—THE WONDER GAS*

R. A. CATTELL

Mr. Cattell has been with the U. S. Bureau of Mines since 1921 and chief of the Petroleum and Natural Gas Division since 1933. His special fields are petroleum- and natural-gas engineering and the production of helium.

HELIUM is the lightest member of the family of monatomic gases composed of helium, neon, argon, krypton, xenon, and radon. It is the nearest thing to nothing that will fill space, exert pressure and do nothing chemically. Hydrogen is a little nearer nothing from the standpoint of weight, but it is very active chemically.

During the dust-bowl era in the Texas Panhandle, the late "Tex" Thornton of Amarillo—one of the nation's best-known oil-well shooters—was called upon to discharge some explosives high in the air near Dalhart, in the hope that the disturbance might cause rain to fall. He rigged up some TNT with a detonator and found a balloon of adequate size to lift the explosives—but when it was suggested that he fill the balloon with hydrogen, "Tex" balked. He told Cliff Seibel, of the Bureau of Mines Helium Plant at Amarillo, that he wanted some helium: "I don't like to fool with hydrogen—it's too dangerous."

Helium is completely safe and for that reason is used in many places where hydrogen might otherwise offer a slight advantage. "Tex" Thornton knew that he might lift a little more TNT with hydrogen in the balloon, but that his chances of seeing the results of the explosion would be better if he used helium.

Nothing happened at Dalhart after the explosion; but the next day "Tex" received a telegram from a friend in Roswell, New Mexico, reading, "Thanks, Tex! We had a nice rain down here."

DISCOVERY OF HELIUM

The helium story began in March 1866—less than a year after the assassination of Abraham Lincoln. Then, as now, man's curiosity was a driving force, relentlessly pushing him forward in search of more knowledge about things only partly understood. One of the topics of current scientific interest and discussion was the sun and the bright streamers of light that seem to dance on its surface, occasionally reaching far out into space. Not content to marvel at the beauty of these prominences, man began to wonder what they are.

J. Norman Lockyer, then a thirty-year-old clerk in the British War Office, devoted his spare time to the study of astronomy. He believed the solar prominences to be of a gaseous nature, and it occurred to him that he might determine the composition of the gases if he could examine the light from the prominences with a spectroscope. Kirchhoff and Bunsen had laid the foundation for spectrochemical analysis and astrophysics only five years earlier.

Lockyer was confronted by many obstacles. He wanted to develop a device that would obscure light from the body of the sun and, at the same time, permit him to observe the spectrum of light from the prominences. His funds were inadequate and his progress was slow, but by October 1868 he had an especially constructed instrument suitable for his purpose. With it attached to his telescope, he showed conclusively that the prominences gave bright-line spectra, proving that they were immense volumes of gas composed, in part at least, of hydrogen.

In the yellow part of the spectrum, Lockyer noticed a line that was a little nearer the green than the *D* lines of sodium; and in November he fixed the new line at the point called D_3. Lockyer became convinced that the D_3 line was caused by an element in the sun then unknown on earth, and he called it "helium" from *helios*, the Greek name for the sun.

Although Lockyer was the first to notice the D_3 line, P. Janssen, of France, saw the lines of other elements while he was in India observing a total eclipse of the sun in August 1868. Separate and independent reports by Lockyer and Janssen arrived at the French Academy of Sciences a few minutes apart, and a medallion was issued by the Academy commemorating their analysis of the solar protuberances. The two men became close friends.

Father Angelo Secchi, director of the observatory of the College of Rome, independently recog-

* All photographs from the Bureau of Mines.

nized the existence of a new yellow line in the spectrum of the prominences and thus verified Lockyer's observations. Sir Edward Franklin, the distinguished English chemist, assisted Lockyer in proving conclusively that the D_3 line could not be caused by any element then known on earth, and that it must represent a new element.

Great men sometimes miss. In 1891 W. F. Hillebrand, of the Geological Survey, had the discovery of terrestrial helium within his grasp, but he missed it. Hillebrand obtained an inert gas from uraninite. The spectroscope showed it to be mostly nitrogen, but the spectrum also contained some lines not identifiable with the mapped ones. Hillebrand did not determine their identity. In March 1895 William Ramsay learned of Hillebrand's work. In his search for argon, he extracted a gas from cleveite, removed the oxygen and nitrogen, and examined the residue with his spectroscope. He saw lines of argon, and, to his astonishment, he also saw the prominent yellow line that Lockyer had found in the spectrum of the chromosphere twenty-six and one-half years earlier. Thus Ramsay became the discoverer of terrestrial helium.

AIRSHIPS, RADIUM, AND HELIUM

In 1901 the Brazilian Santos Dumont made his seven-mile flight around the Eiffel Tower in a nonrigid airship, more than two years before the Wright brothers' Kitty Hawk made its first successful flight. In 1902 the Curies, French and Polish by birth, isolated radium. In 1903 Ramsay and Soddy, Scottish and English, found that helium is a product of radioactive disintegration, and in that same year, when the natural-gas development of the Mid-Continent area was in its infancy, some Americans drilled a shallow gas well at Dexter, Kansas. Nobody then dreamed that these four events were related.

The gas at Dexter would not ignite, and visions of great prosperity vanished into thin air. When a farmer who piped some of the gas to the firebox of his kitchen range was asked about its heating value, he replied, "Oh, it and wood together make a good fire." A sample of the Dexter gas was sent to the University of Kansas, where it was found to contain only about 15 percent of combustible gases and about 85 percent of nitrogen and other inerts. In 1905 Cady and McFarland found that the gas contained about 2 percent of helium.

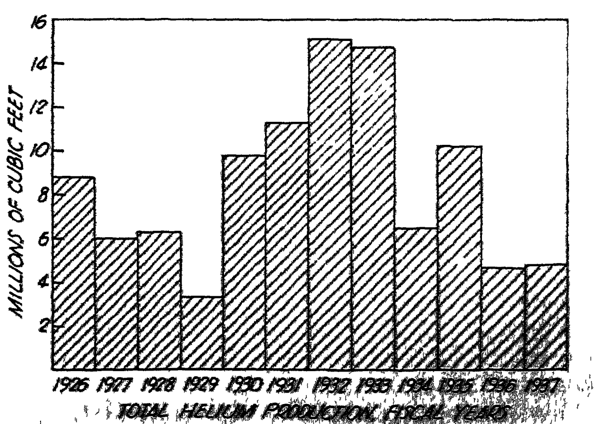
The discovery of helium in virtually nonflammable gas from a small field at Dexter has led to the mistaken conclusion that helium is found only in natural gases of unusually low heating value. On the contrary, nearly all the helium that has been produced in the thirty-two years since the government's helium activities were started has been extracted from natural gas that was being transported to fuel-gas markets. Helium can be extracted economically from poor fuel gases only when the helium content and pressure are very high, the volume is large, and other conditions are favorable. Returns from sale of the natural gas as fuel usually must bear a major part of the cost of developing and operating natural-gas properties to permit recovery of helium at reasonable cost.

All helium-bearing natural gases contain some nitrogen, and some of the gases of unusually high helium content contain large proportions of nitrogen. On the other hand, some natural gases of very high nitrogen content contain virtually no helium.

HELIUM-BEARING NATURAL GAS

Since helium-bearing gas was discovered at Dexter, gases containing 1–8 percent of helium have been found in a number of fields in Texas, Kansas, Colorado, Utah, New Mexico, and other states. The fields suitable for helium production in large volume at reasonable cost are relatively few, and as helium is a minor constituent of gas that is needed as fuel its conservation is difficult. Nevertheless, fields containing more than 7 billion cubic feet of helium supplied gas to five helium plants during World War II, and fields now held as government reserves contain at least 2.5 billion cubic feet of helium.

Helium is separated from natural gas by cooling the gas to a temperature below the liquefaction point of its ordinary constituents, but above the liquefaction point of helium. The gas must first be



Helium production, 1926–37.

treated chemically, however, to remove small quantities of carbon dioxide and water vapor that would solidify in the low-temperature equipment and plug it. After that preliminary treatment, the natural gas is cooled to about – 300° F. in a high-pressure vessel. At that temperature the helium is still gaseous and is drawn from the top of the vessel; the other constituents flow from the bottom as a liquid. This liquid is vaporized in heat exchangers, where it chills the incoming natural gas, and then flows as residue natural gas into a pipe line for transportation to fuel-gas markets.

Helium plant at Amarillo, Texas, went into operation in 1929

The low temperature is attained by expanding compressed and precooled gases, and liquid nitrogen is used as a refrigerating bath in the coldest part of the cycle. For final purification, the helium is compressed to 2,700 pounds per square inch, passed through a gas-liquefaction cycle, then through activated charcoal at the temperature of liquid nitrogen, and discharged into the high-pressure tank cars or cylinders in which it is shipped.

The entire process is continuous. Less than one minute is required to cool any given cubic foot of natural gas to the temperature of liquefaction, remove the helium, warm the residue gas to atmospheric temperature, and discharge it into the pipe line. A plant may process 30 million cubic feet, or about 700 tons, of natural gas in a day.

HELIUM IN WORLD WAR I

The discovery of helium in natural gas opened the way for its production in quantity. In 1917 C. W. Seibel, now supervising engineer of the Bureau of Mines helium plants, was extracting small quantities of helium at a cost equivalent to about $2,000 a cubic foot. The late R. B. Moore suggested that helium might be produced in enough volume for use in airships in World War I. He was right, but the end of hostilities came before large-scale production could be achieved. In July 1917, funds were allotted by the War and Navy Departments to the Bureau of Mines for work on helium extraction, and three experimental plants were built to process gas from Petrolia field, Clay County, Texas. These plants produced about 200,000 cubic feet of helium, and 147,000 cubic feet was ready for shipment to France when the Armistice was signed.

Since the end of World War I, about 775 million cubic feet of helium have been recovered, and its

price at a helium plant is now about 1.5 cents a cubic foot. During World War II, 200,000 cubic feet of helium was produced in less than ten hours.

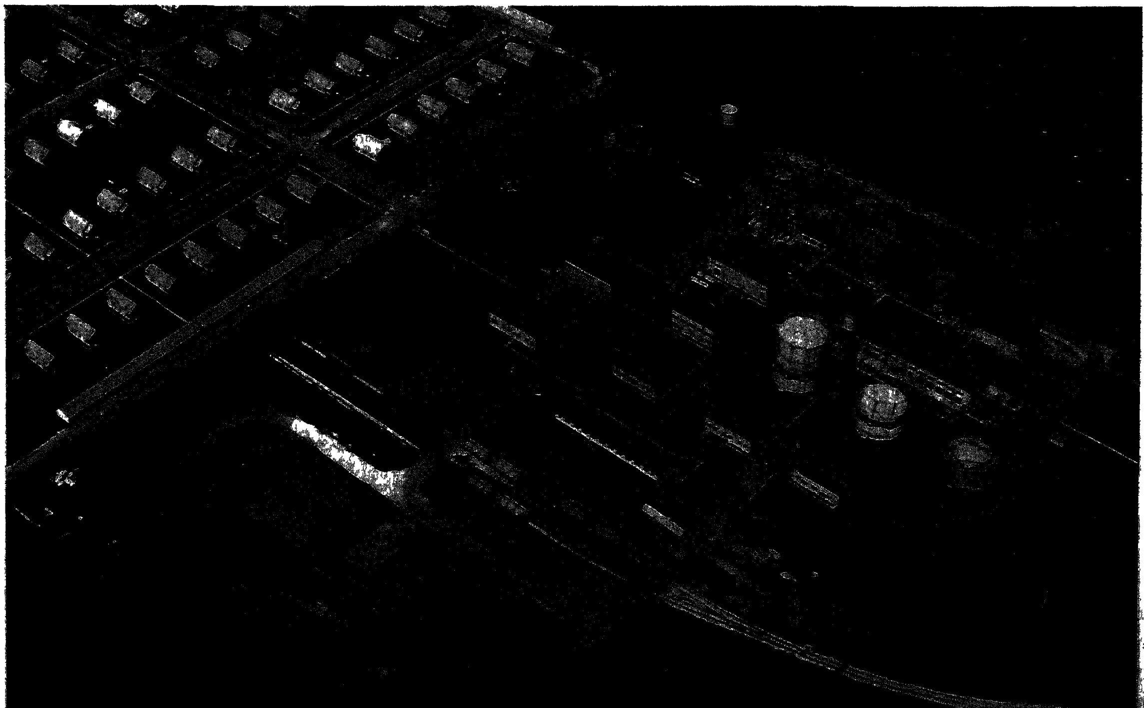
In December 1920, the Navy's C-7 was filled with helium at Bolling Field, and it made the first flight of a helium-filled ship. A full-scale helium plant, built at Fort Worth under the Navy's direction, began to operate in April 1921. On July 1, 1925, that plant and other government activities relating to helium production were placed by Congress under direction of the Bureau of Mines.

Helium production had its ups and downs during the first twelve years of the Bureau's administration. When the Bureau took charge of the Fort Worth plant, the *Shenandoah* and *Los Angeles* were flying, but the *Shenandoah* was lost two months later. Production was on a downward trend because the gas supply from Petrolia field to the Fort Worth plant was declining. Early in 1929, the Fort Worth plant was closed after producing approximately 46 million cubic feet of helium, and a new plant at Amarillo, Texas, was put into operation. The Amarillo plant receives gas with a helium content of 1.8 percent from the Cliffside field of Potter County, Texas, which was brought under full government control by acquisition of all gas rights in 50,000 acres. Annual production at Amarillo rose to about 15 million cubic feet in 1932, but after the loss of the *Akron* and *Macon* the demand dropped to about 5 million cubic feet a year in 1936 and 1937.

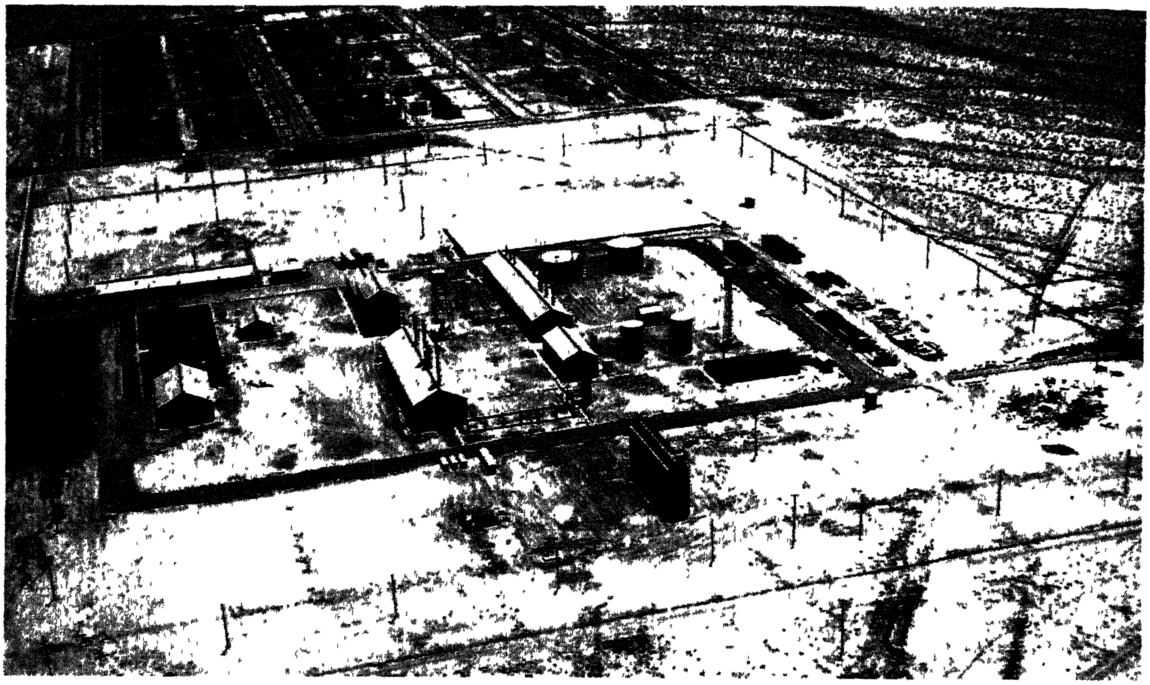
HELIUM IN WORLD WAR II

When Germany invaded Poland in September 1939, the Amarillo plant was producing only about 0.5 million cubic feet a month, or about one quarter of its capacity, and its staff and crew had been reduced to a mere skeleton organization. As the war progressed in Europe, and after France capitulated in June 1940, the helium demand increased about 50 percent. In December of that year, the military services indicated that the volume required to meet their needs might exceed the capacity of the Amarillo plant. In February 1942, the Navy greatly increased its estimate of helium requirements, and three months later the February estimates were doubled when the Navy was authorized to increase its fleet of blimps to two hundred.

In spite of this rapid progression of estimates, the Bureau of Mines met every demand for helium on schedule. At no time were the Army and Navy forced to curtail activities because of insufficient helium production. When it became evident that the Amarillo plant would be inadequate, and funds were made available by the Congress, the Chan-



The Bureau of Mines Exell plant, thirty-two miles north of Amarillo, is the only helium plant now in operation.



Navajo helium plant at Shiprock, New Mexico, completed in 1944, is a Bureau of Mines stand-by plant.

ning area in the western part of the Texas Panhandle gas field was selected as the source of helium-bearing natural gas for a new plant, and Exell, Texas, on the Santa Fe Railroad about thirty-two miles north of Amarillo, was chosen as the plant location. The plant started to operate in March 1943, less than ten months after construction was started; and in July of that year, the Exell and Amarillo plants produced nearly 12,000,000 cubic feet of helium, although their combined rated capacity was only 8,000,000 cubic feet a month.

When the estimated demands rose to 20,000,000 cubic feet a month, additional funds were made available and new plants were constructed at Otis and Cunningham, Kansas, and at Shiprock, New Mexico. The Otis plant came into production in October 1943—nine and one-half months after the start of construction—and the Cunningham plant began to produce in January 1944. From then on the Bureau was able to meet the demand without serious strain on equipment and personnel.

By the time the Navajo plant at Shiprock was completed in March 1944, the German U-boats were under control, and other plants were meeting the helium demand. The Navajo plant was therefore given a test run to prove its efficiency in design and operation and then placed in stand-by status ready to start whenever its output might be needed. Contracts made by the Bureau with the Navajo Tribe of Indians and its lessees reserved a large deposit of gas of 7.5 percent helium content in the Rattlesnake field of San Juan County, New Mexico, to supply that plant.

In December 1943, the Army-Navy "E" Award was presented to the Amarillo and Exell plants for outstanding production of war materials, and the Otis plant received a similar award in March 1945. This recognition, and numerous statements by high military officials, emphasize the importance of helium production in World War II. In the government fiscal year 1944 the production was about twenty-two times that of the fiscal year 1939.

HELIUM TODAY AND TOMORROW

Since Germany surrendered on May 7, 1945, the Exell plant has supplied most of the helium, and it is now the only plant in operation. Its output is about ten times the quantity that was being produced in 1938 and is meeting all governmental and private demands. A 2-inch helium pipe line connects the Exell plant with the Amarillo plant and Cliffside gas field, and when the Exell plant produces more helium than is needed to meet current demands the surplus is injected into the Cliffside field and stored for future use. The cost of producing this helium for conservation is very

low, because a steady plant output can be maintained with substantially the expenditure that would be required for a reduced rate of production. The cost of reclaiming the injected helium will also be low, because it can be recovered in the operation that will be required to extract the original helium content of the Cliffside gas.

The Cunningham plant has been dismantled, but the Amarillo, Otis, and Navajo plants are available for operation whenever demands exceed the capacity of the Exell plant. Since the Amarillo and Navajo plants can be supplied from large reserves of helium-bearing gas now shut in under the Bureau's control, they represent "hole cards" to meet any future emergency demands for helium. The Amarillo plant is headquarters for the field administration of the Bureau's helium activities, including the helium research program. It also is used as a shipping point for some of the Exell plant's output, which is transported to the Amarillo plant through the 2-inch pipe line.

Helium is a unique element, with many extreme properties. At normal atmospheric pressure and 60° F., 1,000 cubic feet of helium will lift 65 5/6 pounds—92 2/3 percent as much as hydrogen. Helium diffuses more rapidly, flows through a hole faster, conducts heat better, and transmits sound at higher velocity than any other gas except hydrogen. It conducts electricity better than any gas except neon.

Helium has a lower solubility in water and other liquids, a lower refractive index, and a lower temperature of liquefaction than any other gas. The rate of ionization of helium when bombarded with electrons is slower than that for any other gas.

Helium is used as a lifting gas in airships and in observation, advertising, meteorological, and toy balloons. Rear Admiral C. E. Rosendahl has said that helium-filled blimps escorted 89,000 surface craft in World War II without loss of a single craft to enemy submarines, although 50,000 of them were in areas where U-boats were known to be present. The stratosphere balloon which carried men to the greatest height they have ever reached was filled with helium. Children have been given much pleasure and protected from injury through use of helium in toy balloons.

Helium-shielded arc welding, developed during the war so that magnesium alloys could be arc-welded successfully, has grown to assume a role of major importance in peacetime. Using helium as an inert gas shield around a tungsten electrode, industry has found that it can arc-weld aluminum, copper, stainless steel, titanium, and other metals without the use of flux or extensive prewelding preparation of the metals or alloys. The inertness of helium also is used to advantage in degassing molten metals, and in preventing oxidation or other chemical reactions in various metallurgical processes.

Helium production, 1938–48.

Helium is used with oxygen in breathing atmospheres for divers and caisson workers. Because of its low solubility in blood and body tissues, helium mitigates the painful and sometimes fatal caisson disease. This illness, known to divers as "the bends," results from the formation of nitrogen bubbles in the blood when men are removed from an atmosphere of compressed air to normal atmospheric conditions. Helium was used in the deepest dive ever made in a diving suit.

Because of its lightness and high rate of diffusion, helium is used with oxygen in breathing atmospheres for asthma patients and persons suffering with other respiratory difficulties. More oxygen can be breathed into the lungs with the same muscular effort.

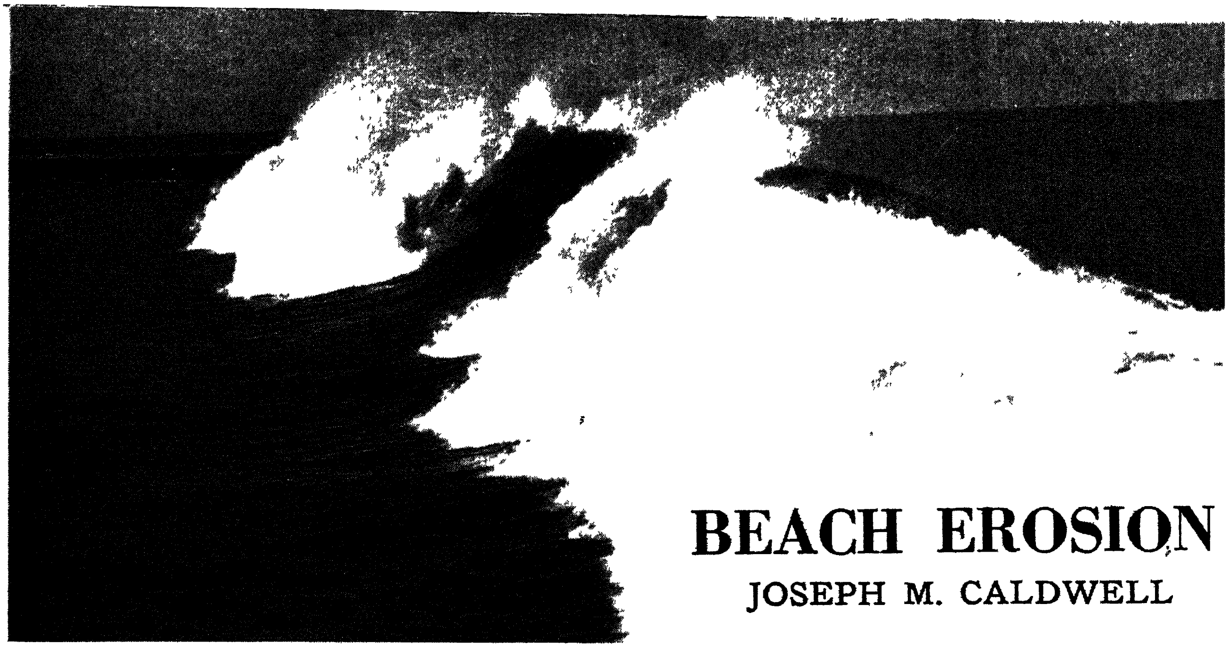
Because its refractive index is very nearly unity, helium is used to fill spaces between lenses in optical instruments. It is mixed with otherwise explosive anesthetics to make them safe in operating rooms. It is used as a tracer material in the detection of leaks in vacuum and pressure vessels, and to determine the underground movement of injected natural gas in oil and gas reservoirs.

Helium is no longer a laboratory curiosity. It is produced in large quantities at high purity and reasonable cost. The Bureau of Mines believes that its growth as an industrial tool depends largely upon the rate at which knowledge concerning its properties, cost, and availability can be disseminated. To this end, the Bureau is conducting a modest research program aimed in three di-

rections: To make available to the American people the purest possible grade of helium at the lowest possible price; to develop known uses and open up new fields of usefulness for helium; and to assure, through conservation of known resources and a continuous search for new resources, that helium will be available to the American people for the longest possible time.

Thus helium, the sun gas, which has carried man into the air, helped him to dive deep into the sea, given pleasure to his children, relieved human suffering, and opened up new fields in science and industry, is now being produced in sufficient quantities to meet all present demands and is being studied and conserved to meet whatever needs the future may bring.

Welding magnesium sheet with a helium-shielded arc.



BEACH EROSION

JOSEPH M. CALDWELL

A graduate in electrical engineering of Mississippi State College, Mr. Caldwell has been chief since 1946 of the Laboratory Section, Engineering and Research Branch, of the Beach Erosion Board in Washington, D. C. Most of his previous experience was with the Waterways Experiment Station, Vicksburg, Mississippi. As a captain in the Army he did intelligence work in the Office of the Chief of Engineers

TO MILLIONS of Americans, the sand beaches of the Atlantic, the Pacific, the Gulf of Mexico, and the Great Lakes represent vacationland. Most of these people probably do not realize that a continual battle goes on to protect these beaches from the erosion caused by the almost ceaseless attack of the wind-generated waves working in conjunction with tides and currents.

A basic factor in beach erosion is the fact that in most localities a rather delicate balance exists between the forces tending to erode the beach by carrying the sand away and the forces tending to move sand onto the beach from other areas. Usually, both sets of forces—those of accretion and those of erosion—are present, and it is the difference between the two that determines whether a beach is eroding or building up (Fig. 1). Where the difference is slight, a fairly stable beach configuration is evident, although after a considerable period—say, fifteen or twenty years—the cumulative effect of the dominant force becomes apparent; where the difference is large, the beach may show radical changes in a year or two.

Any unusual conditions, whether natural or man-made, may upset the balance in such a way that what has been a very stable beach may quickly show significant erosion or accretion. For example, the hurricanes that at times sweep the Atlantic and Gulf coasts of the United States frequently produce pronounced changes on the affected beaches. The construction of the yacht harbor at Santa Barbara, California, so upset the balance in that area that the beaches for some ten miles to the east of the harbor were denuded of sand, sand is now pumped past the harbor periodically in order to restore and maintain these beaches.

The cumulative effect of wave action on a specific beach is somewhat dependent on the type of shoreline in the area. The shoreline of the United States is characterized by a diversity of shore forms, the New England shoreline being rather rugged, with cliffs and headlands fronting directly on the ocean and with rather short lengths of beaches caught between the headlands. The Atlantic shoreline from Long Island south, and the Gulf beaches, are mainly characterized by long, uniform beaches, with the adjacent topography lying only a few feet above high tide; often these beaches are in the form of a barrier beach separated from the mainland by a salt-water marsh or lagoon The Pacific shoreline shows some of the characteristics of both types of shorelines, with long sweeping beaches broken by bold headlands.

The geologist recognizes the material forming the beaches as being largely derived directly from the rocks that originally formed the earth's crust. These rocks were of various chemical and mineral composition and physical characteristics and have gone through changes due to environment, so that many of them have been greatly altered.

For the moment, however, we are primarily interested in beaches, and we find that after all the weathering, frost action, glaciation, hydration, etc.,

of these rocks and minerals there is only one type which exists in significant quantity and which is able to withstand nearly all the so-called destructive forces that nature can impose upon it. This is the mineral silicon dioxide, commonly known as quartz. The composition of practically all our beaches is predominantly quartz sand; small quantities of other rocks and minerals are usually present, but for the most part they have been ground to powder, gone into solution, or otherwise been removed from the picture.

The quartz sand reaches the beach environment by several means. Gravity may carry it from a cliff face into the zone of wave action. Winds may fashion dunes that migrate to the water's edge. Glaciers may leave moraines and drumlins along a coast, and in nearly every environment streams dump sandy detritus at their mouths. Once the sand has reached the coast, it is subject to further movement by a repetition of these transporting forces. Significantly, the effect of ocean waves on the spreading out and rearrangement of the beach components becomes a dominant factor when they come within reach of these waves

TYPES OF SHORELINES

The geomorphologist generally classifies the shoreline as being one of "submergence" or of "emergence" and goes on to show that this classification enables us to explain certain characteristics of the shoreline and predict what its future will be. For the most part these predictions are made on the basis of "geological" time and involve time intervals of the order of hundreds or thousands of years.

The terms "submergence" and "emergence" are more or less self-explanatory. A shoreline of submergence is one where the land mass has subsided with respect to mean sea level. In this case valleys are submerged, and interstream ridges rise sharply above irregular embayments, forming rocky headlands that are faced with wave-cut benches, with intermittent stretches of beaches caught between the headlands. The cumulative attack of waves and weather on these rugged shorelines wears away the irregularities, and will ultimately bring about a fairly straight shoreline characterized by beaches backed by a prominent cliff line. The New England coast is an excellent example of a shoreline of submergence in an early stage, whereas the Oregon coast is a more maturely developed shoreline of submergence.

The term "shoreline of emergence" signifies that the beach is rising with respect to mean sea level. As most underwater areas are fairly level, emergence exposes a gently sloping marine plane, upon which waves, breaking in water 10–30 feet deep, will pile up a barrier beach or offshore bar, leaving a lagoon between the barrier and the mainland. These barrier beaches are commonly cut through at irregular intervals by tidal inlets which connect the ocean and the lagoon The shoreline of New Jersey is an example of a shoreline of emergence The mature stage of a shoreline of emergence is reached when the lagoon has been filled, partly by the barrier beach being forced back into it and partly by normal sedimentation in which clastic debris and vegetation may take part.

WAVE ACTION

Various forces act upon the beach material, a dominant force being the wind-generated ocean waves. The height of waves generated over the open ocean by the wind is controlled by three factors: the wind velocity, the duration of the wind, and the "fetch." The fetch is the length of

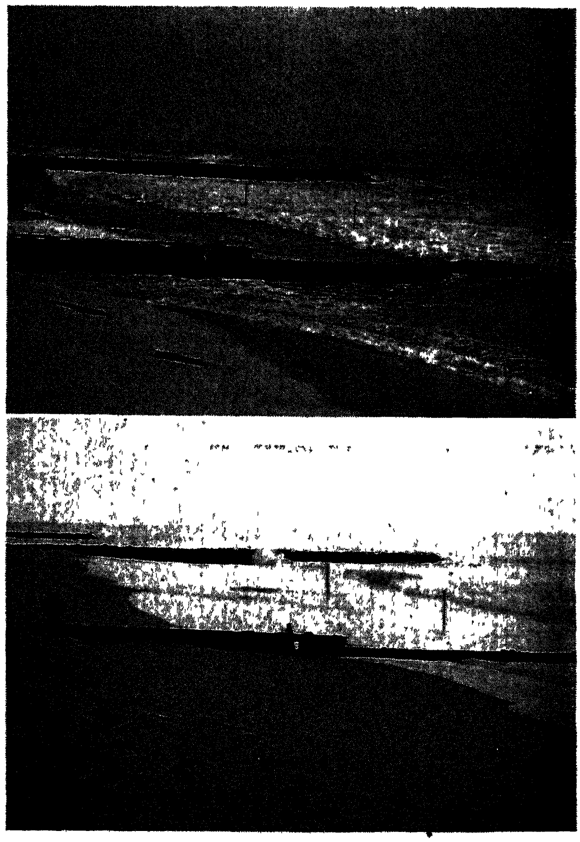


FIG. 1. Two views of shoreline near Long Branch, N. J., showing change in beach elevation over period of one year. This accretion represents more or less normal fluctuation in the beach elevation from year to year.

the stretch of open water actually in contact with a specific wind development.

Once these waves have been generated by a wind disturbance, they may leave the area of the disturbance and travel for hundreds or even thousands of miles before being interrupted by a land mass. In fact, most of the waves seen along our ocean beaches have been generated by distant storms.

There are several distinct classifications of water waves, among them progressive, stationary, and solitary waves. So far as beach action is concerned, we shall confine this portion of our discussion to a description of progressive oscillatory waves. These are waves that show a progressive movement of successive crests in a single direction, whereas the water particles themselves move in an essentially circular path when in deep water. In effect, this means that the wave form moves forward, but there is little or no progressive forward movement of the water particles. This is easily demonstrated by the fact that a cork thrown on the surface of the sea oscillates back and forth in the area of introduction although the wave crests move through the area in rapid succession. The waves themselves are described by the "length" from crest to crest, the "height" from trough to crest, and the "period," which is the time interval between the arrival of successive crests at a stationary point.

As the wave moves into shallow water, the bottom begins to affect the form and mechanics of the wave. First, the circular paths of the water particles are gradually changed into elliptical paths, and the height and length of the wave are altered somewhat. Finally, the wave reaches a depth so shallow that the mechanics of the fluid motion make it impossible to transmit the oscillatory wave form any further; at this point the wave combs over and breaks.

In order to obtain a clear picture of these ocean waves, let us consider them from the time of their inception in the open sea until they destroy themselves by breaking on some distant shore. Suppose a meteorological disturbance over the North Atlantic generates a wind with a velocity of 26 knots blowing for some twenty-four hours over a fetch of 300 nautical miles. Empirical relations that have been established show that these conditions would generate a train of waves that would leave the storm area with a height of about 15 feet, a length of about 300 feet, and a period of about 7.5 seconds. One way of describing such waves is by length-height (L/H), or steepness, ratio. In the present instance, the ratio would be 20. The mechanics of the fluid make it impossible to get a L/H ratio of less than about 7.

As the waves leave the generating area they begin to lose energy, the principal loss being due to atmospheric resistance. This results in a change in the shape of the average wave, specifically a decrease in height and an increase in length. Let us suppose that the 15-foot waves in the above example leave the generating area and travel over some 2,000 miles of open water before reaching a shore. This 2,000 miles is called the "decay distance," and studies show that at the end of this decay distance the selected waves will have decreased in height to 2.5 feet, increased in length to 1,300 feet, and lengthened in period to 16 seconds. The waves have now become low swells, which are the waves usually to be seen on our beaches during fair-weather periods.

The L/H ratio of these waves has now become 520. A large L/H ratio is characteristic of swells; in fact, this L/H ratio is one method by which ocean waves are classified. For general purposes, we can assume that L/H ratios between about 7 and 35 characterize storm waves (waves in or near their generating area), ratios between about 35 and 70 characterize intermediate waves, and L/H ratios greater than 70 characterize well-developed swells.

During World War II, work on the prediction of waves received considerable impetus owing to the many military amphibious operations that involved the use of small craft in open waters. Studies were made to correlate wave height and wave length as a function of the velocity, duration, and fetch of the wind. The change in wave characteristics during travel from the storm area to the beach was also studied. As a result of these studies, it has been possible to prepare charts showing the interrelation of these factors (Figs. 2, 3).

If we study one of the 2.5-foot swells as it comes into shallow water, we find that, so far as the wave is concerned, the water becomes shallow when the depth is equal to about one half the wave length. At this depth we say that the wave "feels" the bottom, and the motion and form of the wave start to adjust themselves to this new condition. Generally speaking, the effect is to increase the height and decrease the length. Also, the internal motion of the water particles changes from circular to elliptical, with a noticeable increase in the velocities at the bottom. In fact, these bottom velocities finally become of sufficient magnitude to roll the sand particles back and forth on the bottom, sand ripple formations result, and some of the sand may be thrown temporarily into suspen-

sion. Once the sand particles are in motion, any alongshore currents may cause a corresponding alongshore movement of the sand particles.

As the wave moves into even shallower water, it finally reaches a depth where the wave crest can no longer be supported, and the wave breaks. This breaking is accompanied by great internal turbulence and causes large quantities of sand

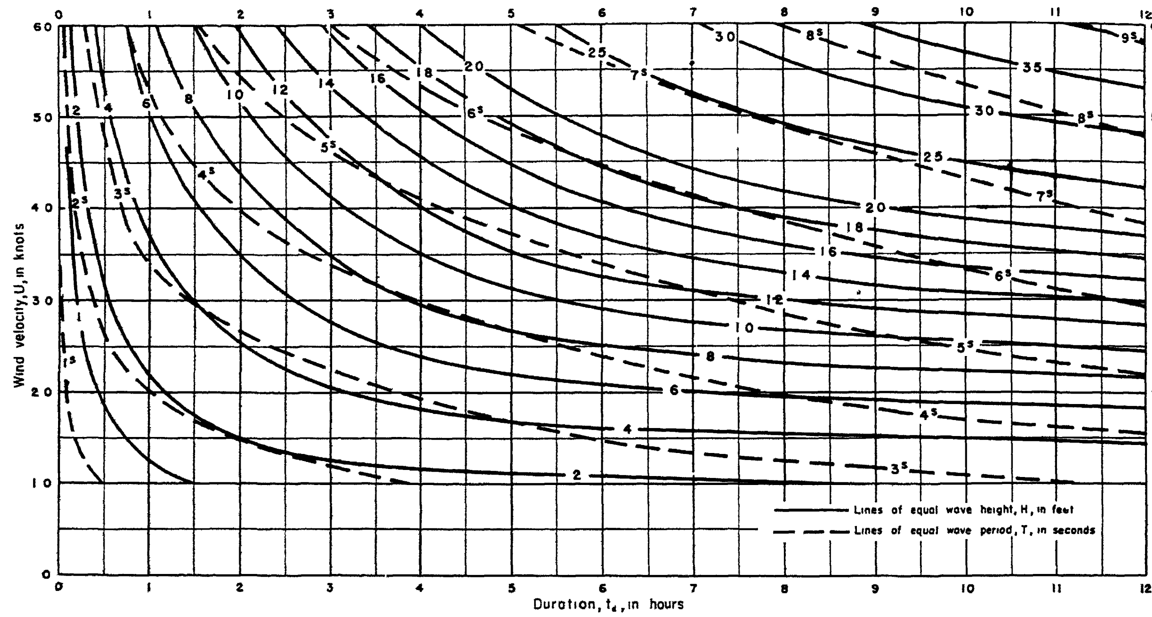


FIG. 2. Wave height and period as functions of short duration of wind and wind velocity (Chart prepared by the University of California for the Bureau of Ships; used by courtesy of the *Bulletin of the Beach Erosion Board.*)

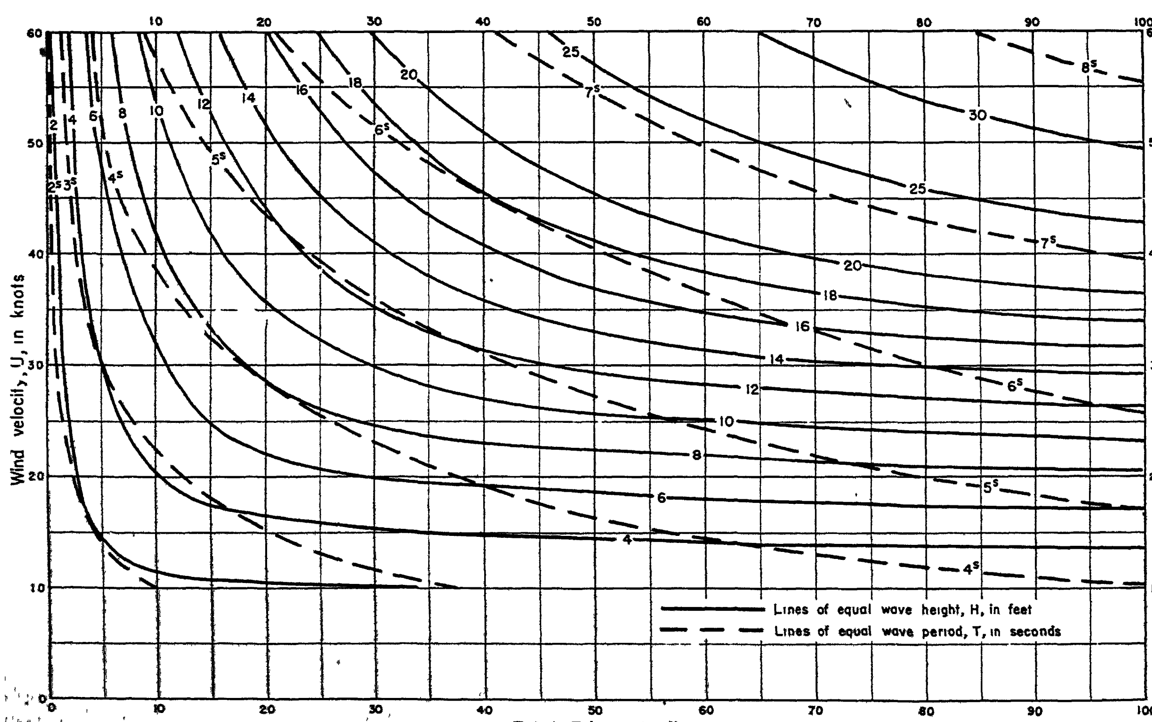


FIG. 3. Wave height and period as functions of short fetch and wind velocity. (Chart prepared by the University of California for the Bureau of Ships and used by courtesy of the *Bulletin of the Beach Erosion Board.*)

to be thrown into suspension. This breaker zone is generally the most active zone from the standpoint of action of the sand particles forming the beach. Hydrodynamic relations show that the 2.5-foot swell will have increased in height to about 5 feet by the time it reaches a water depth of 9 feet, at which depth it will break. On gently sloping beaches, the wave may re-form, only to break again in even shallower water. On the steeper beaches, the broken mass of water will rush up on the beach and return to the sea as backwash.

In nature waves do not travel as simple, uniform wave trains. The picture is usually rather confused, with what might be described as a spectrum of waves present, some high, some low, and of varying length. In deep water the longer waves travel the faster and overtake and move through the shorter waves. Descriptions of the state of the sea usually include only the dominant waves present, whereas an accurate description would involve a breakdown into all types.

A very important feature of wave action on beaches is angle of approach. In deep water waves may approach a beach at almost any angle As they move into shallow water, however, they are refracted in such a way that they tend to adjust their crests parallel to the bottom contours. Thus, a wave approaching in deep water at an angle of 45° to the shore may have an angularity of only 5° or so by the time it finally breaks on the beach. Even this slight residual angularity generally has the effect of setting up a littoral current alongshore in the direction of the angularity. This littoral current, in turn, frequently becomes a dominant feature in beach erosion and accretion.

It might be well to mention the contrasting action of storm waves and swells on the beaches. Generally speaking, the swells tend to move sand from offshore and deposit it on the beach; on the other hand, waves resulting from local storms tend to tear the beach down by removing sand and leaving it in submerged bars offshore. (These two statements are rather generalized; other factors, such as beach slope and sand size, could invalidate them somewhat.)

TIDES

Tidal action also influences the characteristics of a given beach in that, as it rises and falls, the tide shifts the zone of wave attack up and down the beach. The tide may also set up tidal currents that can affect the movement of sand on the beaches.

The magnitude of rise and fall of the tide—i.e., the tide range—varies from day to day at any given locality; it varies noticeably from place to place around the shores of the United States, averaging 4.4 feet at New York, 1.3 at Key West, 4.2 at San Diego, and 7.6 feet at Seattle. In general, the tide follows the moon, and there are two high tides and two low tides each lunar day of 24 hours 50 minutes; thus highs and lows follow each other at intervals of about 6 hours 12 minutes. There are some exceptions to this, particularly along the Gulf Coast in the vicinity of Pensacola, Florida, where there is usually only one period of high and one of low water in the lunar day of 24 hours 50 minutes Of course the tides are also influenced by the sun, although the tide-producing potential of the moon is roughly about 2.25 times that of the sun

The principal variation in the tide is due to the phase of the moon with respect to the sun. Thus, at times of full moon and new moon, the sun and the moon are working together and produce a series of tides of relatively large range, called spring tides, which may have a range of 1 5 times the average. At times of the first and third quarters of the moon, the sun and moon are working in opposition to each other and produce a series of tides of relatively small range, called neap tides. Spring and neap tides follow each other at intervals of about 7 1/3 days. It is recognized that if storm waves reach the shore coincident with high water during a spring tide, the potentialities for damage to the beach and its structures are much greater than usual.

Tidal currents may generally be seen along the coasts only in the vicinity of bays, estuaries, or inlets. The rise and fall of the tide produces a flood and ebb of the current filling and emptying the estuary. This may reach sufficient magnitude in the vicinity of the estuary to cause erosion by its own action. The effect of the current is felt over a much larger area, however, where the currents are too small to effect the movement of the beach sand by themselves but where they can produce a significant action by working in conjunction with the waves breaking on the beach. Some streams and rivers have so much fresh-water discharge that this is more important than the tidal flow, and the fresh-water currents may affect the beach in the same manner as the ebb flow of the tide.

SAND MOVEMENT

The net effect of the forces described is to produce a beach that may be accreting or eroding, may show cyclic or seasonal changes, and is seldom if ever stable. In effect, the entire beach surface

is generally in motion. It might be pictured as a river of sand whose direction and velocity are determined by the character of the forces impressed upon it in the form of winds, waves, and currents.

Possibly the critical point to remember is the fact that once a sand grain is put into suspension, the slightest movement of the surrounding water will produce a corresponding movement of the sand particle. As long as the particle remains in contact with the bottom, currents of one foot per second or more will be required to move even the fine sand. Almost unceasing wave action on the beaches, however, continually keeps a substantial amount of sand in suspension

If the waves approach the beach from the perpendicular, the sand movement is essentially an oscillation back and forth of the particles, with little progressive movement parallel to the shore. If even a slight littoral current is present, however, the movement of material can be relatively great. As already pointed out, these littoral currents can be generated by waves approaching at an angle to the shore, by tidal currents, or by the fresh-water discharge of near-by rivers.

The distinction between the littoral current and the littoral drift should be noted. The current is the movement of the water mass; the drift, the movement of the solid particles. The coast of Santa Monica Bay in the vicinity of Santa Monica, California, is considered to have a fairly rich littoral drift, the net direction of drift being from north to south. Here the average net rate of drift is some 800 cubic yards per day, or a rate of some 300,000 cubic yards per year. A rate of 50–100 cubic yards per day is usually spoken of as a lean drift, although the terms "rich" and "lean" are used relatively.

A

B

C

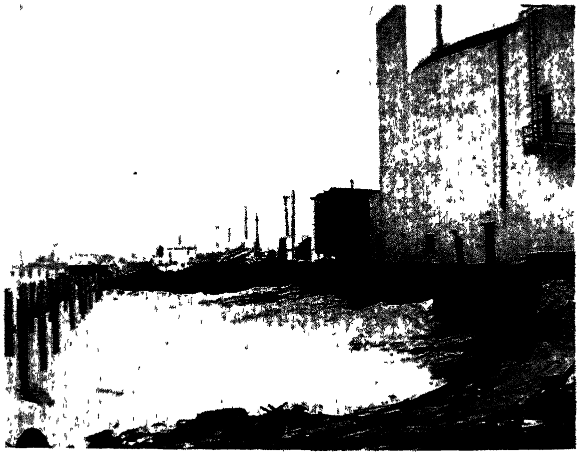


FIG. 4. Views of Redondo Beach, Calif., showing severe erosion resulting from construction of yacht harbor breakwater. (*a*) Condition of beach immediately prior to completion of breakwater. (*b*) Condition seventeen months later. Note top of theatre in upper right background. (*c*) Condition of beach six years later. Erosion has wiped out block of buildings between the theatre and original shoreline.

On a beach where the drift is dominantly in one direction, it can readily be seen that any interruption in this drift may produce significant changes in the adjacent beaches. Suppose an impermeable structure such as a groin is built out from such a beach. The result will be an erosion of the beach downdrift from the structure and an accretion of the beach updrift. In cases where a rich littoral drift exists, this erosion-accretion picture will probably develop very rapidly, and wherever this erosion takes place in a well-developed commercial or residential area severe damage may result in a period of three or four years (Fig. 4). Of course, once the sand-impounding capacity of the structure has been reached, the flow of sand past the structure is re-established.

Frequently, we find that the natural conditions at a beach are not in stable adjustment and that progressive erosion of the beach is under way. In such cases, the eroding condition can sometimes be arrested by the construction of a groin field to check the natural rate of removal of material from the beach. Even under these conditions consideration must be given to adverse effects downdrift from the groin field. Groins of many lengths, shapes, and sizes have been utilized by various agencies in an attempt to discover the most effective type.

The fact has been mentioned that, generally speaking, waves from a local storm tend to tear the beach down, whereas swells from distant storms tend to build it up. This alternate building up and tearing down of the beaches usually causes a shifting back and forth of the water line. In well-developed areas this shifting may be undesirable, and in some cases cannot be tolerated. To forestall such shifting of the water line, particularly its shifting shoreward, reliance is sometimes placed on the construction of a beach bulkhead placed a short distance shoreward of, or near, the mean water line. These bulkheads, or seawalls, are usually of heavy construction, and some types have their seaward toe protected by installations of large stone or rock A tendency to economize on these designs and a lack of understanding of the potential destructiveness of wave action have resulted in many costly failures of this type of protection. A workable solution is often the building of a beach bulkhead in combination with a groin field.

In addition to the various forces previously described, wind blowing over the beach can be an effective agent in moving the sand, and should always be considered when making a study of a specific locality. The large sand dunes along many of our coasts testify to the effectiveness of the wind.

In their attack on our shores, the waves of the sea present a worthy foe to tax the knowledge and ingenuity of our engineers and scientists. The agency charged with handling Federal interests in beach protection, including cooperation with states and municipalities, is the Beach Erosion Board of the Corps of Engineers, Department of the Army. Many of the states with beach erosion problems have, in addition, special agencies set up within the state government to deal with their own problems.

THE GREAT EXPERIMENT

EARL P. STEVENSON

A graduate of MIT in chemistry in 1918, Mr Stevenson became director of research for Arthur D. Little, Inc, the following year. He has been president since 1935. His article is based on an address given at Wesleyan University, April 23, 1949.

IT IS not sufficient today merely to define science as a body of knowledge and a method of apprehending experience. Science, and its expression in technology, has entered too deeply into the consciousness of the layman and into the formation of his daily life—especially here in America. Science is a national concept and one of as much concern to every citizen as the state of agriculture or industry. The recent world war, as the public well knows, was fought not only with men's bodies and lives on the fronts, but with the brain of the scientist and the experience of the engineer. It was Hitler's boast, at the beginning of the war when events went so swimmingly for the German armies, that to keep one man fighting at the front two men worked in the laboratories and factories at home. It is certainly in some measure due to the fact that we in America were able to increase that ratio manyfold that the tide of war turned for us.

The atomic bomb was but one contribution of the scientist during the war, although naturally a particularly impressive and terrible one Dr. Robert Oppenheimer, whose name we instinctively associate with the development of the atomic bomb, has himself laid far more stress upon the importance of the development of radar. But it is no wonder that, as Lyman Bryson has said in *Science and Freedom*, ". . . there is now, in the superficial layers of the public mind, a momentary revulsion against science." It is admittedly difficult for the average citizen to understand the language and even the mentality of the scientist. He must judge the scientist by what the scientist has produced: the atomic bomb. And contemplation of the atomic bomb does not summon as quickly to mind that question tapped out by Samuel Finley Breese Morse, introducing an earlier power to the world with "What hath God wrought?"

I

In the face of this public concern, the Federal government is engaged in a great experiment—the official underwriting of scientific research. Those who follow the progress of bills before the Congress are no doubt cognizant of the fact that a National Science Foundation Bill has, after a stormy passage, been approved by the Senate. This bill will

> create a foundation to serve as a permanent central scientific agency of the Federal Government and both specifically [authorizes] the foundation to support financially and promote research in the fundamental sciences, national defense, medicine and health, to expand the nation's scientific talent through the granting of scholarships and fellowships, and to foster the interchange of scientific information among scientists [It will] provide for an organizational structure for the foundation (*a*) which will permit the maximum participation by the Nation's scientists in formulating the Foundation's program, both as full-time Government employees and as part-time consultants, and (*b*) which will fix responsibility for the execution of the program in officials directly responsible to the President and to the Congress.

The Foundation will be an independent agency of the Federal government, and will be managed by an administrator appointed by the President. The administrator will appoint a director to head each of eight to ten divisions of the Foundation, and he will consult with a National Science Board whose members will be appointed by the President to three-year terms of office. And with the establishment of the National Science Foundation the distrustful American public itself will embark on a science program.

The recognition of science as an essential element in national life has been slow but steady. It is difficult nowadays to realize that for our forefathers theories and facts of science—and engineering—which we take for granted simply did not exist. When James Watt and his partner, Matthew Bolton, first set up a small bead factory, they found the river they were using for power inadequate, whereupon they bought a steam pumping engine—then a very new device—*to pump the water upstream from below the dam*, so that they might use the water power twice over. That seems to us today absurd. We would harness the steam engine to a shaft and utilize the engine's power directly. We cannot realize that the mechanical movements

essential to the transmission of power from engine to work were unknown barely a century ago.

The early dabblers in science, the alchemists of the Middle Ages, seem to have been motivated in part by the idea that their work would lead to something of immediate practical value. Bacon visualized the "House of Salomon" for the investigation of nature by observation and experiment to attain "the knowledge of causes and secret motions of things, and enlarging of the bounds of Human Empire, to the effecting of all things possible." Yet we have had to wait for the turn of this century to find established the present relationship between science and technology alone. The following story has been told of the great mathematician Hilbert, who

would have liked, had the world let him, to have thought of his science as something independent of worldly vicissitudes Hilbert had a colleague, an equally eminent mathematician, Felix Klein, who was certainly aware, if not of the dependence of science generally on society, at least of the dependence of mathematics on the physical sciences which nourish it and give it application. Klein used to take some of his students to meet once a year with the engineers of the Technical High School in Hanover. One year he was ill, and asked Hilbert to go in his stead, and urged him, in the little talk that he would give, to try to refute the then prevalent notion that there was a basic hostility between science and technology. Hilbert promised to do so; but when the time came a magnificent absent-mindedness led him instead to speak his own mind: "One hears a good deal nowadays of the hostility between science and technology. I don't think that is true, gentlemen. I am quite sure that it isn't true, gentlemen. It almost certainly isn't true. It really can't be true. *Sie haben ja gar nichts mit einander zu tun.* They have nothing whatever to do with one another."

The war did much to improve the working relations between the scientist and the engineer. In many instances each was forced to delve deeply into the domain of the other. The gain in mutual respect was reflected in effective teamwork and accelerated programs. Heretofore neglected areas of scientific knowledge were searched for data essential to a difficult design problem—even anthropologists were employed by one engineering group!

Following World War I, the expenditure of government funds on various research projects had greatly increased along with the growth of science and technology in this country. Such agencies as the Bureau of Standards, Naval Research Laboratory, Department of Agriculture, Bureau of Mines—to mention only a few—had been recognized for their contributions to science and technology. The trend toward government participation in scientific research was accelerated by the experience of this country during World War II in mobilizing scientific and engineering talents for national defense. It was realized in 1940 that there was not enough time to expand the existing government research agencies to the extent demanded by the emergency. The only alternative was to make effective use of the facilities existing in industry, universities, endowed institutions, and various government laboratories. Thus was born the Office of Scientific Research and Development, whose mission it was to work through contracts administered by committees of experts in the various fields. Thus there came into being the Radar Laboratory at MIT, which recruited its personnel, starting with a local nucleus, from universities and industry throughout the country. Other large centralized groups were also organized—rockets at the California Institute of Technology—atomic weapons at the University of Chicago—high explosives at the Bruceton Laboratory of the Bureau of Mines—proximity fuses at Johns Hopkins. In most instances the various divisions of OSRD worked through widely scattered facilities, using men and equipment immediately available and integrating various projects into an organized program through small administrative groups.

The system worked beyond the fondest hopes of those responsible for the idea in the beginning. What more natural than that there would be a movement started even before the close of the war to perpetuate this device in the interest of peace?

The first bill toward the establishment of a National Science Foundation was drafted in response to a request of President Roosevelt's, made in November 1944, to formulate a postwar plan for scientific research. When it was first proposed, the role in, and importance of, science to our national life seemed self-evident, and in accord with the benevolent atmosphere of cooperation which marked the years of the war and the early months of the United Nations Organization. There have, however, been subsequent clashes along the way between the thinking of the politicians and the scientists. The necessary legislation was once enacted, in 1947, only to meet the veto of the President. A compromise was then reached in committee during the Eightieth Congress, and in its present form the bill seems acceptable to both factions.

In 1947 the President appointed his Scientific Research Board, comprising several members of his Cabinet and the heads of the various government agencies most interested in research matters, such as the chairmen of the Atomic Energy Commission, the National Advisory Committee on Aeronautics, and the Federal Communications

Commission, under the general chairmanship of Dr. John R. Steelman. Their report, *Science and Public Policy*, should be required reading for alert citizens as well as for scientists. The report indicates the surprising extent to which the Federal government has already participated in research and development projects of national significance. "In the five years from 1941 to 1945 the nation spent $3,000,000,000 for these purposes, almost all of it going for the development work on implements of war. About 83% of the total cost of this large program was financed by the Federal government." Exclusive of the large expenditures allotted to the Atomic Energy Commission, the government spent approximately $625,000,000 in 1947, of which $500,000,000 was allocated to the Navy and War Departments, and of this amount 80 percent was spent through contracts with industrial laboratories, endowed research institutions such as Battelle and the Armour Foundation, and educational institutions. Apart from military services and the National Advisory Committee on Aeronautics (NACA), the largest Federal spenders are the Departments of Agriculture and Interior, each with an annual budget of around $30,000,000 for research. The nation's postwar budget for research and development in 1947 reached the highest point in our history—more than $1,100,000,000, excluding the Atomic Energy Commission's budget of about $600,000,000. Industry's participation in this program is estimated at $450,000,000. Thus, in the partnership we are about to see established between government and industry in research and development, the government is already the controlling partner in point of dollar contributions. On the other hand, the facilities, excepting again the Atomic Energy Commission, are predominantly in the hands of industry.

Thus in the interim between the first proposal of a National Science Foundation, to extend and expand the wartime cooperation among government, industry, and science, and now when it seems about to become law, neither science nor the government has stood still. Many of the facilities upon which the Foundation would have depended have already been pre-empted by other programs. Each of the three departments of the National Military Establishment—Army, Navy, and Air Force—has its own research plan and program, coordinated by the Research and Development Board, of which Dr. Karl Compton is now the head. Yet to some, this "unholy alliance" of military men and scientists is a fearsome state of affairs, and has been accepted only as a makeshift arrangement pending the final resolution of the problem in the National Science Foundation.

II

The nearness of the National Science Foundation Bill to enactment by the Congress raises in the minds of scientist and layman alike most pertinent questions. Will the Foundation become subject to the vagaries of politics? What will be its relationship to other agencies of the government? Will it prove its value in the integration and development of essential basic research in this country? And —will it prove as valuable (or more valuable) to the public weal as research supported by private industry? (NOTE. Whoever has read the published accounts of German experiments in atomic energy under Hitler will have noted with apprehension that the Nazi government refused to make funds or personnel or equipment available for research where it could see no *immediate* gain. Granted, it was a time of dire emergency; granted, also, the government had supreme and unrestricted power over the means to research. But would this not undoubtedly be the instinct of a government which has to answer openly and directly to the public, and which has to maintain itself in office politically? A "four-year mandate" would not necessarily imply that the government could afford public criticism of expenditures as an individual or a business organization might.)

The majority of these questions will only be answered by time and personalities. If the Foundation is supported with intelligence and confidence; if it is truly allowed to be a coordinating body of industry, government, and science; if it is as fully subject to active support and active condemnation by an enlightened citizenry as any department of state or commerce—it may well become a truly great experiment in American government. The last condition deserves, perhaps, closer examination, and it might be helpful to call to mind some of the advances of recent years brought about by private industry engaged in scientific research.

American research and industry have a long tradition of interdependence. In the beginning, research efforts on the part of business were sporadic and unorganized. They were characterized by a desire on the part of keen-eyed merchants with spare capital to "get hold of a good idea" and build it into a new business of quick profits. An invention was less important for any fundamental scientific value it might have than for its suitability for exploitation. Gradually, certain groups of business-

men became convinced of the desirability of sponsoring a regular program to produce and support invention along the lines most necessary to them. They fixed their eye on a longer and bigger business chance, so to speak. It was about this time (1890) that George Eastman was writing to an associate in London,

> It will not be long before your firm will need a practical chemist . . the best way is to make application to the Professor of Chemistry in some good technical school and have him recommend two or three first-class boys. You can . . take your choice . . If he is any good he will be the most profitable man you can hire.

The Aluminum Company of America furnishes at this time one of the first instances of a company whose business was not only made possible by the perfection of a scientific method of treating metals, but which from its inception as a company insisted on the maintenance of a research laboratory as an integral part of the business organization.

But perhaps the most clear-cut example of what private industry can do with sponsored research—and one of the best examples of what a business has been *willing* to do—is that of the amazing petroleum industry.

The discovery of petroleum in Pennsylvania in August 1859 brought to an early decline the age-old industry of whaling: a fraction of petroleum—kerosene—was more than an acceptable substitute for whale oil. Fortunately, the pools of petroleum first drilled contained a substantial quantity of the kerosene fraction, and a minimum amount of the lighter oils which were, in the beginning, destroyed for lack of knowledge of the uses to which they could be put. Work on the internal-combustion engine, which was to provide an outlet for the lighter oil fraction, proceeded so swiftly, however, that by the turn of the century it began to appear that the light oil fractions obtainable would not long satisfy the demand for them. By 1910 demand for gasoline began to approach the amount naturally available from crude oil.

Petroleum, it might be explained, is a mixture of thousands of different hydrocarbon molecules of different sizes and shapes. Hydrocarbons with 5–10 carbon atoms have the volatility and other properties required of gasoline; those with 11–15 constitute the kerosene fraction; 16–23, domestic fuel oil; still heavier molecules comprise the lubricants, heavy fuel oil, and asphalt. Early refining of Mid-Continent crude oil consisted in roughly separating these molecular species into groups or fractions such as kerosene or gasoline, leaving around 70 percent residual for sale as gas oil or in competition with coal as a fuel.

Dr. Burton and his two assistants, who in 1909 constituted the research department of the Standard Oil Company of Indiana, sensed the approaching dilemma. They began to experiment with methods of cracking the larger hydrocarbon molecules, and finally devised the process of heating the gas-oil fraction in direct-fired cylindrical stills about 8 feet in diameter and 30 feet long, to a pressure of 75 pounds and to a temperature of 730°. By this means it was possible to double the yield of gasoline.

A comparatively rapid commercial development followed. Many companies took out patent licenses, and hundreds of Burton stills were installed. Since then, tens of millions of dollars have been spent in the continuing study of the reactions involved in cracking and re-forming the hydrocarbon molecules naturally occurring in crude oil.

The discovery that internal-combustion engines could operate at a higher compression ratio, and hence more efficiently on these synthetic fuels, stimulated research toward improving their quality. Because it was sensed that this behavior was due to the structure of the hydrocarbon molecules, basic research programs were instituted to study this structure. These programs culminated in the development of the modern catalytic cracking plants which so conspicuously adorn the sky lines of a modern refinery.

In the wake of these developments in petroleum fuels there has come into being, in the past twenty years, a petroleum chemical industry producing annually articles valued commercially at $600,000,000. These products of the petroleum chemical industry supply as well many basic needs of our increasingly complex economy. Without certain of these chemical compounds, for example, the synthetic rubber program of World War II would have been impossible, and America, dependent upon rubber-tired transportation, would have come to a slow stop.

The results of research on cracking, while profoundly affecting the direction and extent of technological development in this country during the past three decades, have been of social and economic significance. From 1918 to 1947, the energy consumption of this country increased enormously. Of that total increase in energy consumption, including heat for homes, petroleum and natural gas represent 95 percent, water power 5 percent, and coal has remained at its old level.

Automobiles, trucks, buses, tractors, and air-

planes could never have expanded to the present number without the more than doubling of the country's gasoline supply through cracking. The total vehicular horsepower is now over 3 billion as compared with 70 million in all central-power stations. The output of food per farm worker has been increased by 50 percent through the expansion of gasoline-powered farm equipment in the past twelve years. The horse population has been reduced by about 10 million since 1928, so that we can now feed 40 million more people. This alone means that we can supply Europe with the amount of food needed to exceed the mere subsistence levels at which European civilization would slowly but surely perish. By supplying the British Spitfires during the Battle of Britain with synthetic 100-octane gasoline, and thus with a margin of superiority over German planes, we presumed to direct the course of history. Through our high per capita energy rating we are now able to accept our obligations for these acts, in implementing the Marshall Plan. Each of us in America is served on the average by an amount of energy equivalent to that furnished in ancient times by thirty slaves.

In reviewing the research achievements of the petroleum industry, Dr. Robert E. Wilson, chairman of the Board of Standard Oil Company (Indiana), recently summarized his findings in an address entitled "Research on a Single Reaction and its Social Effects." He said:

> But how did all these beneficial social effects come to pass? Were they the result of some fine central planning and ordering of our industry and our national economy? Far from it. They resulted from the natural operation of the hope-of-profit incentive, teamed up with science and technology. . . . No Government subsidies were asked or needed. Indeed the industries dependent upon cracking have paid billions of dollars in taxes and made employment for millions of men. They are taking a substantial share of the output of our technical schools and are supporting an increasing amount of basic research in our universities. True, the individuals and companies who pioneered in cracking profited thereby—if they had not, cracking would never have gone forward. As with all successful inventions, however, these individual profits were but a drop in the bucket to the enormous social benefits which flowed from them.

III

And now to look at the other side: Can a case be made out for government-supported and -planned research? It is argued that the greater the complexity of our needs, the greater dependence upon fundamental knowledge for advances, the inevitable specialization of our scientists, the higher cost of research, and the need of insuring national security in a military sense can only be met by further expanding the role of government. Proponents cite the experience of the Office of Scientific Research and Development in mobilizing and directing the technical manpower of the nation during World War II. In the instance of the Manhattan District they present the strongest case. All factors combine to support the Atomic Energy Act of 1946 as providing the only possible means under present-day world conditions for the development of nuclear science and engineering. It remains, however, to be demonstrated that an all-out government-planned and -supported program can match the achievements that can be cited from the history of private enterprise.

Industry in turn is expanding its facilities. Last fall a new General Electric Research Laboratory at Schenectady, New York, was dedicated, which, when completed, will cost $18,000,000. Within sight, and about a mile away, is the Knolls Laboratory of the Atomic Energy Commission, which, by contract, is under the direct management of General Electric Company. The projected cost of this establishment is $37,000,000. Bell Telephone Laboratories have just completed their new installations; Standard Oil Development Company, Standard Oil of Indiana, Goodrich Rubber, Shell Oil, and many others are in the process of building, or have just completed, new laboratories for research.

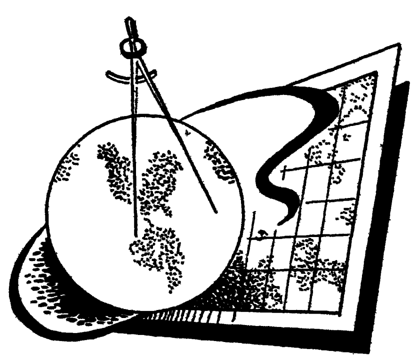
Under arrangements such as that just cited with the General Electric Company, the Atomic Energy Commission is establishing and using numerous laboratories to be operated under contracts with universities and industrial organizations. Carbide and Carbon Chemicals Corporation is operating the Oak Ridge National Laboratory, and the University of California is operating the laboratory at Los Alamos (where work on atomic weapons is centered). A laboratory now under construction near Pittsburgh will be operated by the Westinghouse Electric Corporation. In the outskirts of Chicago thirty-one Midwestern universities and research institutions cooperate in the work of the Argonne National Laboratory; a similar project, the Brookhaven National Laboratory, is located on Long Island. A group of the larger Eastern universities, including Harvard, Yale, Princeton, Columbia, and MIT, formed Associated Universities, Inc., for its administration. In the laboratories built and operated during the war by the Du Pont Company at Hanford, Washington, the Commission is operating, under the management of General Elec-

tric Company, what is probably the largest industrial plant in the United States. This fenced-in and closely guarded area covers 400,000 acres. It is here that we are producing the new element plutonium—the substance of atomic bombs. In April of this year appeared the announcement of a new facility to be built in Idaho for the reactor program.

By this enormous government-sponsored program in the field of nuclear science and technology we aspire in the next decade to an accomplishment such as we have witnessed in this country during the past three decades in the field of chemistry and chemical technology. In the cause of national safety and world peace there seems truly to be no alternative to this procedure. The mere possession by unauthorized groups or individuals of fissionable material—uranium, notably—is forbidden by the Atomic Energy Act of 1946. And aside from this government monopoly granted by an Act of Congress, there is the very practical consideration that the tools used in nuclear research, such as the cyclotron and the nuclear reactors, or "piles," as they are commonly called, are beyond the reach of private enterprise. They simply cost too much.

It seems no longer doubtful that we are definitely trending toward a highly socialized state of affairs in the field of science. One major area is largely controlled by government. A higher percentage of scientifically trained people are now employed on government-controlled and -sponsored programs than can be cited for any other group. The scientific and technical personnel of the overall AEC program are numbered in the thousands. With the enactment of the Science Foundation Bill another step will be taken, although in the beginning it is not anticipated that existing programs will be much extended. The power to spend and plan will, however, be more centralized, and this is the red flag for the opponents of this legislation. They fear the regimentation of science for other than limited national purposes—defense and public health—the control by government of an increasing number of patents, the monopoly by government of research facilities in the universities to the exclusion of industry or of the opportunity for the universities to initiate scientific investigation as in the past.

The atomic energy program is the crucial test. The immediate objective is one of military necessity, but the long-range problem is the development of means for releasing and utilizing the energy of a nuclear reaction It is still far from certain that this is a technically feasible and economically practical undertaking. Nor is this uncertainty the only risk. It remains to be demonstrated that a government agency is capable of carrying through such an enterprise under the directive of an Act of Congress and subject in the last analysis to the control of Congress. This is in every sense a great experiment. Upon its success or failure depends more than the issue of atomic energy for industrial purposes. We are at work on a new social and economic concept of far-reaching consequence either to a world at war or to a world at peace.



ALASKA AND THE GEOLOGICAL SURVEY*

JOHN C. REED

Dr. Reed, who received his Ph.D from Princeton in 1930, has been with the U S. Geological Survey since that time. He has made geological surveys in New York, Oregon, Idaho, Arkansas, and Alaska. He is the staff geologist for the Territories and Island possessions in the Office of the Director of the Survey, a governor of the Arctic Institute of North America, and chairman of the Advisory Board of the Arctic Research Laboratory of the Office of Naval Research

ALASKA has been American territory for more than eighty years. During that period the resources of this vast area have been developed in a sporadic fashion and in general very incompletely. Nevertheless, in that time Alaska has yielded nearly a billion dollars worth of new wealth in minerals alone. In the Russian days the fur industry was, of course, the big item, and the sea-otter and fur-seal population in Alaskan waters suffered to the point of threatened extinction. The three principal industries of Alaska are now commonly considered to be commercial fishing, mining, and furs, in the order named. To speak of fishing, mining, and furs as the three principal industries may be a little misleading, because such a listing includes only the industries that can be measured in terms of new wealth produced. Other types of activity are very important and should not be omitted; for example, there is the retail liquor industry. In 1947 almost 1.5 million gallons of malt liquors were shipped to Alaska, nearly 400,000 gallons of whisky, and 27,000 gallons of other distilled liquors. Similarly, the construction industry in terms of dollars spent, and including both military and civilian construction, probably outranks any other Alaskan industry at the present time.

At various times in the past impetus has been given to the development and exploitation of Alaska. The initiation and growth of the salmon-packing industry; several gold rushes; widespread mechanization of mining through the introduction of dredges, tractors and bulldozers, draglines, and other heavy equipment; the construction of the Alaska Railroad, which was completed to Fairbanks in 1923; the Matanuska Valley development—all these and many other elements have had their short- or long-lived effect. All of them, however, are minor compared to the changes that began just before America's entrance into World War II and that have continued up to and including the present time.

FACTORS IN THE DEVELOPMENT OF ALASKA

Today there is a much greater public awareness of the existence of Alaska than ever before. Many men in the military services were stationed in Alaska during the war. They brought back and disseminated much—if perhaps somewhat local and biased—information on the Territory, its geography, climates, resources, and people. A substantial military population is in Alaska now, and it is evident that the military services are greatly interested in Alaska. This interest presumably is the net result of the consideration of many factors. The effect on Alaska is of great importance, and the various military activities are a large, if not the dominant, factor in Alaska's current development.

The international situation inevitably focuses attention on Alaska's strategic geographic position at the northwest corner of North America just across the narrow Bering Straits from the northeast corner of Asia. Noteworthy also is the long finger of the Alaskan Peninsula and the Aleutian Chain stretching to Attu, three quarters of the way from Anchorage to the Kuriles.

The relation of Alaska to global air routes is well known. It has been said that aviation has now reached such a point that no place on the earth is more than three days by air from any other place. A surprising number of great circles between large population centers in the Northern Hemisphere pass through or near Alaska—New York to Tokyo, San Francisco to Shanghai, Minneapolis to Calcutta, London to Honolulu, Moscow to Vancouver, New Orleans to Manila. Already Alaska is served by Northwest Airlines on a regular schedule from Minneapolis to Anchorage and Shemya in Alaska and on to Japan, China, and the Philippines, Northwest Airlines also connects Anchorage directly with Seattle. Similarly,

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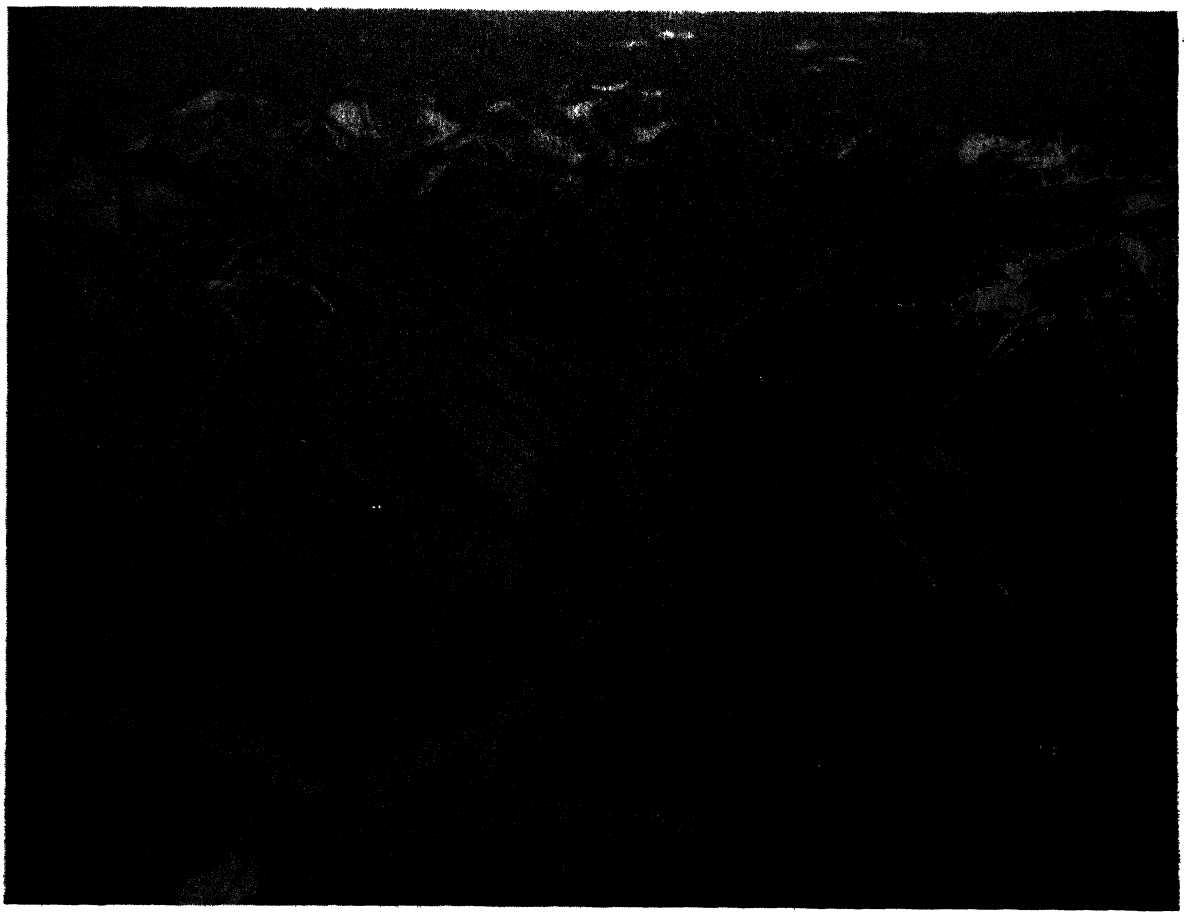
Pan American operates from Seattle to Ketchikan, Juneau, and Whitehorse in Yukon Territory, and then to Fairbanks and either Nome or Bethel.

Many airlines operate on an unscheduled basis between the States and Alaska and provide means of transportation for people and for freight of surprising variety. Within the Territory, the airplane is truly Alaska's streetcar. In addition to several companies operating on schedules over many routes between the principal centers, there are many airlines engaged essentially in "bush flying" on wheels, floats, or skis that can land close to almost any point. Many individuals and companies operate their own aircraft either within the Territory or both within Alaska and between Alaska and the States.

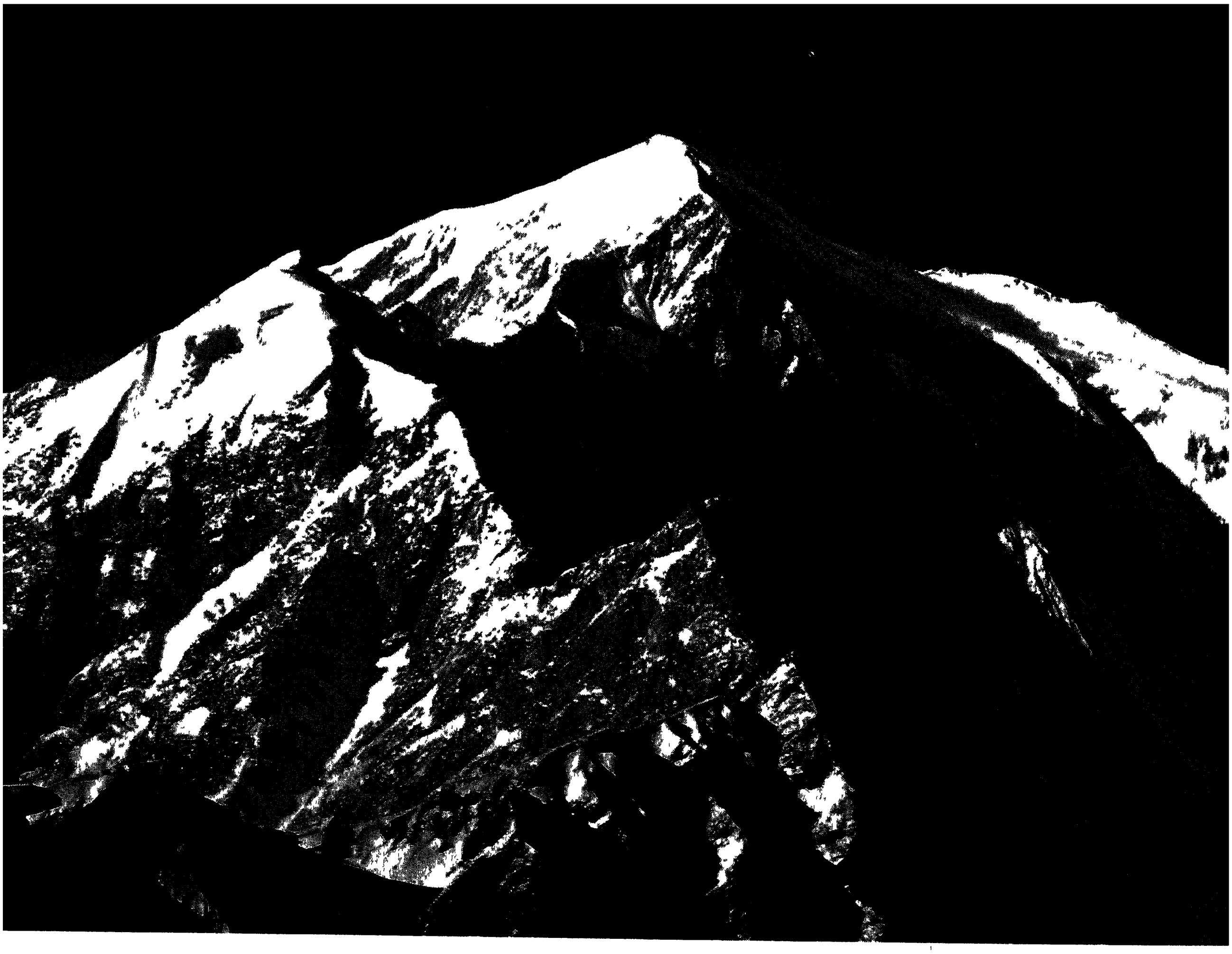
Federal expenditures in Alaska, other than military, have been increasing rapidly, especially since the war. Many of the activities are tied closely to military needs, others are not. A good example, though a relatively minor fiscal item, is my own bureau. The U. S. Geological Survey operated in Alaska before the war on an annual appropriation of less than $100,000. In the present fiscal year appropriated funds for Survey work in Alaska total more than $800,000 and with the funds made available from other agencies, including all three military services, aggregate almost $1,125,000.

Civilian construction workers are widely scattered over the Territory and for the most part are receiving exceptionally high returns for their services if returns are considered to include special items of recompense such as food and lodging, special clothing, transportation to and from the States, and high percentage of overtime.

The Alaska highway system is now developing into an integrated net of substantial proportions. The Haines Cutoff connects the coast at Haines in southeastern Alaska with the Alaska Highway in Yukon Territory. The Richardson Highway connects Fairbanks with Valdez. A resident of An-



The Brooks Range traverses Alaska approximately along the 68th degree of latitude or, roughly, 100 miles north of the Arctic Circle. In summer there is little snow on the range because of the scanty precipitation, and glaciers are few and small. (U. S. Navy photograph.)



In good weather, today's air traveler may view the incomparable mountain scenery of the rugged peaks of the Alaska Range. (U. S. Air Force photograph.)

chorage can now drive to Fairbanks by way of the Glenn Highway to Glen Allen and thence over the Richardson, or to the States by using the Tok Cutoff from Gulkana on the Richardson to Tok Junction on the Alaska Highway. Seward is now the terminus of a road leading to Hope or to Kenai on Cook Inlet, and a connection from Anchorage to this road will soon be completed, as well as an extension from Kenai to Homer.

The Alaska Highway is being used to a far greater extent than many of us had anticipated. A complete count at the Alaska border in 1948 showed that 3,227 cars, 824 trucks, and 113 buses entered Alaska over the Highway, and 2,299 cars, 808 trucks, and 133 buses left Alaska. The Highway is open the year round, and, although neither winter nor summer travel over it is to be contemplated too lightly, such travel is proceeding.

MAJOR PROBLEMS OF THE FUTURE

All the factors that have been discussed are important in the development of Alaska, but many others might be mentioned. Whether Alaska moves forward, or development again slows down as in the past, depends in large part on the degree to which her problems are solved or remain unsolved. Among these problems are the following:

1. *Population.* Alaska needs more people. I am not speaking of military personnel, or temporary construction workers, or seasonal cannery employees, or even those who go to Alaska with the intention of staying a few years to make a stake and then get out. I mean hardy, intelligent, industrious residents who expect to make Alaska their home; the place to settle down, raise their families, and grow with the growth of the Territory. If such people are to become Alaskans in large numbers, they must have opportunities, and to have opportunities there must be industry, agriculture, trade, and adequate living conditions, including housing, schools, and cultural opportunities.

2. *Transportation.* Alaska needs much more adequate transportation facilities and much cheaper transportation rates. The Alaska Highway and other Alaskan roads are not first-class, modern roads; they are not paved. Some are not kept open

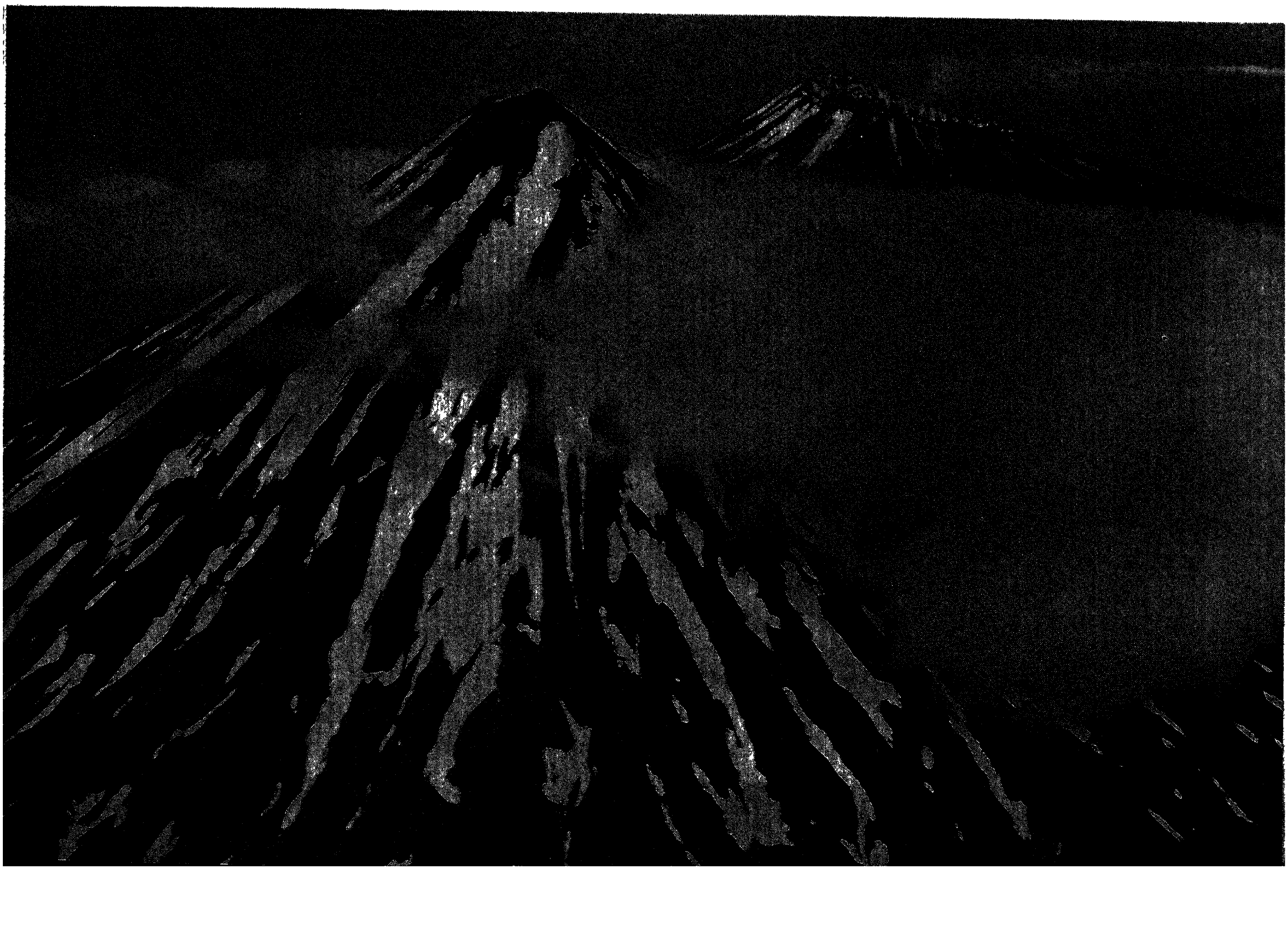
throughout the winter. Steamer transportation is inadequate; it is slow, uncertain, and costly. Maritime strikes continually threaten ship transportation to and from Alaska. Only two United States airlines operate on regular schedules between the States and Alaska, and no Alaska line is certified for such service.

Transportation within the Territory by any means—boat, air, highway, or railroad—is still far too limited, too expensive, and too unreliable. More connecting roads would be of especially great benefit.

3. *Housing*. Deficiency of housing seems to be a problem almost everywhere, and in Alaska the shortage is critical. The shortage is most noticeable in the railroad belt at such places as Anchorage and Fairbanks where, in addition to service personnel at near-by bases, there are large numbers of construction workers. Those two cities are also the destination of many of the prospective settlers who have driven to Alaska over the Highway. The deficiency of housing throughout much of Alaska is accentuated by the rigorous winter climate and the extraordinarily high cost of construction materials.

4. *Year-round employment*. A large proportion of the work done in Alaska now is seasonal. Commercial fishing, the largest industry, is a summer activity; so is mining to a large extent, especially placer mining. Even the large construction projects are prosecuted much more vigorously in the spring, summer, and fall, although some construction work does go on throughout the year. Every effort should be made to develop in Alaska industry of the year-round type, or, better yet, industry that could be active principally in the winter in order to use some of the labor that otherwise would be unemployed in winter or would return to the States. To plan or visualize such smoothing of the over-all employment curve throughout the year is very easy on paper but, in a more or less free economy, is very difficult in practice. It is no great feat to plan that a cannery worker in the summer could carve trinkets or make furniture in the winter,

In the central part of the Aleutian Chain are the islands of the Four Mountains, dominated by a group of high, symmetrical cones, among which are Mounts Cleveland and Herbert. (U S. Air Force photograph.)



but the cannery worker may have no ability or desire to use his winter in that fashion.

5. *More diversified industry.* Closely related to the problem of year-round employment is that of encouraging and stimulating more diversified Alaskan industry. Many items used in Alaska that must come from the States at present could be found, made, assembled, or grown in the Territory. Any steps toward doing this would provide Alaska with a more stable economy and at the same time would decrease the Territory's dependence on insecure and expensive shipping from the States. Even better would be the establishment and growth of industries that would yield products exportable to the States or elsewhere.

WORK OF THE GEOLOGICAL SURVEY

Some of the factors leading to the present interest in, and development of, Alaska and some of the problems that Alaska faces have been briefly summarized. Let us now review the responsibilities of the Geological Survey in Alaska to see how they integrate with work now going on in the Territory and with anticipated future development.

The Geological Survey is responsible for the topographic mapping of the United States, for geologic mapping and investigations of mineral resources, for water-resources studies both of surface and underground waters, for classification of the public lands as to their mineral or nonmineral character and as to their water-power potentialities, and for the supervision of mineral leases on the public lands. In short, the Survey is primarily a fact-finding research organization dealing with an important group of natural resources. All the responsibilities of the Survey in the States also belong to it in Alaska.

The complex polygonal surface pattern over much of the Arctic coastal plain of Alaska is a conspicuous permafrost feature. In many places there is striking contrast between present lakes and several generations of earlier ones. (U. S. Navy photograph.)

The Survey is very firmly of the belief that the rational development of underdeveloped areas such as Alaska, and the wise use of the resources with which the Survey deals, depend in large part on a full knowledge of the basic facts. It is the Survey's business to collect, systematize, interpret, and report on certain of those basic facts.

Unfortunately, information on the mineral resources of Alaska, on the configuration of the surface, on its water resources, and on the mineral and water-power classification of its lands is now woefully inadequate. This fact has been generally recognized in this period of interest in Alaska, and consequently the funds and facilities at present available for the Survey's Alaskan activities are far larger than ever before. Large as the program now is, compared with the past, it is still not adequate at the present scale to provide soon enough the fundamental information needed by all the various interests, public and private, that have a part in the Territory's development. Thus the Survey must carefully appraise the various needs and select projects for early accomplishment that appear to be the most important.

In making the field plans for any given year various factors must be considered. Some of the major ones include:

1) Study of the national need for the proposed investigation. Here are considered such things as the position of the nation in regard to, say, a mineral commodity that may be present in the area of the project and the chances of substantially improving the national situation as a result of the proposed project.

2) Appraisal of the Territorial need for the proposed investigation. To what degree may the work contribute to bettering the Territorial economy? Investigation of certain gold-bearing areas, for example, may be of great importance so far as the Territory is concerned but of little or no significance nationally.

3) Consideration of the needs of the military services. Because of Alaska's importance from a defense standpoint, such needs are given a high priority when projects are planned.

4) Balancing of the needs of other Federal agencies. Survey information is a prerequisite to the activities of many other Federal agencies. In general, special consideration is given to projects that contribute to the needs of several other agencies.

5) Fulfilling the requirements of agencies that are making funds available to the Survey for the work. Such requirements must, of course, be given top priority if the funds of others are accepted.

A symmetrical ice bridge formed in the lower part of Muldrow Glacier in Mount McKinley National Park. Size is indicated by figure of geologist (*left center*). (U. S. Geological Survey photograph by the author.)

Thus it is apparent that the matter of programming is quite complex. The planning must be fluid to allow for continually changing requirements and, at the same time, must be designed to permit systematic progress in fulfilling the Survey's general responsibilities, as previously stated.

With the philosophy just outlined as a background, and keeping some of the major problems of Alaska in mind, the Geological Survey is now carrying on the program summarized below.

The over-all program for the summer of 1948 embraced about forty-five field parties widely scattered over the Territory from Ketchikan to Barrow and southwestward to the farthest Aleutian Islands. The work was done in a wide variety of environments and in many different ways. Some parties worked near main settlements such as Fairbanks, others in mining camps; many were in isolated areas, living with facilities taken along. Work was performed with the use of motor vessels, with river boats, some collapsible and transported by air; aircraft were used by most parties, including two helicopters in topographic mapping. Weasels were extensively used in the tundra areas of northern Alaska, and trucks and jeeps elsewhere. The number of parties was somewhat larger in the 1949 field season.

The geologic program now includes work in general, military, and engineering geology, geophysics, the study of permafrost, or permanently frozen ground, and mineral investigations of coal, petroleum, metals, and nonmetals.

Much of the topographic mapping is now done photogrammetrically with aerial photographs in Denver, but ground control must be obtained by surveys in the field. Most of the mapping

work is now on three series of maps—first, a series of general maps of all Alaska on various scales; second, coverage of the whole Territory by a series of topographic maps on a scale of one inch equals about four miles; and, third, a series of topographic maps on a scale of one inch equals approximately one mile. Some of these new maps have already been published, and others will be appearing in increasing numbers.

Although some water-resources investigations have been made in the past in Alaska, they were for the most part sporadic and discontinuous. The present systematic program began about three years ago and is now making substantial progress, although not on a satisfactory scale. Thus far attention has been concentrated in the Alaska railroad belt and in southeastern Alaska. The work now includes both surface-water and ground-water studies, and a quality-of-water laboratory has been established at Palmer, in the Matanuska Valley.

Land classification in Alaska as to mineral, water, and power potentialities is not adequately financed. Nevertheless, some progress is being made toward coal-land classification, and several streams such as Eklutna Creek and the Little Susitna River near Palmer, Ship Creek at Anchorage, and Power Creek near Cordova have been studied for their water and power possibilities

ALASKA PUBLIC WORKS PROGRAM

Legislation authorizing the General Services Administration to carry out a five-year $70,000,000 public works program aimed at the intensive development of Alaska and her resources has been passed by Congress and was signed by President Truman on August 24, 1949.

Funds will become available as and when they are appropriated by Congress. It is estimated that the program could involve $10,000,000 in the first year —$2,000,000 in cash and $8,000,000 in contract authority, depending on appropriations, with a proportionate increase in subsequent years.

Sewers, water systems, schools, and hospitals will be provided in the Territory on a matching fund basis. That is, the Federal government, with headquarters in Alaska, probably at Juneau, will build facilities and sell them to individual communities when they have been completed, at prices ranging from 25 to 75 percent of the actual cost. The law provides that the entire public works program will only cost Alaska an average of 50 percent, which really means, of course, that the Territory will receive what amounts to a 50 percent grant in aid. The arrangement whereby the government will build the public works was made because it was felt by the sponsors of the bill, Delegate Bartlett and Governor Gruening, that the costs of administration alone would be prohibitive to most small Alaskan communities.

Incorporated in the law are enabling clauses that permit the communities and the Territorial government to make applications at once, with a minimum of red tape, for whatever facilities may be needed. Because of the over-all 50 percent aid provision, however, it will be necessary that a majority of the applications be in the hands of the General Services Administration before a complete program can be planned.

AN ANIMAL'S HOME IS ITS CASTLE

DAVID E. DAVIS

Dr. Davis (Ph D., Harvard, 1939) is in the Rodent Ecology Project of the School of Hygiene and Public Health at Johns Hopkins. He is a student of the behavior of birds and of the correlation of the function of the endocrine glands and behavior

THE old English adage "A man's home is his castle" expresses a desire in man to own and maintain inviolate a suitable place for living. This fundamental urge seems to manifest itself in one way or another throughout the entire animal kingdom. Among vertebrate animals, for example, many fish, reptiles, birds, and mammals confine their breeding activities to a definite area and will defend the spot against intruders. This defense of a restricted area used for breeding is known as territorialism. Each species manifests territorialism in a different manner, showing great variation through the different vertebrate classes.

As a simple instance of the urge to defend a restricted territory, the behavior of the song sparrow may be described from the work of Nice (1937). In the spring, after migrating to the nesting region, each male settles in a well-defined area where he sings and fights to defend the territory, and drives out any male intruders by threatening or by actual combat. When a female arrives, pairing may occur, and the female usually builds within the territory, which the male holds until the young are fledged. During this time the neighboring territories are delimited and defended by other males, and the sparrows recognize their boundaries by mutual agreement. The defense continues until the young are well-grown.

From this briefly described example it may be seen that the behavior of animals in defending a territory presents numerous problems in animal psychology. First, the psychological causes of fighting might be analyzed: one seeks to discover what processes in the nervous system of the individual stimulate him to react to an intruder, what motivations urge him to fight. Second, results accomplished by the defense of a territory may be determined—in some species food is conserved for the young; in others a mate is obtained within the territory. Third, the origin and the course of evolution of these widespread behaviors may be studied.

As in other aspects of animal behavior, it is a difficult task to segregate the various problems under consideration. For example, we see two sunfish, or two lizards, or two robins, or two stags fighting. What are the conditions and causes and motivations of the fighting? Some fighting results in the establishment of an order of dominance or social rank within the group of individuals; other combats are waged in reference to females; still others concern the piece of land owned by an individual. Although there are more functional causes of fighting, we shall here describe only the manifestation of the defense of an area in certain species of vertebrates and mention some questions thereby raised.

The development of the concept of territorialism is relatively recent. Several early ornithologists noticed that certain birds live in a restricted area and maintain a territory. Altum in Germany, Montagu in England, and Bullock in America mentioned the defense of a piece of land. But it remained for Eliot Howard (1920) to propose a comprehensive theory of territorialism based on his intensive studies of the English warblers. His book, *Territorialism in Bird Life,* is considered the greatest stimulus to ornithology since the work of Darwin. It is interesting that Moffat in Ireland in 1903, Mousley in Canada in 1919, and Hammer in South Africa in 1922 independently expressed, although in much briefer form, the same ideas. More recently, many species have been studied in great detail.

TERRITORIALISM IN FISH

Numerous kinds of fish defend a small area and lay eggs there. The family Centrarchidae, to which many familiar game fish belong, including bass and sunfish—demonstrates this behavior better than any other group. A good example is the common sunfish, *Eupomotis gibbosus,* which breeds in the shallow waters of ponds and small lakes. The male excavates a shallow depression, or nest, with his fins and remains on this nest for much of the day in the breeding season. When another sunfish approaches, he at once attacks it to determine the sex. If the newcomer is a ripe female, she identifies her sex and ripeness by behavior. The male then

induces her to come onto the nest, and fertilizes the eggs as she lays them. If, on the other hand, the intruding fish is a male it raises its gill, thereby displaying the conspicuous scarlet spot that marks its sex. In this case the owner of the nest continues to attack the stranger. In this and many other species the male drives from the nest other males or unripe females even though a ripe female may be absent. The motivation for fighting is therefore presumed to be defense of the nest site, not defense of a female. The function of the fighting is the provision of a place in which eggs can be laid.

TERRITORIALISM IN AMPHIBIANS AND REPTILES

As yet no account of the behavior of frogs or salamanders clearly describes territorialism for an amphibian. It is known that toads remain in one location for long periods of time and that frogs in the breeding season croak from a particular location. Further studies are necessary to determine to what extent these areas are defended and maintained.

The information concerning reptiles is more adequate. Studies of the lizard *Anolis* (Evans, 1936) show that the males defend a restricted area and maintain a harem. The owner drives away other males but corrals the new females and adds them to his harem. Each female, on the other hand, tends to maintain a small area within the larger territory of the male. One female seems to be dominant over the others, and if not prevented by the male will drive out newcomers. Removal of the testes results in a loss of the fighting instinct, whereas injection of suitable hormones into castrated males or females produces the fighting reaction.

Among other reptiles, available information shows only that individuals remain in an area for a considerable period of time. For example, I observed that a boa constrictor sunned itself daily on a rock beside the laboratory at Barro Colorado Island, in the Canal Zone, and that an alligator lived on a definite section of a riverbank in British Guiana. In these cases, however, the individual may merely have lived in the area without actively defending it. Until more is learned of the behavior of most reptiles, one must speak of the home range of an individual, without attempting to analyze the psychological aspects of its behavior.

TERRITORIALISM IN BIRDS

The behavior of birds in relation to territory has been extensively studied for two decades. Many species have been investigated, some superficially, a few in great detail. Throughout this group there appears to be great variation in territorial relations. In some instances, it is difficult to be sure that the behavior of the species should be classed as territorial; in others, the behavior, although no doubt territorial, reveals interesting modifications in this respect. The following descriptions of the behavior of several species indicate the extent of the variation in territorialism.

The European reed bunting may be considered a typically territorial bird. In the spring, after migration, the males settle in a definite area, which is defended from other males, if possible, until the females arrive from the south. When a suitable female enters the territory the establishment of a pair occurs. After pairing, the male practically ceases singing, which in this species is primarily a defense measure and an announcement that the male is unmated. During the nesting period there are continuous changes in detail of the territorial boundaries; a newcomer may force his way into an area by obtaining parts of the territories of two or three males. Both sexes defend the boundaries; the females drive out other females, and the males drive out other males. For the second brood a pair may maintain the same territory.

The reed warbler of England, a bird related to North American kinglets, also demonstrates this type of territorialism. The male isolates himself in a part of the reed bed and sings loudly until a mate is found. During the nesting period the male is silent, except for an occasional weak attempt at song, but vigorously defends the territory against intruders. After the young have hatched, the family remains more or less within the territory until the young are able to fend for themselves.

Many species of birds present interesting variations of the territorialism outlined above. For example, the European robin (Lack, 1939) shows a modification that occurs also in mockingbirds and shrikes. The males and the females each obtain a territory in the fall and sing and fight to defend it. In January, however, the females lose the urge to defend the territory and at this time seek males. During the breeding season the females occupy the territories of their mates but do not sing.

The guillemots present a variation that occurs in many other marine species. These birds nest on rocky cliffs, and each pair defends a territory a foot or two in width around the nest site. This piece of land has all the characteristics of a territory but is small in extent. Apparently this restricted type of territory is related to the type of food; since the rocky cliffs used for nesting pro-

vide no food, the birds have no need for an extensive territory. Many other colonial birds, including gulls and terns, occupy a similarly restricted nesting territory.

The phalaropes, an aberrant kind of sandpiper, present a further modification. In these species the female is the more brightly colored sex and courts the male. The male incubates the eggs, and the female usually deserts the nest during incubation, leaving the male to raise the young. As might be expected, the female herself selects and defends the territory.

The European rook, a kind of crow, lives in colonies that consist of many individuals. Each colony maintains a territory and drives strangers away; furthermore, it is claimed that individuals may not leave the group. Within such a colonial territory each pair maintains its own territory around the nest site. This may be a foot or two in width. Since the male defends the territory vigorously, even in the absence of the female, it is believed that he fights for the territory and not for the female. The usefulness of the territory in this case is that it prevents other rooks from stealing nesting material and destroying the nest.

In South America a group of cuckoos, the Crotophginae, has lost the instinct to defend a territory individually. These birds, called anis, live in flocks of five to ten individuals, all of which cooperate to build a single nest. Several females lay in the communal nest, and the whole group defends a territory around this nest site, fighting fiercely to keep out intruders. The birds may pursue an outsider for two or three days to drive it away. They will immediately demolish a dummy bird placed in the territory, and will kill live birds, if they are unable to escape, in a few minutes. Sometimes, however, under conditions not yet understood, the stranger is allowed to join the colony.

Cowbirds present an interesting history of stages in the evolution of territorialism. One South American species of cowbird occupies and remodels a nest of other species of birds and defends a territory around that nest. Another species lays its eggs parasitically in the nest of the first species and apparently defends a territory only during the first part of the nesting period. The North American cowbird lays its eggs in the nest of any kind of bird and probably seldom maintains a definite territory.

The European cuckoo, which is also parasitic on other birds, has slightly different habits. Since each female normally lays its eggs in the nest of only one species of bird, each female maintains her own territory and will not permit another female which parasitizes the same species to enter. But a female cuckoo which parasitizes other species is not driven away, according to present accounts.

The foregoing descriptions indicate some of the variations in the manifestation of territorialism among birds. Many other species have been studied, at least superficially, and fit into the same general outline of behavior.

TERRITORIALISM IN MAMMALS

Although in birds territorialism is quite well understood, in mammals the situation is not yet clarified. The nests of mammals are primarily places for warmth and sleeping, whereas in birds and other vertebrates the nests are primarily places for rearing the young. This difference may represent a fundamental divergence in the behavior of mammals and birds. Most individual mammals, however, have a definite home range—the area in which the individual lives Bears and mountain lions cover a large but nevertheless restricted area in hunting. A pack of wolves may range over a hundred square miles in search of food, making a more or less definite circuit from week to week. Apparently each individual limits its activities to a definite home range, and, in general, the males utilize a larger home range than do the females. The provision of food and shelter appears to be the primary function of the home range, and the home ranges may overlap in many cases.

Many rodents live in a very restricted area, each individual occupying a part of an acre. The male of the common white-footed mouse (Burt, 1940) maintains a home range which includes the range of several females. Chipmunks defend a territory which is contained within the home range, and some squirrels defend an area to secure pine cones for food. Fremont's squirrel in Colorado has a churring note used to defend its territory against intruders. At the present time it is difficult to compare closely the home range of a rodent with the territory of a bird or fish. Fighting can be so seldom observed in the nocturnal mammals that there is little basis even for conjecture.

Certain other mammals present behavior that may be considered a true territorialism. Many dogs defend their yards against intrusion by another dog. Male Eskimo dogs, upon reaching sexual maturity, begin to defend a territory by driving away other males. In our domestic dogs, however, territorialism seems to have been masked by civilizing influences.

The red deer of Scotland (closely related to the

American elk) possesses a matriarchial social organization (Darling, 1937). An adult female leads a group of females and young males. The adult males wander about individually except in the rutting season. The herd of deer, led by a female, maintains a territory and drives out other adult females. When the young males reach maturity at the age of two years, they separate from the herd. This organization seems to be territorial in a broad sense of the term.

The behavior of male fur seals during the mating season is clearly territorial. The bull obtains a piece of the rocky shore and drives away all other males. The battles are fierce and frequently result in serious wounds or even death, especially at mating time. One bull may have as many as one hundred cows in his harem, though forty or fifty is more usual. The defense of this area requires much time and energy, and he does not feed for several months during the breeding season, becoming emaciated and battle-scarred. Young males do not fight for a territory until they are about six years of age.

Among the primates, the South American howler monkey, which possesses a patriarchial social organization, is known to maintain a definite territory (Carpenter, 1934). A clan, consisting of an adult male and several females and younger males, maintains a territory partly by vocalization and partly by fighting. The concert of roars produced by two rival males in the defense of the territory can be heard for more than a mile and is one of the wonders of the jungle. Since the territory is large, it is not unusual for one clan to invade for a short time a part of the territory of another group until the owners return.

The gibbon of Siam also maintains a territory for each group, and is of particular interest because it is apparently closely related to the human stock. Each group is locked in its own territory by the surrounding groups. Vocalization, as a substitute for fighting, is used to repel invaders and indicate the territorial boundaries.

SOME BEHAVIORISTIC PROBLEMS

The foregoing account indicates the widespread extent of occurrence of territorialism. The origin of such behavior is worthy of consideration. For the present one can merely suggest a possible mode of evolution, based on the material now available. In fish, territory is clearly a place for fertilizing and laying eggs, and the necessity for a place for fertilization seems to be the basis of the trait in this group. In some forms the territory is large and in others small, but in all it serves that function. In the lizards, also, the territory secures a location for reproduction. In birds, the behavior has expanded into multifarious types which obscure the origin of the trait. In some primitive birds, however, the territory is clearly a station used in establishing the pair as a social unit. In the black-crowned night heron, pairing and subsequent copulation occur on the nesting platform. The territory extends for a short distance around, above, and below the nest. It seems likely that in other species the territory has been extended around the nest site. It must be remembered that territory has four dimensions and has been extended in time as well as in direction around the nest. This extension in time assumes that a type of behavior has been extended beyond its original use and also develops antecedent to its use. The extension of a behavior pattern forward in time is a common occurrence in birds and probably in other vertebrates. For example, certain species of birds begin to "incubate" before the eggs are laid.

The various types of territory in birds have been classified by Mayr (1935) and revised by Nice (1941). It seems worth while to change the order of the classes in an attempt to suggest a phylogenetic sequence of behavior.

A. Restricted to narrow surroundings of nest.
B. Mating and nesting, but not feeding ground
C. Mating, nesting, and feeding ground.
D. Mating station only.
E. Winter territories.
F. Roosting territories.

This order supposes that the place of fertilization was the earliest type and that species increased the number of functions served by the territory to include feeding. It must be supposed that the birds in class D have passed through stage B and then discarded the function of territory for nest building. Also, it must be supposed that classes E and F are extensions of the function of territory. The degree of correctness of these suppositions will only be known after more research.

The origin of territorialism has undoubtedly been closely linked with the function or biological value of such behavior in the economy of each species. Territorialism serves different functions in each class of vertebrates and each species. In general, in addition to its pristine function of securing a location for fertilization or copulation, a second important function of territorialism is to provide food for the young. Since the population in an area is trebled or quadrupled during the nesting season, it is essential that adequate food

reserves be present for the young birds. And this food must be obtained with dispatch, so that the young do not become chilled or exposed to predators during the parents' absence. The territory in many species provides suitable food reserves within a short distance of the nest.

Two consequences of territorialism probably occur. The first is an extension of the range of the species, owing to the centrifugal tendency of such behavior. Since each pair of animals forces other individuals to remain at a distance, the species is obliged to spread out over the available nesting habitats. Young birds seeking nesting locations for the first time probably must seek entirely new localities. Thus in many species the individuals are constantly extending the range. The second consequence is the limitation of population Although data are scanty, at least for some birds the territory can be reduced in size to a limited extent. Hence, a given area can support only a certain number of birds because of the territorial requirements, although colonial territories may overcome this requirement to a certain extent.

When a battle is observed in the field or forest, close observation and careful analysis are required in order to ascertain the motivation. In addition to fighting over territory, other types of combat occur, types easily confused with territorial fighting. Most common is that which has reference to the sex partner (sexual fighting). If a male is seen fighting outside his territory and in the vicinity of a female, it is usually due to the presence of a possible rival male. In the life of fish, sexual fighting is probably more important than territorial fighting. In birds, on the other hand, territorial fighting frequently masks sexual fighting and assumes primary importance.

Another common cause of fighting among members of the same species is the regulation of "peck order," or social dominance. Many vertebrates which live in flocks or schools have a definite order of superiority. A new individual must fight until the proper place is found in the rank of the group. A flock of roosters or of hens, for example, will fight until the social hierachy is established and then, with exceptions due to injury, molting, or age, maintain this ranking by threats which cause an inferior individual to run when approached by its superior. The settlement of the peck order sometimes requires fighting for several days. This type of fighting is not territorial and must be clearly set apart in all descriptions of behavior.

Other types of fighting are interspecific. Some fighting is against predators. The kingbird, which valiantly drives away a hawk ten times his size, is not concerned with territorial boundaries. In fact, both male and female kingbirds will go far outside their territory to attack a predator. Another example of competition between species is the prolonged and fierce fights between starlings and woodpeckers for nesting holes in trees. Such fights may scarcely be considered territorial, because the cavity is the obvious object concerned. However, interspecific fighting may be territorial at times. Even birds make mistakes in recognizing one another. Because of frequent errors of identification, individuals of one species may be mistaken for intruders into the territory of another. Such possible rivals for the possession of the land are driven out as though they were members of the same species. This type of fighting occurs commonly in birds and also in fish.

Each of the aforementioned types of fighting presumably has a different physiological or psychological basis. Certain introductory studies indicate that territorial fighting is based on the endocrine system. In lizards the injection of female sex hormones into males results in a cessation of territorial fighting. In night herons the injection of male sex hormones into females will induce territorial defense. Additional research will undoubtedly indicate the extent of hormonal control of territorial behavior.

REFERENCES

BURT, W. H. *Territorial Behavior and Populations of Some Small Mammals in Southern Michigan. Misc. Publ. Zool. Univ. Michigan*, 1940, **45**, 1–58.

CARPENTER, C. R. *A Field Study of the Behavior and Social Relations of Howling Monkeys.* 1934, *Comp. Psych. Mon.*, **10**, (48), 1–168.

DARLING, F. F. *A Herd of Red Deer. A Study in Animal Behavior.* Oxford Univ. Press, 1937.

EVANS, L. T. Territorial Behavior of Normal and Castrated Females of *Anolis carolinensis. J. Genet. Psychol.*, 1936, **49**, 49–60.

HOWARD, H. *Territory in Bird Life.* London: Murray, 1920.

LACK, D. L. *The Behavior of the Robin.* Part I. *The Life-History, with Special Reference to Aggressive Behavior, and Territory.* Part II. *A Partial Analysis of Aggressive and Recognitional Behavior. Proc. Zool. Soc London*, 1939, **109A**: 169–219.

MAYR, E. *Bernard Altum and the Territory Theory. Proc. Linnaean Soc., N. Y.*, 1935, **45–46**, 1–15.

NICE, M. *Studies in the Life History of the Song Sparrow.* I. *Trans. Linnaean Soc., N. Y.*. 1937, **4**, 1–247. The Role of Territory in Bird Life. 1941, *Am. Mid. Nat.*, **26**, (3), 441–87.

ANY OLD RAGS TO SELL?

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THE story of rags to riches has no better illustration than in the growth of papermaking. This growth has been made in modern times and has progressed from the small mills where paper was made by hand, in sheets one or two square feet in size, to the present mills that turn out tons of paper each day in rolls eight or ten feet long containing thousands of feet of paper. We now have color in our papers ranging the full breadth of the spectrum and we have papers for almost every conceivable purpose, some of them new and others merely revivals of uses nearly as old as recorded history.

A country such as ours uses tremendous quantities of paper and paper products, with the result that we have come to take paper for granted, little realizing what goes on behind the scenes to make these paper products available to us. It is also difficult to realize that the pulp and paper industry as a whole is classed among the largest in the country; furthermore, as stated by D. Clark Everest (president of the Marathon Corporation), "No other industry . . . has anywhere near the number of scientists, technicians and technically educated people connected with it, directly or indirectly, as the pulp and paper industry."

GROWTH OF THE PAPER INDUSTRY

Ts'ai Lun is given credit for making the first paper from vegetable fibers about 1,800 years ago. This Chinese experimenter macerated some inner bark of the mulberry tree and some young bamboo shoots, liberating the cellulosic fibers, which he then suspended in water. He dipped out some of this suspension with a screen—probably made of silk fabric—allowed the water to drain off through the cloth, and then set the screen, carrying the layer of matted or felted fibers, out in the sun to dry. He finally stripped off this layer of fibers and had a sheet of paper. Throughout the intervening centuries the basic processes required for making paper have not essentially changed. The first step is that of separating the cellulose fibers from the parent substance, the pulping process; the second is to treat the fibers so that they will felt together into a strong layer, the beating process; the third is to form the fibers into such a layer; and the fourth step is to drive off the water, these last two constituting the papermaking process.[1]

If the printing press had never been invented, and our books, magazines, and newspapers were published with woodcuts or printed with brush and ink, as is done by the Chinese, then such primitive forms of paper might serve our purpose fairly well. The rapid progress in the graphic arts not only gave papermaking its greatest impetus, but, in modern times, has placed demands upon our papers that were not dreamed of by the printers of the Gutenberg Bible. Higher-speed printing presses require paper of greater strength and ink receptivity, and multicolor offset printing demands paper that does not change dimensions or curl with changing moisture content. Fine illustrations must be printed on paper with a clear, smooth surface. These are but a few of the difficulties confronting the printer who uses new and constantly improving printing processes. The properties of the paper he uses can send him home in the evening muttering in his beard, a menace to his family, or in a mood to take them all to the movies.

For most of its history papermaking was an art (a heathen art at that) which, after it had been introduced into Europe by the Moors, was practiced exclusively by heretical groups. Only in recent years have science and technology been applied to this art. Through this application great strides have been made, not only in the quality and quantity of paper produced, but also in the growth of new and allied industries. Even in the past twenty years there has been phenomenal growth in the size of the industry and in the numbers of technical personnel involved. In 1928 the annual consumption of paper and paperboard products was 12.5 million tons; by 1948 it was 26 million tons, which were furnished to the country by approximately 800 mills.[2] In 1928 there were about 233,000 wage earners in the pulp, paper, and allied products industries earning $287,000,000;

in 1948 there were 394,000 workers drawing $1,063,000,000 in wages[3] Of these, 3,557 were specialists who are members of the Technical Association of the Pulp and Paper Industry. This figure does not include technical personnel not belonging to that organization.

RAW MATERIAL SHORTAGES

Few modern industries possess the heritage that has been handed down to the manufacturer of paper. Throughout its whole history papermaking has been subjected to economic pressures, and in turn it has forced new inventions and the creation of new industries. The first such pressure felt by the papermakers was that caused by shortages of the raw materials from which fibers could be obtained. The first source of fibers was wood, that is, bamboo wood and mulberry bark. By the time the Christian world acquired the art of papermaking (about A.D. 1100) the source of fibers was old linen rags. The tremendously enlarged market for paper that was created by the invention of the printing press brought about an acute shortage of rags; this situation led the more venturesome craftsmen to experiment with cellulosic fibers from grasses, straws, and other plants. This experimenting still goes on The chemurgic program and the government regional laboratories are constantly seeking new fiber sources that can be used for paper and paper products.

To return to the rag shortage problem, it is interesting to realize that the first government "directive" was an order from our Continental Congress demanding that old rags be saved and collected for use in the infant paper industry. In this colonial period,

> Scarcity of paper gave printers more trouble than any other factor. A papermill, built shortly after 1690 on Paper-Mill Run, a tiny stream in Germantown, started the industry, but even the English mills produced less than £25,000 worth of paper in 1690. The absence of sufficient rags, paper's chief raw material, long rendered domestic manufacture precarious and importations costly. The financial insufficiency of the colonists and persistent British opposition contributed heavily to the lag of paper making behind paper using.[4]

THE DEVELOPMENT OF WOOD PULPING

The keen observation of Reaumur, a French naturalist, that wasps' nests were made up of layers of a paperlike material and that bees got their raw material from old wood evolved, eventually, into our modern pulping industry. Pulping processes are designed to obtain cellulose fibers suitable for the manufacture of paper and paper products. For some types of paper products, destroying

Papermaking in Europe in the seventeenth century

the original structure of the wood or other parent material is sufficient, but for higher-quality pulps the pulping process must also free the cellulose fibers from lignin, resins, pectic material, and other noncellulosic constituents.

With the return to the use of wood fibers and the advent of the modern wood-pulping processes, the shortage of raw material appeared to be a thing of the past. That nothing is static or permanent with respect to science and technology is illustrated by the fact that, with apparently unlimited sources of pulp, new industries were created that used this new supply to manufacture new paper products. In 1918 our per capita consumption of paper (all grades) was 121.2 pounds, in 1928 it had increased to 207.8 pounds, and in 1948 it was 358.5 pounds. We are far from self-sufficient in the matter of wood pulp, for this huge consumption of paper products necessitates the importation of more than 2 million tons of pulp from Canada, Mexico, Newfoundland, and Europe. In 1948 we produced 12,872,000 tons of pulp in our own country.

The multitude of paper and pulp articles that our children take for granted and that we deem essential to our everyday life were nonexistent

only a few years ago. For example, about the only paper containers we had in those "good old days" were brown paper sacks and shoe boxes. (What would lunch on the train have been without them?) Now, thanks to research, those brown sacks are white, and the architects of shoe boxes have put "picture windows" in them, and they are being used for everything from their original purpose to the marketing of frozen foods. The exigencies of World War II promoted the application of technology to such things as paper containers that could be unloaded from boats by dumping them into the ocean and letting them float ashore, and to the more difficult production of containers that would prevent the passage of moisture vapor which, in turn, would spoil the contents. The suggestion is made that the per capita consumption of paper and paper products can be used as a barometer to indicate the standard of living of a nation.

THE NECESSITY FOR A CONSERVATION PROGRAM

These greatly expanded uses for wood pulp have put a tremendous strain upon our resources of wood, for it requires approximately 2 cords (128 cubic feet) of wood to make one ton of chemical pulp by any of the standard processes. The free and easy days of the old-time sawmill, with its small investment, and the prodigal use of our timber resources are over. There is too large an investment today in permanent buildings and expensive machinery for the pulp mills to abandon cutover forest land and then move on to virgin stands of timber. We are now forced to grow trees as a crop and replace them as we use them. A Forestry Service report of February 21, 1949, points out, with respect to good forestry management, that the best showing is by the lumber and pulp companies, which together own 15 percent of the commercial forest land in private ownership. Small holdings, which comprise three fourths of private forest land, are said to be "the toughest problem."

> Land owners can now be told, truthfully, that growing and harvesting crops of trees is a profitable business. Many small owners, who once believed that timber land burned over and used for grazing was more valuable than when used as a fully stocked wood producing area, have changed their minds. They now see prospects of more substantial and adequate returns in production of pulp, poles and timber, than in the growing of hogs, sheep and cattle. The profit motive is giving point and purpose to mass education in forest practices.[5]

Of increasing importance is the integrated lumber and pulp mill which utilizes waste material from the sawmill for the manufacture of paper and rayon pulp. This has been made possible by better methods of utilizing the trees, by better methods of bark removal from logs and larger branches, and by good management of the lumber companies. More of such intelligent use of our resources is necessary, otherwise we shall be facing even more critical shortages. Our predictable increase in population in the next ten years alone could cause this.

Conservation as advocated by the Forestry Service and many of the states presents some of the most difficult problems for technology to solve. Many pulp and paper research laboratories are engaged in finding ways of profitably using the lignin that constitutes 20–30 percent of the wood. Synthetic vanilla flavoring and molding resins are two of the more spectacular products that have been developed to commercial use. The research work on lignin and other waste materials from the pulping industry has been intensified by legislation directed toward cleaning up our rivers. The sulfite-pulping process is the worst offender in stream pollution, for, unlike the alkaline-pulping processes, since the chemicals in the cooking liquor cannot be recovered, they have been dumped into the streams. Much expensive research has been devoted to this problem, and at least one possible answer has been obtained. This consists, very briefly, of using either the magnesium or the ammonium bisulfite rather than the calcium compound as the cooking chemical; either chemical can be recovered for reuse. The lignin and other products separated from the cellulose fibers still present a disposal problem. At present they are burned for the production of power. This is but one of the many stream-pollution problems to be solved and constitutes too large a research program for any one mill to undertake. The industry has banded together in the National Council for Stream Improvement (of the Pulp, Paper and Paper Board Industries), Inc., to finance comprehensive work at such research laboratories as the Institute of Paper Chemistry, Mellon Institute, Kalamazoo College, Louisiana State University, Purdue University, Virginia Polytechnic Institute, Bates College, Rutgers University, Manhattan College, Oregon State College, and the University of Washington.

SAUCE FOR THE GOOSE IS NOT SAUCE FOR THE GANDER

To return for a moment to the original economic pressure of rag shortages, what about our supply of rags for paper at the present time? This question is still critical to the mills that make rag-

content papers The reason, however, has changed. The new cotton fabrics developed by textile technologists contain synthetic threads which ruin the rags for papermaking. Thus progress in the field of textiles is forcing the rag-content paper manufacturers to seek new sources for cotton fibers. They are doing the obvious—that is, going back to the original cotton plant for their papermaking fibers. The instability of the cotton market has been a constant deterrent to such a change from rags to raw cotton. This same uncertainty directed the rayon manufacturers to change from cotton to wood pulp, a move which some fine paper manufacturers made when highly purified wood fibers became available. The pulp manufacturers, through their newly refined pulping processes and bleaching operations, have rendered it possible for the papermaker to produce finer grades of paper from these wood fibers than any that were attainable a quarter of a century ago.

MAKING PAPER

The beating process. An essential operation in papermaking is that of treating the fibers so that they will felt together to form a strong sheet. This is a mechanical process of rupturing or fraying out the outer layers of the cellulose fiber so that tendril-like fibrils project from the parent fiber. Many theories have been propounded to explain the process and its effect upon the felting properties of the fibers, but it will suffice to bring out here that the degree of such treatment, or "beating," governs the manner in which the fibers mat together to form a sheet of paper. This operation is critical because the rate at which the water drains from the fibers on the wire (or screen) of the paper machine determines not only the production rate but many of the properties of the final sheet. Means of testing and predicting this behavior are still a controversial subject among paper technologists.

The first "beating" was probably done with a rock or sticks of wood, later on with a big mortar and pestle. Stamp mills were used by the early European paper mills, but the slowness or small production rate and the high power consumption of such devices inevitably led to a more efficient machine. Such a machine was invented by a Dutchman (whose name has been lost to posterity) and

Washing and beating of pulp in Hollander-type beaters.

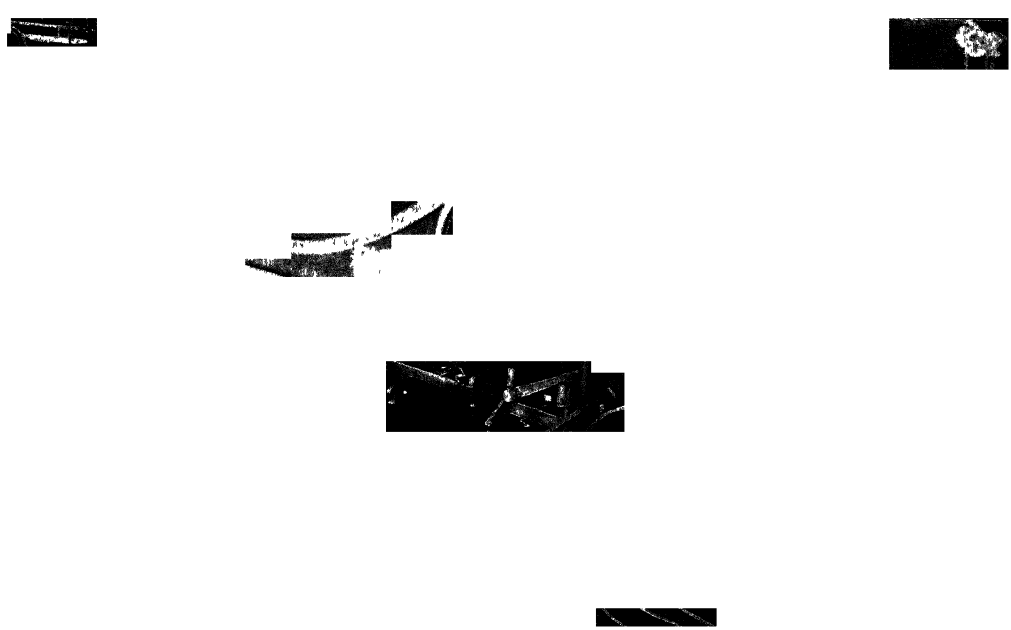
was called the "Hollander." With slight modifications, this type of machine has been in general use for nearly 250 years. Within the past few months experimental information has been released claiming that a new machine puts a "permanent wave" into the fibers and thus increases the desirable felting and strength properties demanded of paper, but without the excessive fibrilation and cutting of the original fibers done by the "Hollander" beater.

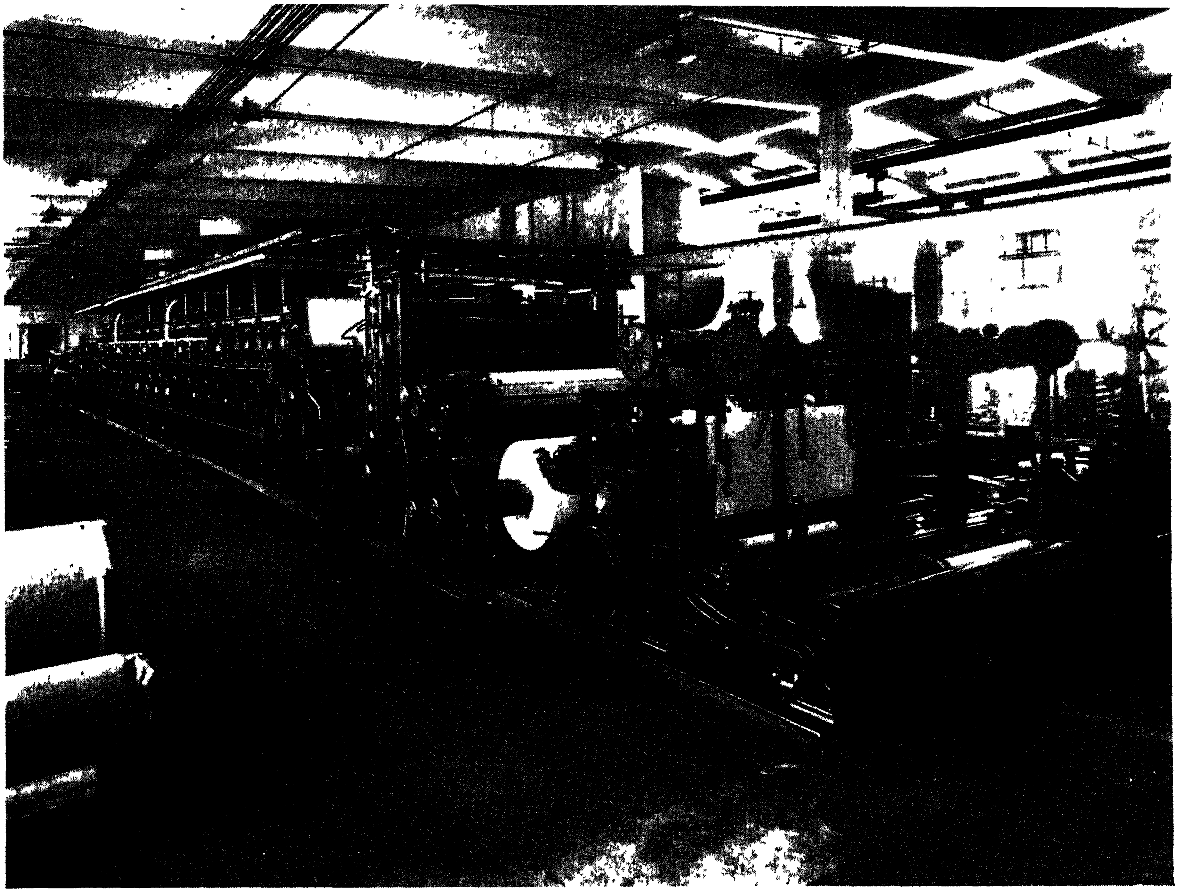
The paper machine. Much energy has been expended by engineers in the design and construction of paper machines that will make paper at a greatly increased rate, assure an even flow of pulp suspension onto the endless belt of wire screen on which the fibers are felted into a sheet, and effect other minor refinements that improve both production rates and quality. The essential features of the machine invented by Louis Robert and developed by the Fourdrinier brothers still remain. These are, in the last analysis, nothing more than applying the fundamental method of Ts'ai Lun to a continuous operation that makes a sheet of paper of indefinite length rather than individual small sheets.

"Sizing" of paper. The ancient Chinese currency, ceremonial, writing, and wall papers required a treatment that prevented the spreading and penetration of their inks and paints. This was accomplished by dipping the sheets of paper into water dispersions of rice flour or starch. The early European papermakers used animal glue for this purpose. In modern parlance this is called "tub sizing," and the two chief raw materials have been starch and glue. Scientists, however, have been responsible for "modified" starches, as well as for improved techniques in the use of both starch products and animal glue They have also developed new cellulose and starch derivatives that are being tested in paper-mill laboratories with the ever-present hope that better papers at lower cost or papers for special uses can be obtained.

In 1800 Illig contrived a new way of sizing paper. His method consisted of stirring a rosin soap in with the water suspension of fibers in the beater and precipitating the rosin on the fibers with an acid or an acid-producing material such as aluminum sulfate. For many requirements no subsequent tub sizing was needed, but, for papers highly resistant to water or writing inks, both types of sizing were necessary. Probably the chief objection to this rosin-sizing method has been that it was not always dependable. Furthermore, it is probably no exaggeration so say that every re-

"Wet end" of paper machine showing the formation of the sheet on the "wire," or endless belt of wire screen





"Dry end" of paper machine showing steam-heated drier rolls, calendar stack, and rewinder

searcher in paper technology has tried to find a better material than rosin for this purpose. Other materials have been found, but the economic competition based upon cost has always been in favor of the cheap and easily handled rosin sizing. Combinations of rosin with other materials, such as waxes, have been developed, but they have not received general acceptance in the industry—except that they are quite useful for special papers. Recently a more successful attack has been made on the rosin-size problem; this is based upon increasing the efficiency of the rosin as a water repellent. Fundamentally, this operation depends upon keeping the precipitated rosin particles in sufficiently fine state of subdivision so that a given quantity will have greater "covering power" in the sheet of paper. Animal glue, casein, soybean protein, cellulose, and starch derivatives have been employed for this purpose with varying degrees of success, and some of them have been developed into patented processes.

The voluminous literature on the subject indicates that the basic theory of the mechanism of rosin sizing has been, and still is, a controversial one. New techniques developed by microscopists have settled the question as to whether the rosin is deposited on the fibers as a layer or as microscopically small particles looking like nodules. The latter having been established as correct, the colloid chemists have been called upon to explain how such a deposit can prevent the flow of inks and other water-dispersed systems around these particles and thus through the paper. The answer has been based upon the phenomenon of "adhesion tension," which is indirectly measured by the angle of contact of the fluid on the solid paper surface. Having been introduced to papermaking, the colloid chemists have had a field day studying many other phases of paper technology. They have found, for example, that variations in the *p*H of the glue tub size have a pronounced effect upon the manner in which it sizes paper effectively. They suspect that the differences in the magnitude of the electrical charges on the cellulose fibers and the surrounding water system in which the fibers are suspended govern the surface activity and therefore the manner in which the fibers felt together. (Work of this nature has recently been started at the Pulp and Paper Research Institute of Canada.) Such underlying research obviously included studies of the structure of cellulose from different sources. Microscopists are making valuable con-

tributions to this knowledge through the use of light microscopes, the newly developed phase contrast techniques, and the electron microscope.

Bleaching. Bleaching is generally accomplished by the use of chlorine or compounds of chlorine. This treatment makes the pulp appear brighter by reflected light by rendering coloring matter soluble (either by chlorination or by oxidation) so that it may be readily washed out from the pulp or rag stock. This result suggests that the operation involves two problems: regulating the action of the bleaching agent on the coloring matter so as to obtain a maximum quantity in a soluble condition without at the same time chemically damaging the cellulose; and washing out the solubilized coloring matter as completely as possible. The introduction of multistage bleaching afforded a means of carefully controlling the reaction on the coloring matter and also a means of washing this material from the fibers at intervals to accomplish its more complete removal. The number of such stages may vary from two to six. The time-honored bleaching agent has been chloride of lime, but in recent years chlorine gas, sodium hypochlorite, chlorites, and hydrogen peroxide have been successfully applied to commercial operations. The success of these newer methods has been due, in no small measure, to better engineering design of bleaching equipment. These improvements in bleaching have made possible the more extended use of such wood fibers as sulfate or Kraft pulp, and this practice in turn decreases our dependence upon a few selected woods, such as spruce and hemlock, for white pulps.

Fillers. The brightness of a paper is obtained not only by the bleaching of the pulp and rag fibers but also by the proper use of fillers and dyes. Fillers have other functions as well. China clay, for example, has long been used as a "loading" material to increase the weight of paper and give it greater opacity. In the fine paper field, brightness and opacity are the properties sought from a filler. If cellulose fibers can be packed so closely together in a paper that there are practically no air spaces between them, a transparent sheet is obtained which is termed "glassine." On the other hand, if the fibers are loosely packed, we get an opaque and bulky paper such as blotting paper. If, however, opacity is to be gained without bulkiness, a material must be incorporated into the paper that has an index of refraction sufficiently different from that of cellulose so that light transmitted through the sheet is scattered. The commercial development that produced titanium dioxide pigments furnished the papermaker with one of his most efficient fillers. Although expensive, its very high refractive index reduces the amount required to obtain the necessary opacity for book and bond papers.

Coated papers. Mention has been made of the smoothness of paper demanded by some of the modern printing processes. This brings us into the field of coated papers, a large and specialized subject in itself. Unlike the tub-sizing process, coating of paper leaves a definite layer of the coating upon the surface. Without going into the technology of this branch of paper manufacturing, it can be pointed out that the old casein-china clay mixes are being supplanted by newly developed proteins, starch, and cellulose derivatives, as well as lacquers, which, combined with new pigments, give greater latitude to the printer in the choice of coated papers. Packaging experts are also finding greater choice in coated papers that are both more decorative and more serviceable.

Technical papers. Probably the most stringent requirements that are placed upon any papers are those of the "technical" papers. This broad classification includes artist papers, tracing papers, and those on which light-sensitive coatings are applied. Art papers require permanence of color and certain intangible properties of "texture." One cause of yellowing of paper is the oxidation of the rosin sizing that slowly takes place during aging of the paper. In comparatively recent years, the producers of rosins have offered a hydrogenated rosin which does not undergo this color-producing reaction and which, for this reason, is recommended for "permanent" paper. Some of the requirements for blueprint paper are that it possess high wet tensile strength and sizing adequate to prevent penetration of the coating solution through the paper. On the other hand, it must not be so water-resistant that it inhibits the absorption of a sufficient and uniform amount of this solution on the surface. Wartime research developed urea-formaldehyde and melamine resins that impart exceptional wet strength to paper, but some of these cannot be used for photosensitive papers because other properties will suffer harmful effects, such as the "spoiling" or predevelopment of blue color in blueprint paper. Some of the newer resins appear to have overcome this difficulty.

Twenty years ago nearly all technical grades were 100 percent rag papers. The depression of the 1930s forced the papermakers to learn how to produce less expensive but equally satisfactory technical papers from wood pulp and rag mixtures.

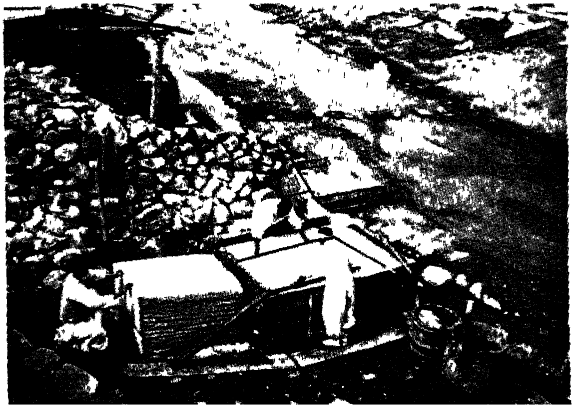
The resultant mixtures of different types of wood pulp and rags, with their many and varying properties with respect to formation on the wire of the machine, and their sizing characteristics, are still problems of the papermaking industry.

These are an illustration of but a few of the difficulties that have confronted the technologists working on technical papers, and, although the final answers may not have been found, considerable improvement in these papers has been made. Then, too, one might ask, What *is* a final answer in scientific research, with its ever-changing horizons?

NEW ALLIED INDUSTRIES

The specialty paper field is replete with new products that have been the result of recent technical developments by the paper technologists. A compiled list of these articles would be long and tiresome reading. A rapid scanning of the index to *The Paper Year Book* shows 53 kinds of bags and sacks, 9 kinds of blotting paper, 13 kinds of book paper, 33 of boxes and cartons, 21 kinds of envelopes, 34 kinds of paper board and 21 kinds of wrapping paper. The 21 kinds of envelopes, for example, mean not sizes but different use requirements. A more interesting way of gaining a realization of these newly developed products would be for the reader to go on a shopping tour and make note of the things he could not have bought as a youngster. A surprizingly large number of these would be found to be made of paper or pulp. Some of them might not be easily recognized, for they appear to be made of "plastics" and molding resins. To give added strength and other desirable properties to many of the molded resin products, highly purified pulps or cellulose fibers are used as "fillers" in the resins.

Many of us can remember watching the construction of a new house and the particular fascination of seeing the lather, his mouth full of nails and a specially designed hatchet in his hand, put up lath with extreme speed and dexterity. No highly skilled labor is needed for that part of construction these days, as large sheets of wallboard are put in place very quickly. Some types of wallboard are sheets of plaster covered on both sides with paper. Other types are made either from by-products or from refuse materials. Bagasse from cane-sugar refining, straw and cornstalks from the farm, and wood pulps that will not make better grades of paper have furnished the raw materials for some of the more recently developed wallboards.



Korean workers using ancient style hand-sheet mold.

Thus it can be seen that the art of Ts'ai Lun, carried on through many centuries with little or no essential change, has, in comparatively recent years, been subjected to economic pressures that have changed papermaking from an art to a technology, forced the growth of the industry to gigantic size, and increased our dependence upon paper and paper products. In some cases these products are poor substitutes for the objects they attempt to supplant on the market, their chief sales appeal being either their novelty or their cheapness. On the other hand, many of them are not merely substitutes but fill very definite needs in our daily lives. In either case the pressures in our free economy present many challenges to paper and allied technologists.

REFERENCES

1. HUNTER, DARD. *Paper Making Through Eighteen Centuries.* William Edwin Rudge; HUNTER, DARD. *Paper Making.* New York: Knopf.
2. *Chem. and Eng. News,* Mar. 7, 1949, 684.
3. *Paper Trade J.,* Feb. 24, 1949, 113.
4. LEE, ALFRED M. *The Daily Newspaper in America.* New York: Macmillan.
5. *Pulp and Paper Mag. of Canada,* April 1949, 124.

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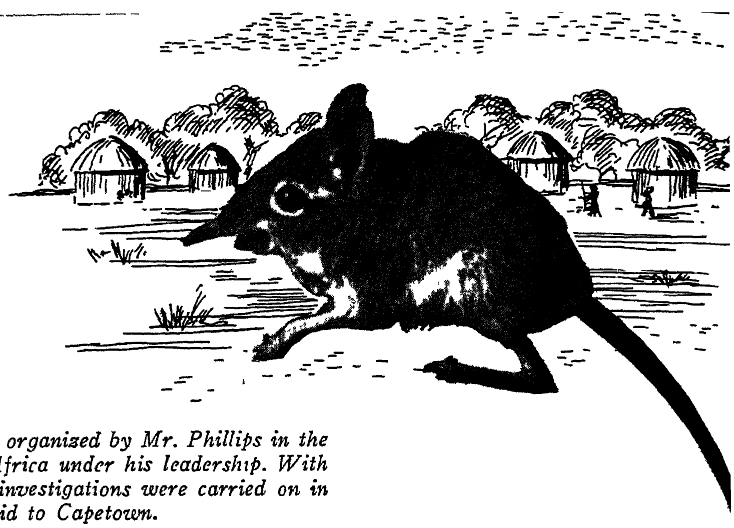


AFRICA FROM NUBIA TO TURKANA

WENDELL PHILLIPS

The University of California African Expedition was organized by Mr. Phillips in the fall of 1947 and spent most of the ensuing year in Africa under his leadership. With the aid of more than fifty scientists and technicians, investigations were carried on in twenty-two countries, from Port Said to Capetown.

UPON the completion of the varied programs of the University of California African Expedition in Egypt and Sinai (see "Recent Discoveries in the Egyptian Faiyûm and Sinai," *Science,* 1948, 107, (2791), 666–70), the major section of the Northern Party, under Expedition Field Executive William B. Terry, traveled up the Nile, crossed the Libyan Desert, and began work at Wadi Halfa in the Sudan.*

The United States Naval Medical Science Group, whose participation had been arranged through the Bureau of Medicine and Surgery (Rear Admiral Lamont Pugh, MC, USN, Deputy Surgeon General) and the Geophysics Division of the Office of Naval Research, in collaboration with the Regents of the University of California, had the following major objectives in Africa:

1) To obtain an intimate knowledge of certain tropical diseases, their diagnoses, clinical manifestations, treatment, and methods of control and prevention.

2) To utilize this information for the prevention, treatment, and control of tropical diseases in the Armed Forces, wherever these diseases might be encountered.

3) To make colored motion pictures and clinical photographs to serve as material for teaching purposes at the United States Naval Medical School, Department of Tropical Medicine.

4) To collect insects, animal hosts of disease, and clinical specimens for teaching purposes.

5) To conduct research in parasites of birds, mammals, and reptiles, with emphasis on parasites related to human diseases.

6) To collect scientific specimens for research workers in civilian institutions, to help fill in important gaps in their scientific collections.

SUDAN

Tropical medicine. The first shakedown of laboratory equipment to be used in the field throughout Africa was made at Wadi Halfa prior to entering the Nubian Desert; parasitological and X-ray equipment, particularly, were given their first workout. Hunting parties began their nightly trips into the desert, discovering that game such as jackal, hyena, desert fox, and hare ranged far and wide, and came close to or even into town. The specimens were photographed and combed for ectoparasites, of which they carried considerable numbers. These were carefully put into preservatives for taxonomic study and correlation with human disease if possible. The animals were then examined for intestinal and other internal parasites, and all these were preserved for further study in America.

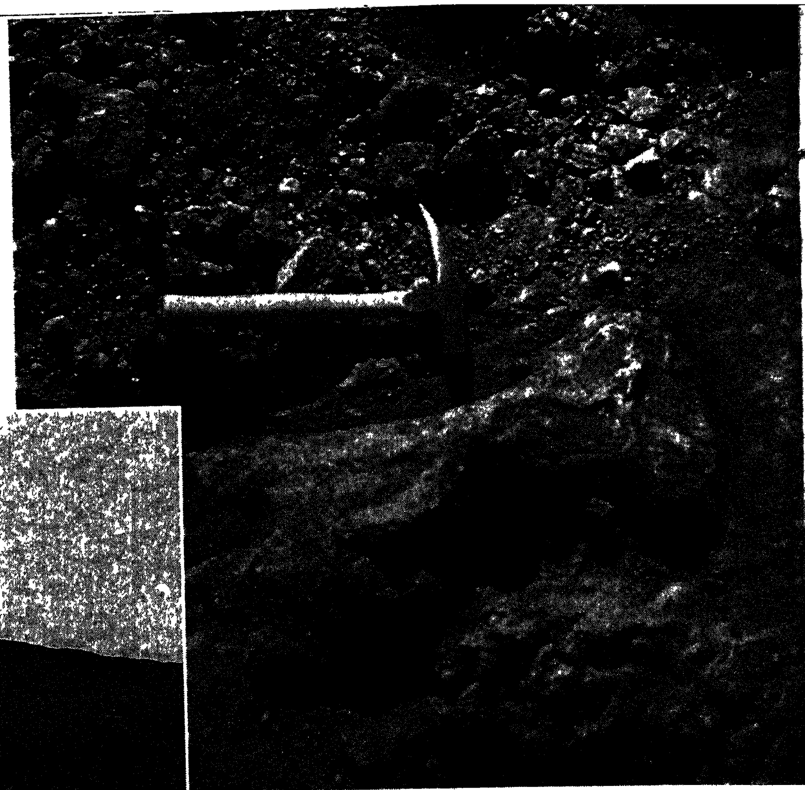
Ward rounds were made at the local native hospital with the Sudanese district medical officer, Dr. Mohammed Ahmed Ali. Many types of fractures were seen at Wadi Halfa, as well as many contagious diseases and other interesting medical phenomena. X-ray pictures were taken for the benefit of the patient and to aid Dr. Ali in the diagnosis and confirmation of his findings.

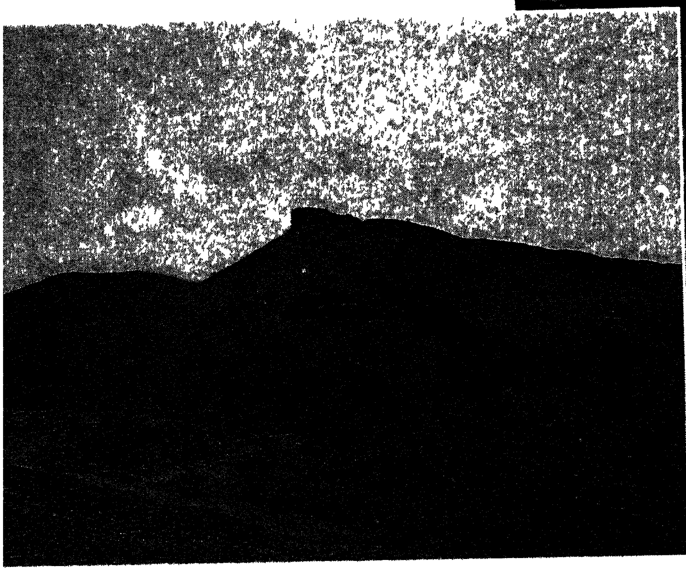
At Wadi Halfa a species of chironomid fly, which came up out of the Nile about sundown, was

* The staff of the Expedition's Sudan phase included Gladys Terry, business manager; Dr. Henry Field, physical anthropologist; Commander Julius M. Amberson, MCR, USNR, medical officer (in charge of the Navy Medical Program from Cairo to Nairobi; Captain James J. Sapero, MC, USN, directed the Medical Program from Nairobi to Capetown); Commander Trenton K. Ruebush, MSC, USN, parasitologist; Harry Hoogstraal, mammalogist-entomologist; Dr. Ernest Schwarz, zoologist; Chief Hospitalman Deaner K. Lawless, USN, medical research assistant; Photographer's Mate 2/c Harley F. Cope, USN, naval photographer; M/Sgt. Charles D. Evans, USMCR, motion-picture photographer (in charge of motor transport in the Sudan after the return to America of Transportation Chief Major G. G. Edwards, USMC, who was injured while serving the Expedition); Keith Marker, motor transport; Richard Alison, assistant mammalogist; and Walter Thompson, technical assistant.

Warriors of the Masai meet to hear an address by the British Governor of Kenya.

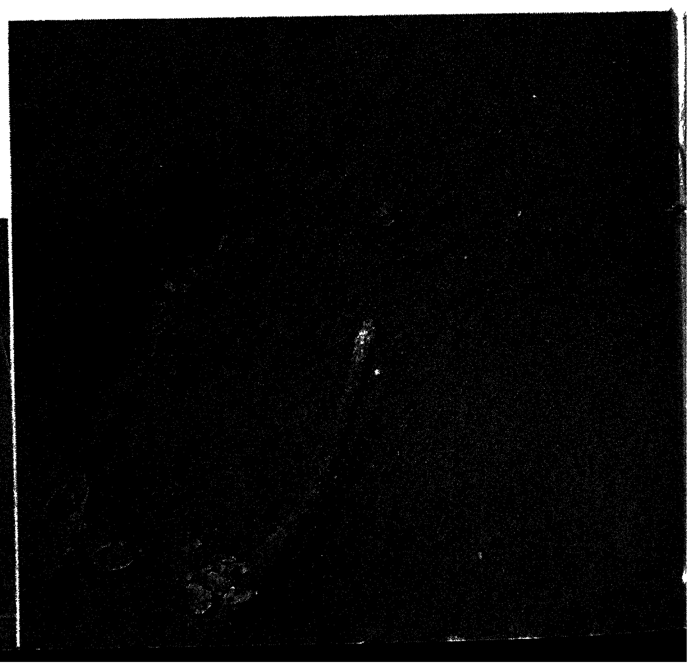
Rhinoceros skull in rock, Turkana



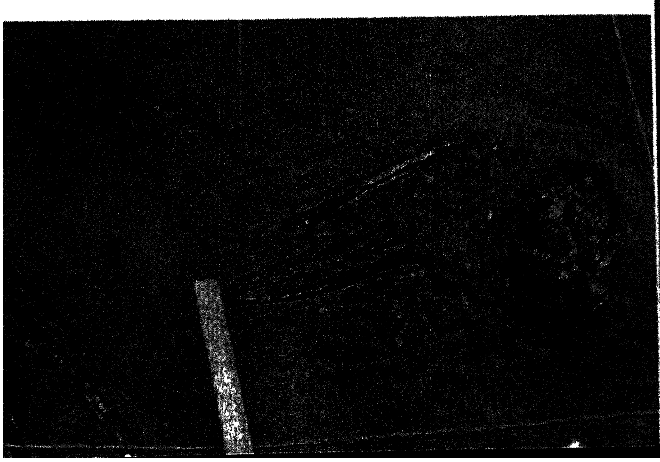


Fossil beds, Kenya Colony.

Mesolithic skeleton, showing artifacts, Turkana.



Skeleton in Neolithic burial mound, Turkana.



Expedition vehicles crossing the Sobat River.

present in enormous numbers. The medical authorities at Khartoum have shown them to be the cause of an asthmatic condition, and to some of the acute asthmatics benadryl was administered with good effect. Diabetes was seen among the Sudanese population both at Wadi Halfa and at Abu Hamed. The direct cause for this is not known, but the fact that the diet of these people is largely carbohydrate, with low amounts of fats and proteins, may be an explanation

At Abu Hamed, on the great bend of the Nile, no doctor was available, but a trained African medical assistant administered to about 10,000 people in this general area. In this latitude there were a few cases of yaws and some malaria, but other than this the diseases were those that are also problems of the European. The black fly *Simulium damnosum* occurs in abundance in this region. This fly is a known carrier of onchocerciasis, a disease caused by a worm that lives in human tissues. In certain areas the worm produces epidemic blindness.

After breaking camp at Abu Hamed, the party crossed the Atbara River, which is the last tributary that feeds the Nile in its northward flow to the Mediterranean; it forms an important barrier between the Mediterranean fauna and the true African fauna. According to Dr. Schwarz, although there is no absolutely sharp line of demarcation, it can be said that the desert fauna, of which the Mediterranean red fox (*Vulpes vulpes aegyptiaca*), the common gerbil (*Gerbillus gerbillus gerbillus*), and the dorcas gazelle (*Gazella dorcas littoralis*) can be taken as index species, fades out from the Atbara River line, and disappears from the Blue Nile at Khartoum, and that the true Sudan fauna, with baboons and many species of true antelopes, appears south of the Atbara. Another important line is the Sobat River, in the valley of which there is a secondary overlap of the ungulates of the Sudan and the Uganda fauna. This is due to the disappearance of the forest screen which connected the Uganda forest with the relict forest of the Imatong and Didinga mountains. That this screen has disappeared fairly recently is shown by the identity of the fauna in these mountains with that of the primeval equatorial forests, as demonstrated by the occurrence in both areas of the blue monkey (*Cercopithecus mitis stuhlmanni*), and the blue duiker (*Philanthomban monticola aequatorialis*). From the equatorial forest the shifts of fauna obviously have an influence on the spread of infections and parasites of which the animals are reservoir hosts. A vegetational change occurs at this point, the plants being greener and of more variety on the south bank.

At Khartoum extensive studies of the medical problems of the entire Sudan were conducted. There were visits to the Kitchener School of Medicine, Graphic Museum, Zoological Gardens, Cotton Research Station at Shambat, and the Veterinary Research Station, and discussions with various missionaries who were in Khartoum or passing through on their way to other outposts Here, also, the first definite information on the location of the elephant shrew was obtained. Donald McClure, a Presbyterian missionary in the Sudan-Abyssinian border country, told us that the animal might be found near Kapoeta, Equatoria, in the southeastern Sudan. In this area Dr. Schwarz studied the distribution of the commensal rats and mice and showed that the Norway rat does not occur anywhere in the Sudan along the Nile itself. The material examined at Khartoum showed that previously published records were incorrect and that the animals identified as Norway rats were white-bellied rats (*Rattus rattus frugivorous*), which belong to the black rat group. The discovery of the white-bellied Mediterranean house mouse (*Mus musculus praetextus*) in Khartoum is the

Tribes of the south-central Sudan include (*left*) Dinkas, and (*right*), Nuers, who wear no clothing.

southernmost record of it in the Sudan so far. It was learned from the Sudan medical people that hydatid disease, or *Echinococcus granulosus,* might be encountered in the same region.

At Wad Medani the first real tropical diseases were found. Cases of madura foot, kala azar, tropical ulcers of the extremities, malaria, schistosomiasis, anemias, and parasitism were observed and photographed. Visits were made to the Agricultural Research Station, where extensive studies in cotton and other economic crops of the Sudan are in progress. The snail hosts of schistosomiasis are also under intensive study here.

Typical clinical manifestation of tropical ulcer.

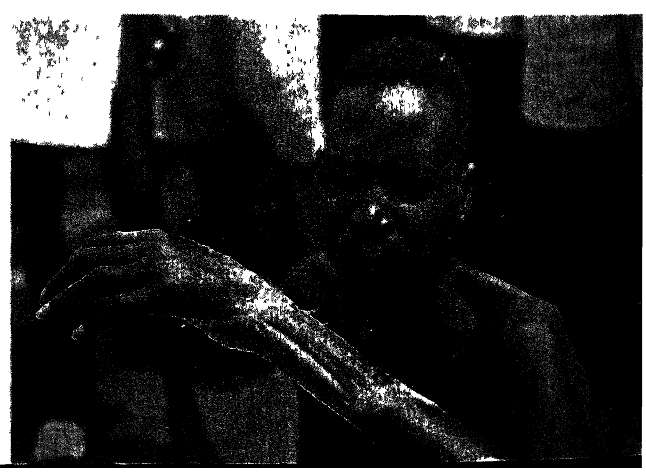
After leaving Wad Medani, we crossed Gezira Triangle and set up camp near Kosti, where tropical malaria was prevalent. Here nets were used for the first time, and all members of the party began to take suppressive doses of atabrine or paludrine. A trip was made across the river to visit the Kosti Native Civil Hospital, where cases of madura foot were present in greater numbers and variety than heretofore. Following a road and trail along the White Nile, en route to Juba, stops were made at Malakal, Er Renk, Paloich, Bor, and Mongalla, which was once the provincial capital but is now in ruins, having been abandoned because of the prevalence of malaria and blackwater fever. The capital has been moved to Juba, which is on higher ground and less infested with mosquitoes. At Bor an epidemic of smallpox was encountered among the natives.

Headquarters for the next six weeks were established at Juba, and from there parties were sent into both the eastern and western portions of Equatoria. At Juba it was found that elephantiasis in the native was due to *Onchocerca volvulus.* This is not a new discovery, but it has been thought that in most places the disease was due to *Wuchereria bancrofti.*

In eastern Equatoria province, Commander Julius M. Amberson, MCR, USNR, assisted a Sudanese doctor with numerous laparotomies on natives ill with *Echinococcus granulosus.* Cysts were obtained in the course of these operations and preserved for the U. S. Naval Medical School. Considerable effort and attention were given to a survey of the blood parasites of the indigenous fauna. At Kapoeta the most interesting find was that of a peculiar malaria in the blood of the elephant shrew. This parasite, it was hoped, would be of some value as a laboratory research subject, and field studies on the parasite and its hosts, in addition to acclimatization studies of the hosts, were made. One hundred and four living elephant shrews were then returned by air to the Naval Medical Research Institute at Bethesda, Maryland. There it proved that the species of malaria had such a peculiar cyclic course that it would have little value for comparison with human malaria; it will, however, shed light on the taxonomic and evolutionary status of the malaria parasites of man and animals, and as such could be very useful.

After the shrews were returned to America an attempt was made to discover the vector of the parasite in the Sudan, but with only makeshift facilities available the problem proved insoluble in the short time allotted. It is hoped to continue this project at some later date. Harry Hoogstraal, who was in charge of the program, and Chief Hospitalman Deaner K. Lawless conducted the field research on the shrew and later were sent back to the Sudan from Kenya by Captain Sapero to continue the project.

In addition to the elephant shrews, live guinea fowl and francolin partridges harboring blood pro-



tozoa were sent back to the Naval Research Institute and are now being studied. These birds are the hosts of *Plasmodium* and *Leucocytozoon*. *Haematozoon* were found in ten reptiles. *Trypanosoma* were found in five types of animals, including the elephant shrew, and several other animals also contained filariae, which are being studied.

In western Equatoria observations were made on West African sleeping sickness (due to *T. gambiense*), blood filariae, and leprosy. A film on sleeping sickness, covering most of the epidemiology, clinical manifestations, and the ecological conditions under which *Glossina palpalis* thrives, was made in this region. Blood slides of the various filariae were obtained and stained on the spot to preserve them for use at the Naval Medical Center. In addition, collections were made of black flies, *Flebotamus*, tabanids, mosquitoes, tsetse flies, filth flies, etc. Hundreds of cases of leprosy, amoebic dysentery, hookworm disease, and the two common forms of bilharziasis, which is probably the worst scourge of Africa, were observed and photographed. A special study was made of the water snails that carry schistosomiasis, or bilharziasis, and comprehensive material was collected for the study of their taxonomy and distribution.

The press of other commitments in East Africa curtailed medical work in this area, which is so rich in interesting tropical diseases.

Anthropology and archeology. This program was conducted by Dr. Henry Field, who left the Sudan Party at Khartoum and flew to Nairobi to carry out anthropometric studies on one hundred and fifty Masai of Kenya. Further anthropometric studies were conducted in East Africa by Professor M. Mitwally, of Farouk University, Alexandria.

The rock drawings discovered at Abka by Oliver H. Myres, of Gordon Memorial College, Khartoum, were examined and photographed by Mr. Cope. Dr. H. B. S. Cooke and Professor Paul Deraniyagala, of the advance party, also examined and photographed these pictographs and were able to furnish Mr. Myres with valuable comparative information.

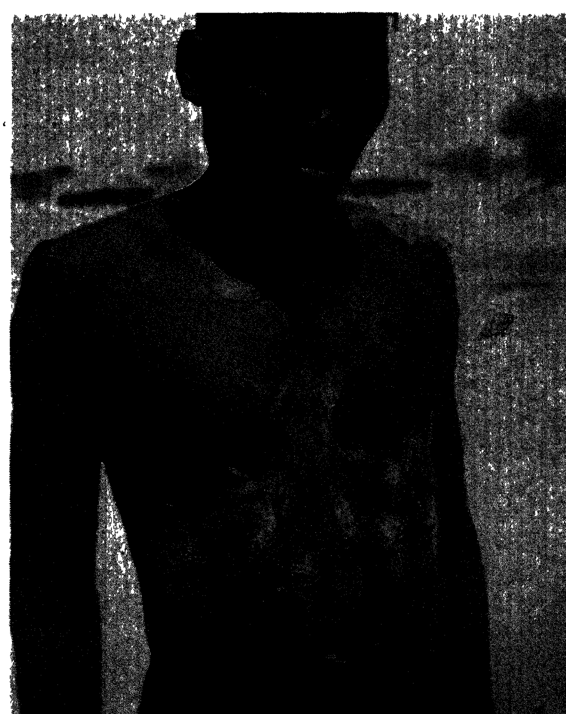
Twenty-seven men in Abka were measured and photographed, and Nubian words and phrases recorded. This was the first anthropometric data to be recorded on the Nubians in the Wadi Halfa area. In the Nubian desert between Wadi Halfa and Abu Hamed, several surface sites containing worked flint and quartz implements, and pottery, were discovered.

Thirty-eight members of the Rubatab tribe were measured at Abu Hamed, and flint implements were collected in the area. At Hagar el-Mirwa ("The Quartz Rock"), thirty miles south of Abu Hamed, scrapers, choppers, pounders, and flakes were collected. Quartz stone pounders and flint, rhyolite, and quartz flakes were accumulated forty-five miles south of Abu Hamed and near Wadi Selim, north of Atbara. Dr. Field and Mr. P. L. Shinnie, Sudan Commissioner for Archeology, and his wife discovered a Neolithic "gouge-culture" station near Omdurman.

Thirty-eight men of the Gumueya (Gumuia) were measured, studied, and photographed at Umm Disa near Khartoum.

NORTHERN KENYA

Vertebrate paleontology and archeology. The Paleontological and Geological Section of the Northern Party, upon completion of work in the Eocene and Oligocene of the Egyptian Faiyum, proceeded directly south by truck convoy through the Sudan, across Uganda to Nairobi, Kenya. Included were Dr. Cooke, geologist (in charge); Professor Deraniyagala, paleontologist; Dr. Robert Denison, paleontologist; Captain George Russell, USA, geographer; M/Sgt. James Houle,



Keloid formations. (Photograph taken at Kapoeta.)

USMC, motor transportation; and David Cohen, scientific assistant. Geological and archeological studies were made en route.

Upon arrival in Kenya, the party refitted and made its way northward to the province of Turkana, which lies in the extreme northern part of the Colony, just west of Lake Rudolf. (Further paleontological investigations were conducted in East Africa by Professor G. H. Awad, of Farouk University, Alexandria, Egypt.) Base camp was established halfway between Lodwar and Ferguson's Gulf at the foot of a hill known to the local natives as Muruarett. During a brief reconnaissance in this area in 1932, Professor C. Arambourg had found vertebrate fossils of supposedly lower Miocene age in sediments interbedded with lavas. These beds, which the present party investigated more thoroughly, were of considerable interest because of the similarity of their fauna to that of the Miocene beds in the Lake Victoria region, which have yielded a number of fossil apes to Drs. L. S. B. Leakey and C. G. MacInnes.

In the immediate vicinity of Muruarett hill there were two small areas in which the sedimentary rocks were well exposed. Within these areas a distinctive dark-red marly tuff was found to be the most productive, the other horizons being nearly barren. This productive series appears to consist almost entirely of material derived from volcanic ash, carried by shallow waters and laid

Madura foot, result of infected thorn wounds.

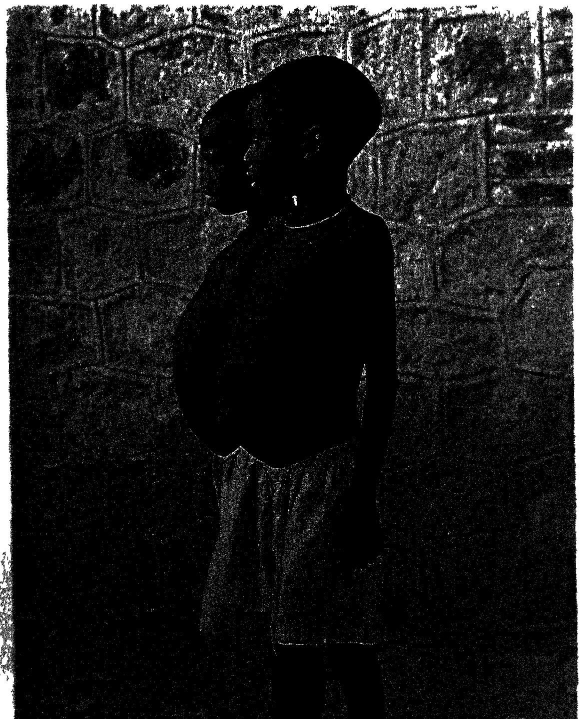
down under deltaic conditions. The specimens collected were mostly surface finds of fragmentary or isolated skeletal parts, although occasional more nearly complete specimens were discovered, including one partially articulated skeleton. Intensive and detailed prospecting of these two small areas produced the major part of the collection from Turkana.

Reconnaissance prospecting trips were made, particularly within twenty miles of camp. Similar sediments interbedded with lavas were found, but they were for the most part unproductive. Only the region around Muruarett hill produced any sizable collection. At another locality below the peak known as Lothidok, a huge turtle, approximately 4.5 x 6.5 feet, was discovered and partially uncovered, but it was found impractical to move it because of the logistic problems involved.

Notable among the specimens from Turkana were fragmentary jaws of two apes, the first to be discovered in this region.†

In general, the first fragment emphasizes the relatively close kinship of the fossil apes named *Pliopithecus* from the Miocene of Europe, *Dryopithecus* from the Miocene and Pliocene of Europe, *Sivapithecus* from the Pliocene of India, and *Proconsul* and allied genera from East Africa. It probably represents one of the smaller species of the *Proconsul* group, but until closer comparison with these forms can be made, a more exact de-

† The writer wishes to express his appreciation to Professor William K. Gregory, American Museum of Natural History, for a detailed analysis of these two specimens, which will be published later.

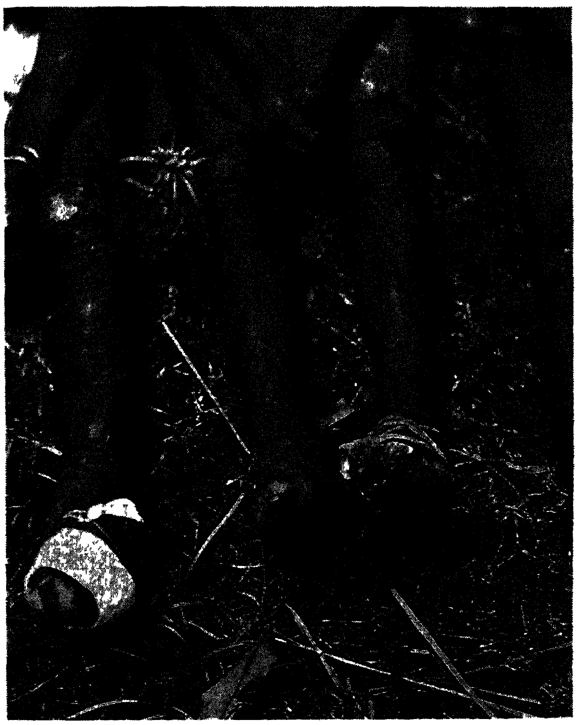


Children of the southeastern Sudan, showing evidence

termination seems inadvisable. The second fragment strongly recalls the corresponding parts in *Proconsul africanus* Hopwood.

Other fossil mammals included antelopes, pigs, elephants, carnivores, and rhinoceroses. Aquatic forms in the fauna were represented by crocodiles, turtles, gastropods, and pelecypods.

Three human skeletons from much later deposits were discovered near camp. The older human burial was associated with microlithic tools and occurred in a river terrace deposit, which has been tentatively identified as Mesolithic in age. The younger skeletons found buried under rock mounds had no artifacts with which to date them, but they are undoubtedly ancient. Professor Deraniyagala, who discovered and excavated two of the skeletons, first drew my attention to the possibility that several of the mounds of boulders situated around the base of Muruarett hill might represent Stone Age burial tumuli.



Advanced cases of leprosy in the Sudan.

Human burials covered by stone mounds occurred at many points on the hundred-meter terrace, but not at much lower levels, suggesting that the mounds were not very remote from the lake shore at the time they were built up.

Geological reconnaissance was carried out over a wide area and served largely to confirm the excellence of the work conducted by Arambourg in 1932. There seems little doubt that rift faulting and folding has affected the whole region. The main escarpment of the Rudolf rift on the west side is thought to be concealed by Pliocene and later lake deposits. Raised beaches occur about one hundred meters above the present lake level and contain a few stone implements. Large numbers of microlithic tools occur on the surface. Study and description of implements from Turkana are being undertaken by Dr. L. S. B. Leakey, curator, Coryndon Memorial Museum, Nairobi.

A number of very black and phosphatized fish vertebrae were found in the beach gravels at Ferguson's Gulf. They closely resemble the material from the Omo beds of Abyssinia at the extreme northern end of the lake. Their occurrence a hundred miles south of the southernmost outcrop near Todenyang is suggestive of a former greater extent of the Omo beds (Lower Pleistocene), which are probably concealed by younger deposits It is possible that further exploration will reveal some outcrops at isolated areas around the periphery of Lake Rudolf.

The region north of Lodwar to Lokitaung on the Abyssinian border was investigated. Although the series of volcanic rocks is well exposed throughout this region, no accessible areas of fossil-bearing sediments were found. Near Namaraputh fort small exposures of the Omo beds were examined, and a few mammalian and fish fossils were collected.

The story of the remainder of the University of California African Expedition, which included investigations in Kenya, Zanzibar, Uganda, Tanganyika, the Belgian Congo, Northern and Southern Rhodesia, South Africa, Mozambique, South West Africa, and Madagascar, will appear in a forthcoming issue of The SCIENTIFIC MONTHLY.

Photographs on page 264 by H. B. S. Cooke; top, page 265 by C. D. Evans; all others by Harley F. Cope, USN.

Guinea-worm emerging from left wrist. Another is about

SCIENCE ON THE MARCH

THE METEOROLOGY OF OTHER PLANETS

THERE has been a recent revival of interest among astronomers in the problems of the physical constitution of the planets, which had become overshowed by the very rapid advances in stellar astronomy. It is now realized that the powerful methods of astrophysics, both observational and theoretical, can be applied to the planets with great success.

Thus G. P. Kuiper, director of the Yerkes and McDonald observatories, has begun an attack on a broad front upon the problems of the planets and their satellites. His observations of the infrared spectrum of different parts of the planet Mars have shown conclusively that its polar caps consist of water (probably in the form of hoar frost). He finds also that the green areas on this planet cannot be due to higher forms of plant life as had often been supposed, since chlorophyll is absent. Also at the Yerkes Observatory, S. Chandrasekhar has achieved a detailed theoretical solution of the intricate problem of the illumination of planetary atmospheres. A third development is the cooperative program undertaken by the Lowell Observatory and the U. S. Weather Bureau, which has shown that modern meteorological theory can be fruitfully applied to the atmospheres of the other planets.

A very interesting new approach to the study of planetary atmospheres has been opened by the German astronomer Wilhelm Becker. His original intention was to determine whether the brightness of the sun varied appreciably. Since the direct determination of the apparent magnitude of the sun is technically exceedingly difficult, Becker instead considered the brightness of the planets, which are much easier objects of photometry. Accordingly, he collected, and reduced to a uniform scale, all measurements made since 1850 of the brightness of the planets Mars, Jupiter, Saturn, Uranus, and Neptune. All these observations were corrected for the effects of the varying distances of the planet from the earth and the sun, and for the effects of phase.

This laborious compilation uncovered unexpected phenomena of great interest. All five planets showed conspicuous slow changes in brightness. These changes were unquestionably real, as independent series of measurements by different observers showed the same variation in planetary brightness. Nevertheless, these variations could not be attributed to the solar variability, for the planets did not increase or decrease in brightness simultaneously. Clearly, the cause must lie in the atmospheres of the individual planets.

Consider the case of Mars. For intervals of several decades, this planet remains at constant brightness, but will then brighten by 0.5 magnitude, and in a few years fade again to its usual state. This behavior is just what would be expected if the usually clear atmosphere of Mars would from time to time become hazy for a few years; the greater reflecting power of the haze, as compared with the relatively dark surface of the planet, would cause a temporary increase in the brilliance of Mars.

The behavior of Jupiter is quite different. This planet waxes and wanes rhythmically in brightness, with a range of 0.3 magnitude, periodically in a cycle of twelve years. It is noteworthy that certain cloud features in Jupiter's atmosphere undergo periodic alterations in color synchronously. Thus, the English astronomer A. Stanley Williams showed that the equatorial zone of Jupiter varies between red and white in a twelve-year cycle. Jupiter is brightest when the equatorial zone is reddest. The cause of these interlocking changes is obscure; the near equality of twelve years with both the length of the sunspot cycle and the period of Jupiter's orbital revolution around the sun offers two hints, both of which may prove misleading.

The light variations of Saturn are quite unlike those of the two planets just described. Saturn usually remains at maximum brightness, but at irregular intervals, of the order of about a decade, will decline in light by about 0.3 magnitude, and regain its former brilliance after two or three years. This appearance is much as if the bright cloud surface of Saturn were liable to a temporary gray veiling. It is a noteworthy circumstance that the appearance of brilliant short-lived white spots on Saturn in the years 1877, 1903, and 1933 has in

all three cases occurred when Saturn has been abnormally faint.

For Uranus more than 4,400 observations of brightness made between 1864 and 1948 were examined by Becker. This richness of material is fortunate, for the case of Uranus turns out to be more complicated than for the other planets. First there is a very well-marked eight-year periodicity in which Uranus varies by 0.3 magnitudes. Underlying this is a very slow change in brightness, which arises from the spheroidal shape of the planet. Uranus is unique among the planets in that its axis of rotation lies nearly in its orbital plane. Accordingly, when in 1903 and 1945 its pole pointed toward the earth, the largest cross section of the planet was presented to a terrestrial observer, so that the planet appeared relatively bright. This light variation shows two maxima and two minima during the eighty-four-year orbital period of Uranus. Becker was able to calculate from the observed amplitude of these brightness changes how great the polar flattening of Uranus is; his figures are in very fair agreement with those from the direct micrometer measurements of the shape of the planet.

The intrinsic variations in the brightness of the outer planets are to be counted among the most significant discoveries of recent years in the astronomy of the solar system. Although these phenomena still await detailed interpretation, it is evident that these changes arise in the planetary atmospheres. Accordingly, the brightness of a planet is to be regarded as an index of the meteorological conditions in that planet's atmosphere.

Becker has raised the interesting point that it is entirely reasonable that the brightness of the earth, as seen from another planet, may vary as the amount of cloudiness in our atmosphere changes. And, as he remarks, in this connection the long-period terrestrial climatic cycles suggested by Brückner and others take on a new significance.

REFERENCES

1 KUIPER, G. P. (Ed.). *The Atmospheres of the Earth and Planets.* Chicago. University of Chicago Press, 1949.

2 BECKER, W. Sitzungsberichte d Preussichen Akad. d Wissenschaften. *Phys-Math. Klasse,* 1933, **28,** 839

3 BECKER, W. *Astronomische Nachrichten,* 1949, **277,** 65.

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BUILDING MENTALLY AND PHYSICALLY STRONG HUMAN BEINGS

ACCORDING to authoritative reports from all over the world, more than half the people on the earth are existing on marginal rations, or just enough to keep them alive. If a state or nation is to be strong, its people must be strong, mentally and physically. The human body will produce such a result if, from birth, children receive food containing all the necessary elements.

The composition of the human body is less than 6 percent mineral, the principal minerals being phosphate and lime. When children get a marginal ration from birth, the first deficiency easily determined is in the bones of the hands and feet. Figure 1 shows the hand of an eight-year-old boy who has never had enough to eat; Figure 2, the hand of a normal boy who has had the proper food from birth. Note that some of the bones are missing and some are small and underdeveloped in the hand of the boy who has received only the marginal ration. Children on such rations will show definite physical and scholastic deficiencies, and if we attempt correction by feeding a well-balanced ration, authorities tell us it takes two to four years to show any visible results.

Even though the analysis of the composition of the human, or animal, shows him to be less than 6 percent mineral, what will happen when available minerals give out? When bromine was wanted to produce tetraethyl lead to stop the knock in gasoline, William J. Hale told us to go to the ocean. When a metal lighter than aluminum was needed for airplanes, Hale again said go to the ocean, and get magnesium. Now other elements are being taken from the ocean, and it is claimed that they are cheaper than those taken from the earth. A combination of chlorine, soda, and some other elements is called "sea soil," which may contain the elements necessary to complete the cycle in agriculture. Perhaps this will enable us to grow plants that will supply even the trace elements that are so vital if a plant is to produce perfect food.

Table 1 gives an analysis of a product (not "sea soil") taken from the ocean and now on the market. It will be seen that it contains the elements and minor elements. The sea contains all the elements in such abundance that they cannot be exhausted.

When the world gets hungry, it cries for protein foods and for fats. The University of Florida Ex-

TABLE 1

	Percentage
Mg as MgO	82.5
Ca as CaO	11 3
Cl as NaCl	1.1
SO_4	1.86
SIO_2	1.91
R_2O_3	2.65 (corrected to 1% or less)
Mn	0.097
P_2O_5	0.109
F	0.039
B	0.024
Br	0 004
K	0 030
Al	0.25
I	0 00005
Cu	0 0003
NI	0 0002
CC	0 00005

periment Station has shown that just one summer legume (Fig. 3) has returned to the soil 104 pounds of organic nitrogen per acre and has repeated this for four years. Commercial organic nitrogen fertilizer would cost at least $10.00 per unit, making nitrogen cost fifty cents per pound in terms of protein food. An acre of this legume will return at least 640 pounds of proteins. When our soils have lime, with a reasonable amount of phosphate and potash added, a favorable condition is created for growing legumes, and by inoculating the seed when they are planted, little nodules which form on the roots will house the organisms that fix the atmospheric nitrogen. These organisms can be bred like plants, so that strong organisms take in more nitrogen from the air than weak ones.

Fig. 1. Effects of malnutrition on bones of the hand of an eight-year-old boy.

Fig. 2. Bones in the hand of a normal eight-year-old boy.

The nitrogen-fixing organisms vary according to the different strains of legumes. They multiply within the hour. On a good stand of legumes, raising four plants per square foot, there would be on an acre, according to one microbiologist, 52,272,000,000,000 of such organisms. The amount of nitrogen above every acre is 95,256,000 pounds.

The southeastern section of the United States is one of the few places in the world that has phosphate and lime at its door, heavy rainfall, an abundance of sunshine, and soil and plant activity over the entire twelve months of the year. In Florida, the nitrogen-fixing organisms work night and day, twelve months in the year, in contact with atmospheric nitrogen under pressure. In some sections of the country, there is less than six months of soil and plant activity, so enough food must be harvested during the active months for the remaining months.

Knowing what we do, is it not reasonable to suppose that the South could furnish more of the principal elements—phosphate, lime, and atmospheric nitrogen—in abundance than any other region? And doesn't it stand to reason, since we have twelve months' activity of the soil, that careful research by chemists, physicists, microbiolo-

gists, and plant breeders could result in great contributions to the world's food supply? Thus the South could not only increase the mental and physical strength of future generations but do it faster than any other section of the country.

Progress in every field follows broad, intensive, practical, scientific research. Step by step, research will point to the correct course. In my opinion, however, the proper and necessary scientific research is not now being carried on.

One reason this problem appeals to me so strongly is my experience in Germany something over fifty years ago, before I came to Florida. Contact with the average German impressed me with his superior mental and physical strength. Undoubtedly the Germans were at that time the strongest people in the world. You needed no statistics—you simply went from Germany to France, from Germany to Spain, or from Germany to Italy, and you were conscious of the difference. When I arrived in Florida, one of my old associates was operating the first large phosphate business in that state. His company owned its own terminals in Jacksonville, mining high-grade phosphate, running 75 percent bone, phosphate, and lime. Every pound of this phosphate was loaded aboard ships and sent to Germany. Up to the time of World War II Germany was getting nearly one third of the phosphate produced in Florida.

When I read reports from other states—Ohio, Michigan, Wisconsin, and nearly all the others—they show deficiencies in food. We are shipping billions of pounds of food to Europe—which is fine, but we are also shipping soil fertility, and rapidly approaching a period when our entire population may be on a marginal ration. There is a great deal of talk about this, but little is being done.

What I am trying to do in a small way in Florida is to balance the soil so as to produce a perfect animal, for I believe the food from such soil

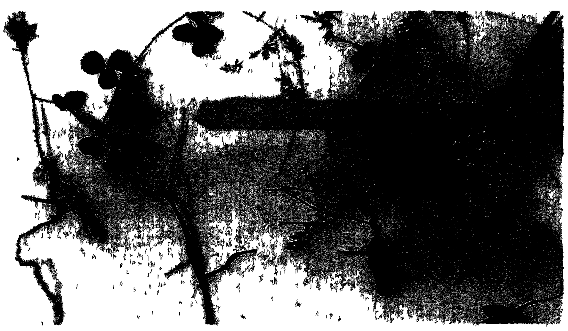

Fig. 3. Some of the legumes that grow wild in Florida's acid flat woods soils.

Fig. 4. Steak from a calf barely six months old.

will produce future generations of perfect humans. We have a little acreage where livestock have been grazing without added food for five years. We have never fed them. Figure 4 shows a steak from one of these animals at six months of age. This livestock can be improved by breeding, and we can greatly improve and enrich the soil, particularly by scientific research in the naturally-occurring legumes and the nitrogen-fixing organisms.

When we learn more about how to use the elements in the air, in the earth, and from the sea, learn to breed plants and microorganisms, we should be able to produce enough good food to satisfy the present world population, as well as expected increases in population, increase soil fertility, and at the same time build stronger human beings, both mentally and physically.

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BOOK REVIEWS

TWO NATURALISTS SPEAK TO STUDENTS OF THE SCIENCE OF BEHAVIOR

George Ellett Coghill, Naturalist and Philosopher. C. Judson Herrick. 280 pp. $5.00. University of Chicago Press. Chicago.

BOOKS are news. Noteworthy news when a distinguished naturalist and philosopher writes at length about another distinguished naturalist and philosopher with whom he was intimately associated over a period of forty years. C. Judson Herrick, as a labor of love and particularly as a service to scientific youth, has prepared a most unusual volume of biography, commentary, and dialogue concerning the life and work of George Ellett Coghill. Indeed, the volume is really three books in one, each book quite distinct in subject matter and method of treatment. They constitute a unique memoir, and incidentally afford many pleasing autobiographic glimpses of their author, whose devotion to the science of behavior has paralleled that of his subject.

The construction of this three-way volume is interesting. Book One is a sympathetic, intimate biography of Dr. Coghill, a personal history of his scientific career, not omitting disappointments and frustrations, but emphasizing his pertinacious adhesion to research goals. Book Two is a carefully wrought analysis of Coghill's scientific method and of his contributions to the interpretation of processes of growth in their relation to psychological processes. The treatment here is appropriately made somewhat genetic by tracing the growth of principles chronologically. Principles matured slowly in Coghill's mind, and bore fruit in philosophical reflections, which, however, were formulated in only a fragmentary manner. Book Three rescues these fragments and places them in an arresting frame of reference, using the literary device of the Socratic dialogue. The reader is invited to "imagine the two of us, George Coghill and I, lounging comfortably before an open fire and letting the talk drift at will over the whole range of our interests." Before the embers die the talk touches and even penetrates age-old problems of structure and function, the psychoorganismic individual, space, time and space-time, purpose, causality, and freedom.

These three books are by no means unrelated, and are woven into an interpretive whole. Herrick takes the position that Coghill's work is a singularly integrated scientific product and that to estimate its import one must know the man whose life it expresses. Herrick has a thesis:

> The traditional code of science—that is, the objectives sought and the methods of investigation—cannot satisfy the requirements of our critical times, and this is why science has failed to measure up to the opportunities and obligations before it. . . . The time has come to recognize the humanistic significance of science—so-called pure science, I mean—and to adjust our practice accordingly.

It is this outlook upon modern science which will make Herrick's book peculiarly suggestive to students of behavior and to young scientists who have to reckon in some measure with concepts in the fields of theoretic biology, psychology, and philosophy. The book does not preach: but it is replete with factual data, which provide clue and orientations. It is the kind of book that encourages reflective thinking. Take Chapter 16, for example. In less than seven pages and in thirteen cogent theses there is presented an analytic summary of the processes of growth, as conjointly disclosed by the lifework of Coghill, the anatomist, and of Herrick, the comparative neurologist. Here is food for active thought; the same sort of thought that set Coghill going on his long and productive career of research and teaching.

"Even as a undergraduate student," says Coghill, "I became aware that the rational approach to the kind of psychological information I wanted, lay through the physiology of the nervous system. Obviously, also, the physiology of the nervous system must be approached through its anatomy, about which I knew nothing." And so for some thirty years he made minute studies of the genetic and functional histology of *Amblystoma*. For a long period he worked in relative isolation, and papers fundamental to psychology were reported to fellow-anatomists. In 1928, University College, London, and colleagues in the Institute of Anatomy invited Coghill to deliver the three lectures that appeared in a volume entitled *Anatomy and the Problem of Behavior*. This slender volume bids fair to become a classic. Englishmen may rightly claim that they revealed Coghill to American science. Herrick's book continues this revelation.

The titles of the basic lectures are worth recalling: *Lecture I.* The Development of Behaviour and its Anatomical Explanation in a Typical Vertebrate; *Lecture II.* Dynamic Antecedents of Neural Mechanisms; *Lecture III.* Growth of the Nerve Cell and the Interpretation of Behaviour. The final lecture dealt with learning in terms of behavior, the limitations of the neurone concept, conditioning, individuation, "forward reference," Gestalt, and growth as the creative function of the nervous system. The mere enumeration of such topics indicates how extensively Coghillian principles have in the past two decades pervaded the domains of biology and psychology.

Coghill concluded his London lectures with a characteristic comment which was philosophic, but which grew out of prodigious research:

The real measure of the individual, accordingly, whether lower animal or man, must include the element of growth as a creative power. Man is, indeed, a mechanism, but he is a mechanism which, within his limitations of life, sensitivity and growth, is creating and operating himself.

The student who is seeking his own measure, and who is wondering about the measure of mankind, will be repaid by a reflective reading of Coghill and of Herrick on Coghill.

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FAUNAL STUDIES

Birds of Concord. Ludlow Griscom. 340 pp. Illus. $5.00. Harvard University Press. Cambridge, Mass.

IN THIS, the second of a series of faunal studies from areas of Massachusetts, Ludlow Griscom has excellently contrasted the records of William Brewster, noted early member of the Nuttall Club, with the detailed studies of the present generation. The area studied centers around the Sudbury and Assabet river valleys, which join at Concord to form the Concord River. Several basic factors are considered by Griscom in his study of the local avifauna of the Concord region. The geology and climate which condition the flora and control the growing season and composition of the flora are considered first. The flora comes next. Land birds of eastern North America occur only where there is vegetation. Following the vegetation, which supplies shelter and food, the birds themselves are considered. Regarding them, Griscom says, "Whatever their requirements and limiting factors are, these birds were conditioned long before the Concord region, in any modern sense, existed." Those birds which occur in the Concord region are the ones that find conditions from favorable to just within the limits of their tolerance. The white man and his civilization are considered as basic factors affecting population trends, as is the power of a species to take advantage of changed or changing conditions. Finally, long-range climatic changes and extremes in climatic cycles are considered in setting the stage for population trends in birds. Each of these basic factors is well presented and supported by local illustrations, factual data, or other evidence.

Having completed this background, Griscom treats birds as a class. This section includes a discussion of temperature tolerance, food requirements, habitat preferences, reproductive capacity, and other features, such as mortality, distribution, and adaptability. Griscom's concluding proposition, based on his review of early studies, consideration of the basic factors and of birds as a class, is that "an absolutely static population of all species of birds for any length of time is a biological impossibility and contrary to common sense!"

Following a short section on fluctuations in numbers of animals and birds, which is filled with factual data, population declines and increases of the Concord region are treated. This is the most valuable section in the book. To illustrate, this section contains data on the American egret and wood duck, that have been recorded periodically in the Concord area since 1650—three centuries of observation and information describing population trends.

A systematic list of birds of the Concord region completes the volume. It contains sixteen photographic illustrations, all of which were obtained from the National Audubon Society.

Birds of Concord is a scholarly contribution. It should be of great interest to biologists and ecologists, as well as to ornithologists and bird lovers. It is unfortunate that a more appropriate title was not selected for this valuable volume.

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HEREDITY VS. ENVIRONMENT

The Nature-Nurture Controversy. Nicholas Pastore. xvi + 213 pp. $3.25. Columbia University Press. New York.

RIGOROUSLY objective as biologists and psychologists may try to be, the more careful and sensitive ones realize that they tend to be influenced in what they see, in how they weigh facts, and in what they conclude by their own cultural backgrounds. Comparative cultural anthropology and the sociology of knowledge promise to help them and other scientists to understand better and to compensate somewhat for the idea systems they took on as children.

Pastore's study of the nature-nurture controversy is an effort to show, through a detailed analysis of the writings of twenty-four scientists who made their major contributions in 1900–1940, how their ideas on nature-nurture correlate with their liberal or conservative social ideas. This is a difficult task. The list of twelve hereditarians and twelve environmentalists is brief, careful, but of course subject to criticism. A longer list would have presented, among other problems, the complexities of adequate analysis. The author mentions a number of other individuals who were considered and dropped either "because they did not express themselves sufficiently extensively to make their inclusion worthwhile" or "because their positions were quite similar to those who have already been discussed." None, he says, was omitted because of sociopolitical outlook.

Pastore's use of terms is a common-sense and workable one. He applies the term "conservative" to those who are "pessimistic with regard to the potentialities of the average person or who [are] critical of attempts to broaden the participation of the citizenry in governmental affairs. Acceptance of the *status quo* is also taken as indicative of a conservative orientation."

He characterizes as "liberal" those who believe "in the necessity of change" and who are favorably disposed "toward the possibilities of the average man and toward the democratic concept." The "radical" are, of course, interested "in the necessity of thoroughgoing change in social, political, and economic institutions."

In terms of these definitions, Pastore found that eleven of the twelve environmentalists were either liberals or radicals, and eleven of the twelve hereditarians were conservatives. The exceptions in the two groups were, respectively, John B. Watson and Lewis M. Terman. Pastore therefore concludes that "the sociopolitical allegiances of the scientists were a significant determinant of their position on nature-nurture questions." He also points out, from his extensive survey of the literature, that the "nature-nurture controversy, qua controversy, has been sustained by the conflicting social philosophies of the scientists."

Free scientists in the United States can behold, understand, and lament the spectacle of a politically reinforced nature-nurture orthodoxy which has developed and has been promulgated in the USSR. But it is all the more important for free scientists to learn how to understand their own social preconceptions and those of their fellows. They can only thus keep their cultural backgrounds from weakening their ability to see and to understand. Pastore's work is a useful contribution to the development of the sociology of knowledge as a tool for the further development of scientific method.

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SUGAR'S VERSATILITY

Sugar: Its Production, Technology, and Uses. Andrew Van Hook. ix + 155 pp. Illus. Ronald Press. New York.

THE magnitude of the world sugar industry and the importance of sugar in our diet make a book on this commodity a very necessary addition to popularized science. This book is brief, with a reading time of three hours or less, and it attempts a review of the production of sugar cane and sugar beets, the processing of these crops to refined sugar and by-products, and the world production and trade in sugar. Space is also given to the chemical nature of sugars and the historical development of their use and manufacture. It seeks to present a general picture for the beginner and others who wish to become informed about the sugar industry.

The difficulties of presenting so many chemical, technical, and statistical facts in terms understandable to all were not fully overcome. Definitely the reader will be impressed by the complexity of the structural chemistry of sugars, by the lengthy procedures needed to prepare commercial white sugar, and by the importance of sugar historically and in world trade. It is also certain, however, that he will be somewhat confused. This will be due partly to the lack of unity in the material included under specific headings and to the numerous digressions. One example is the inclusion of a discussion of corn-sugar manufacturing in the chapter on Production—Cane. Many of the fine illustrations are not placed near the subject matter with which they are associated.

It may be a matter of personal bias, but probably many others will also feel that too much space is given to the world production and trade statistics and too little to mechanical harvesting, that there is too much stress on the application of the ion-exchange process and too little on the established improvements brought by continuous diffusion, and that the ten-year-old glutamic acid by-product industry is mentioned too briefly, whereas the undeveloped possibility of pectin manufacture is emphasized. We also note the omission of kraft paper and synthetic resins as products prepared from bagasse.

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NEW OUTLOOK ON SCIENCE

Philosophy of Mathematics and Natural Science. Hermann Weyl. x + 311 pp $5.00. Princeton University Press. Princeton, N. J.

THE greater part of this book comprises a translation, with alterations, of Hermann Weyl's contribution to R. Oldenbourg's *Handbuch der Philosophie* of 1926, the time "when the theory of relativity had reached completion and the new quantum mechanics was just about to rise" (p. vi). Six added essays "are more systematic-scientific and less historico-philosophical in character than the main text." They deal with the most recent developments in mathematics, physics, and biology.

As a presentation of the new outlook on science and the world, Weyl's book may provoke comparison with Eddington's *The Nature of the Physical World* and *New Pathways in Science.* Although some of the discussions refer to the same subject, and many of the conclusions are similar, the basis and the range are very different. Where Eddington causes sparks to fly between highly charged new concepts and earthy conventional convictions, Weyl sets the gems of newly gained insight into the solid grounds of mathematics and philosophy. These grounds are not easily accessible. For the mathematical parts in particular, the reader will have to bear in mind that "one of the principal tasks of this book should be to serve as a critical guide to the literature listed in the references" (p. vi).

In connecting the most advanced thinking in mathematics and physics with that of the past, the author shows that time is as necessarily a dimension in our thinking as in the brave new world of the four dimensions. From Plato to Newton and through Leibnitz and Descartes to Helmholtz and Husserl, our

thoughts have been formulated, and even the much-maligned philosophy of Schelling has concepts which, like his "shapeless fluid," are relevant to what we have just found.

Problems that haunted philosophy for centuries find their solutions in the new science. It teaches to keep "open into infinity . . . the ordered manifold of possibilities which can be generated by a fixed process" (p. 38). The rules of isomorphic mapping clarify the relationship of things in themselves to phenomena. Absolute space is reduced from a problem of transcendental idealism (Kant) to a combinatorial fact. The binary gradation into a priori and a posteriori is replaced first by a ternary one (p. 135) and, finally, by "a rich scale of gradations of stability" (p. 154). We start with theories which are only roughly correct and continue to improve them on the basis of experiments, although "trust in induction, if it is to be justified, can only be justified by the principle of induction itself. But trust in the world and in oneself is in no need of justification; it is the natural attitude of the mind's life, especially as it manifests itself in thetic acts of reason" (p. 163 f.). Our primitive combinatorial schemes may not be adequate, yet it seems best "to make the picture itself, however limited its value, as definite as possible." In doing so, we can follow the advice of Nicolas Cusanus (p. 274). Scientific construction seeks the explanations in an "outer" realm of objects and in immanent laws, not in origins. There is another approach, that from "within;" "what is darkest for theory, man, is the most luminous for the understanding from within; and to the elementary inorganic processes, that are most easily approachable by theory, interpretation finds no access whatsoever." A kind of exclusion principle seems to exist here. "Understanding, for the very reason that it is concrete and full, lacks the freedom of the 'hollow symbol'" (p. 284).

The use of symbols leads to the construction of theory which, in mathematics and physics, is universally compelling. The process of such construction relies on the unity of nature, but it does not overcome the logical paradoxes which arise in old and new forms. Hilbert's new attempt of solving "once for all" the problems of mathematical accuracy had the effect of bringing new paradoxes to light.

It is the obvious permanence of paradoxes which makes philosophical study of mathematics and physics necessary. If, stimulated by Weyl's lucid exposition, we turned this statement around and asked whether philosophical study did not make paradoxes necessary, we could find the answer in this book. In all our paradoxical and ironical (see example on p. 202!) situations, we still have the "trust" which needs no justification, and we have the experience which shows that when we form final answers prematurely, nature compels us to recognize a more profound harmony (p. 159).

In a time of oppressive departmentalization in science, the need for synthesis has come to be strongly felt. Weyl presents a most impressive example of scientific synthesis. It is gained by unrelenting search of facts and meanings in the system which must remain "open to infinity"—open, also, to immediate further effort. That questions arise from the difficult attempt at synthesis is, therefore, within its purpose.

A different set of questions pertains to the translation, particularly because of its general excellence. The German words *erkennen* and *Vorstellung* have no direct and sufficiently poignant literal counterpart in English. *Erkennen* means the thorough, final knowing which "to perceive" expresses perhaps better than "to cognite." *Vorstellung,* in most cases, is less accurately rendered by "presentation" than by "ideal image."

The main question, however, which Weyl's book may cause us to ask is this: Should we not be amazed at those who claim that all this thinking about foundation and synthesis is not practical since it is not practiced in business and politics? How many grievous mistakes are made in these practical decisions, and how many could be avoided by applying more science and philosophy?

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THE ROUND EARTH ON FLAT PAPER

The Story of Maps. Lloyd A. Brown. xix + 397 pp. Illus. $7.50. Little, Brown. Boston.

TO MOST people, maps are fascinating. This is probably related to the very human curiosity of wondering and trying to learn something about the earth we inhabit. The impulse to portray graphically the immediate environment, or the world as a whole, is encountered at all levels of civilization, and at all stages of history.

The Babylonians inscribed crude maps on clay as early as 2300 B.C. Modern map making, in contrast, is a highly technical process involving precision instruments and specialized techniques. The steps in the development and evolution of these tools and methods of cartography, and the personalities and nations engaged in mapping in the world during the past 4,000 years, are interestingly described in *The Story of Maps.*

Determining the size and shape of the earth was a cartographic contribution of the ancient Greeks. They also provided answers to the questions of how much of the world was habitable, and how much of it was actually inhabited.

Claudius Ptolemy, all-time cartographic great, put into practical use many of the ideas advanced by the Greek geographers. Working in Alexandria in the second century A.D., "he demonstrated more clearly than any of his predecessors, and in greater detail, the numerous feasible methods of applying the facts of astronomy to the study of the earth; he systematized the mapping of the earth's surface, and in his writings set forth the principles and techniques

employed by modern geodesy." Ptolemy's atlas set the cartographic standard for almost 1,500 years.

Scientific map making was eclipsed, during the Middle Ages, by the fanciful and illogical scriptural maps. But even the Dark Ages made their contribution to world mapping, for the religious pilgrimages and Crusades stimulated trade and exploration. More navigation and travel called for better charts. Cartographers obliged with gaily decorated, and amazingly accurate, portolan charts.

Three significant developments during the latter half of the fifteenth century stimulated new interest in map making. Printing was invented, Ptolemy's *Geographia* was revived and made generally available in printed form, and Columbus sailed westward to discover a new world. The result was a "golden age" for cartography during the sixteenth and seventeenth centuries, led by such map-making immortals as Martin Waldseemüller, Gerard Mercator, Abraham Ortelius, Jodocus Hondius, Christopher Saxton, and Willem Blaeu.

The eighteenth century was characterized by great advances in scientific methods of surveying and mapping. New instruments made possible more precise determination of latitude and longitude and consequent greater map accuracy. Large-scale topographic surveys on a country-wide basis were carried out. With general acceptance of a universal prime meridian and uniform standard of time around 1890, "mankind was mentally prepared and scientifically equipped to think and act in terms of global cartography."

The Story of Maps might better be called "The History of Map Making." The emphasis definitely is on the past, and upon the techniques and processes of mapping rather than upon the maps themselves. There is but brief mention of the new methods, instruments, and techniques for mapping devised during the past half century and particularly during the recent war years.

The book is profusely illustrated with portraits of the great cartographers, pictures of instruments, and reproductions of old maps. Unfortunately, the illustrations are scattered at random, and only by chance do they appear adjacent to the text to which they are related.

Such a comprehensive history of map making has not previously been attempted. As one explanation for this lack, Brown quotes from Robert Hooke, seventeenth-century physicist, as follows: "There are but few who, though they know much, can yet be persuaded they know anything worth communicating."

Readers of this book will agree that the author knows much about his subject. They will rejoice, moreover, that he was persuaded to communicate to others the results of his extensive research and study, and of his personal experiences with maps.

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BRIEFLY REVIEWED

Syphilis. Its Course and Management. Evan W. Thomas. xix + 317 pp. Illus. $5 50. Macmillan. New York.

DR. EVAN THOMAS has a very active part in several syphilis treatment programs. He is a recognized scientist, as well as an excellent clinician. His book emphasizes treatment, but, like other such treatises, must also take up diagnostic procedures and clinical findings in order to make the course of the disease during and after treatment a complete picture. There is little to dispute throughout the early sections of the book that deal with infectious syphilis or latent syphilis. The types of syphilis that have not yet been studied for long enough periods of time or in great enough numbers to draw any sweeping conclusions after penicillin therapy, such as cardiovascular syphilis, neurosyphilis, interstitial keratitis, and optic atrophy, are handled in a manner that should satisfy the skeptic as well as the enthusiast. There is no one better qualified than Dr. Thomas to write a text on syphilis, its course, and management, and any reader interested in the modern treatment of this disease will find his book the most up-to-date one of its day.

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Social Psychology of Modern Life. (Rev. ed.) Steuart Henderson Britt. xvi + 703 pp. Illus. Rinehart. New York.

IN THIS volume, a particularly well-informed and versatile psychologist gives students and laymen a good idea of the breadth of the field of social psychology. A revision of a 1941 publication, the new edition incorporates considerable recent research and experimental data. Following some general introductory material, the author treats many diverse aspects of the impulses, ideas, attitudes, and behavior of people in group situations ranging from the intimate home setting to the crowd, and to the more structured, larger group. Included among the topics discussed are group reactions, dominance, and leadership; the impact of institutions like the family and religion; social conflict in delinquency, discrimination against minority groups, nationalism, and war. Britt concludes his presentation with a timely criticism of ivory-tower scientists who are content merely to observe without assuming responsibilities of leadership or even an active role in correcting flaws, weaknesses, and inequalities in the social structure. A detailed list of citations, texts, and related sources is provided in three appendices.

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A FALLACY UNDERLYING GARRETT'S USE OF THE DATA OF THE ARMY ALPHA AND BETA TESTS—A COMMENT

The Army tests were instruments designed by a committee selected from the ranking psychologists of the period in order to classify men efficiently for Army purposes in World War I. Utilizing the techniques then available, this committee of psychologists constructed a series of tests intended to measure the native intelligence of individuals Close to 1,800,000 soldiers were tested and classified. The results of this vast testing program, quite useful to the Army, were immediately seized upon by scientists, educators, and journalists in order to strengthen or advance some particular scientific, educational, or political aim. Was heredity more important than environment? Did individuals drop out of school because of defect of native intelligence or because of some social and economic disadvantage? Was democracy a failure? Were the new immigrants worthy of American citizenship? Were there any significant differences among individuals belonging to different nationalities and physical types? These were some of the questions underlying many discussions in the turbulent postwar period, discussions which involved the interpretation of the results of the Army testing program. Today these questions are rarely raised—at least not in the black-and-white fashion that formerly characterized their phrasing—nor are the Army test results generally drawn upon in psychological discussions. There are some important exceptions, however. Garrett, in a SCIENTIFIC MONTHLY article, uses the Army test results as partial evidence for the intellectual inferiority of the average Negro.[1] Benedict and Weltfish, on the other hand, in their pamphlet *Races of Mankind*, use the Army test results as evidence for Negro equality.[2] The question is: Is the intelligence of the average Negro equal to that of the average white person?

The purpose of this brief comment is to call attention to certain features of the Army tests that are relevant to the discussions of comparative intelligence.

According to an assumption of test methodology, a test should be constructed in such a manner as to yield a series of scores which is continuous (assuming that the underlying variable itself is continuous, which holds for intelligence).* An obvious implication is that the least intelligent individual in the series should obtain some definite score (other than zero), and the most intelligent individual in the series should obtain some score less than the maximum possible score. A good intelligence test—"good" in the sense of conforming to the notion of continuity of intelligence—should produce a series of scores with no zero and no perfect scores. A large proportion of zero scores, or of perfect scores, would serve to indicate that the test is unsatisfactory as a measure of intelligence at either one of the extremes. The same line of reasoning also applies to the subtests of a test

* A distinction should be made between the scientific and practical aspects of the Army tests. A test which is not in accord with this principle may still be very useful as a practical instrument. As a scientific instrument, however, it requires congruency of this principle with obtained data. In this discussion, we are concerned with the Army tests as scientific instruments which measure intelligence.

TABLE I

PERCENTAGE OF ZERO SCORES ON SUBTESTS OF ARMY ALPHA AND ARMY BETA. COMPILED FROM DATA ON DISTRIBUTION OF SCORES FROM TABLES 436–39, PAGES 874–75, REFERENCE 3

Group	Percentage of Zero Scores on Subtests							
	Test Alpha							
	1	2	3	4	5	6	7	8
White Draft (Group I) N = 33,100	5	2	10	25	20	12	15	8
Negro Draft (Group IV) N = 6,560	20	10	33	55	40	35	50	30
	Test Beta							
	1	2	3	4	5	6	7	
White Draft (Group I) N = 7,500	7	8	11	12	15	3	18	
Negro Draft (Group IV) N = 12,230	30	20	35	45	45	15	50	

With reference to the argument of the preceding paragraph, what is the situation insofar as the Army tests are concerned? Two things should be noticed in Table 1: (1) the large proportion of zero scores for both groups; (2) the fact that the proportion of zero scores is much larger for the Negro group than for the white group. Point (1) indicates that the tests are not operative as measures of intellectual ability, at least in the lower ranges. Point (2) means that the Negro group, by virtue of factors possibly extrinsic to intelligence as measured by these tests, would be more adversely affected by the inclusion of zero scores than the white group. Furthermore, it should be realized that the various percentages refer to the Negro draft group as a whole. The percentage of zero scores for the Southern Negro draft group alone must have been in excess of 60 on some of the subtests.

With regard to the total Alpha score, there are similar discrepancies between Negro and white groups inso-

far as the zero scores are concerned† Seventeen percent of the Negro draft (Group IV), N = 6,327, attained zero scores (Table 254, p. 724).[3] Furthermore, there was much variation between states for this group Thus, 34 percent of the Negro draft from Florida, N = 499, attained zero scores. On the other hand, the corresponding figure for New York was less than 2 percent, N = 197 (Table 254, p. 724).[3] The data for the white draft are not broken down in a manner to give estimates comparable to those of the Negro draft. There are indications, however, that the percentage of total zero scores for the white draft (Group I) was in the neighborhood of 2 percent (Pt. III, Chaps. 3, 4).[3]‡ This situation could have been predicted, since the percentage of the Negro draft getting zero scores exceeded the corresponding figures for the white draft on every subtest. Without going into the details, similar intergroup comparisons can probably be made with the results on the Beta test.

The practical conclusion of this discussion is that the Army test results should no longer be used in attempts to compare the intellectual status of the Negro with that of the white. Furthermore, the evidence based on these data, as typically presented in textbooks, should be properly revised so as not to give an incorrect impression of the validity of the primary data. These suggestions should not be interpreted as a denial of the value of these tests. In a history of scientific psychology, these tests, combined with the scientific and popular discussions engendered by them, and the stimulus they provided to the mental-test movement, will probably retain a significant position. The excesses that were associated with the interpretations of the test results probably derive from the peculiar postwar climate into which the tests were projected, rather than from their intrinsic merits or defects. It may be cogently argued, especially by those interested in the sociology of knowledge, that it was the presence of defects in the Army data themselves that paved the way for subsequent misinterpretations. The discussion of this matter really belongs elsewhere. Suffice it to add that the volume of the *Memoirs of the National Academy of Science* offers no decisive evidence as to the causes of subsequent misinterpretations.

† For this part of the discussion a zero score will refer to a score in the range 0–4, a range which, practically speaking, is equivalent to zero when the error of measurement is kept in mind The data pertaining to total scores are grouped in intervals, the smallest of which is the 0–4 interval.

‡ The discrepancy in the proportion of zero scores may account for some facts implicit in the Army data which have not been elucidated to date. With a mental age of less than ten years as a criterion of feeblemindedness (in that period a widely used criterion was a twelve-year mental age), 77.7 percent of the Negro draft from South Carolina (N = 1642) should have been classified as feebleminded (Table 247, p. 721)[3] Parenthetically, the interval of 9–14 on the alpha test was assigned a letter rating of *D* minus, and the mental age equivalent of this interval and letter rating was 9.4 years or less. The corresponding figure for the white draft (N = 581) from the same state is about 8 percent (Table 205, p. 690).[3] This difference may have a sufficient explanation in its association with a like difference in the proportion of zero scores. Furthermore, the absurdly high figure of 77.7 percent in itself would be interpreted by pyschologists today, and probably thirty years ago as well, as meaning that the Army tests were defective as measures of intelligence.

REFERENCES

1. Garrett, H E. Negro-White Differences in Mental Ability in the United States, *Sci. Mon.*, 1947, **65**, 329–33.
2. Benedict, R., and Weltfish, G. *The Races of Mankind. Public Affairs Pamphlet No. 85.* New York: Public Affairs Committee.
3. Yerkes, R. M., Ed. *Psychological Examining in the United States Army.* Washington: Nat. Acad. Sci., Memoirs, Vol. XV, 1921.

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MORE ABOUT STATISTICS

I have just read with great interest Professor McConnell's article "ESP—Fact or Fancy" (August 1949). In the first main section of this article he describes three sets of trials with the standard Rhine deck The descriptions of these three experiments close with the following statements.

"The probability of such a score by chance alone is less than 10^{-20}."

"There is no need to apply statistics to such a score to recognize its extra-chance character."

"The probability that this might occur by chance is about 0.000,000,000,064."

Absolutely independently of whether the experiments in question are or are not significant, I would wish to urge most warmly that these statements are irrelevant and misleading.

The mere fact that an event which has occurred has a very small a priori probability does not at all justify one in concluding that something other than chance has operated. Has Professor McConnell, by any chance (negligibly small, of course), read the article "Probability, Rarity, Interest, and Surprise" in the December 1948 issue of The Scientific Monthly?

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THE SCIENTIFIC MONTHLY

NOVEMBER 1949

SCRUB TYPHUS, OR TSUTSUGAMUSHI DISEASE

CORNELIUS B PHILIP

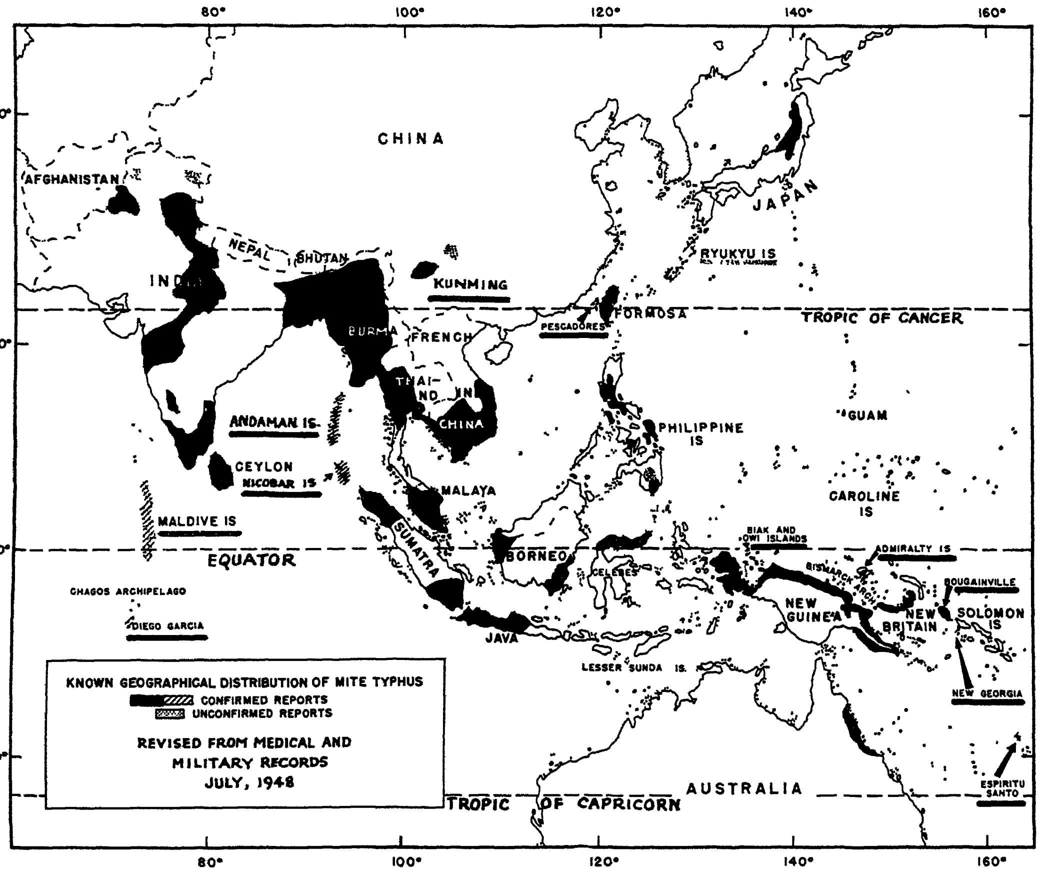
Dr. Philip (Ph.D., 1930) is principal medical entomologist with the USPHS Rocky Mountain Laboratory, Hamilton, Montana, where he has been stationed since 1930. A specialist in rickettsial diseases, he spent three months in Malaya in 1948 with an Army team testing the efficacy of the new antibiotic chloromycetin on scrub typhus. During the war, Dr. Philip was attached to the U.S.A. Typhus Commission as a colonel in the Sanitary Corps of the Army.

A SURPRISING number of important diseases of man and his domestic animals, which have eventually proved to be conveyed by insects and their allies, had long been reputed by the inhabitants of affected localities to be actually so caused. Yet, more often than not, these beliefs and "superstitions" were confirmed slowly and reluctantly by medical and scientific observers.

Such has been the story of flood-river fever, or tsutsugamushi disease, in Japan. Smith and Kilbourne's epic report (1893) demonstrating the first arthropod transmission of pathogenic organisms (the agents of tick-borne cattle fever) appeared in the same year that Kitasato gave the first serious credence to the early beliefs of Japanese farmers that "flood fever" was actually caused by the bites of another acarid, called by various colloquial names, including *akamushi* ("red bugs," or "mites"); this became the eventual species name for the vector in Japan. Certain of these minute creatures were also called *tsutsugamushi* (from early Chinese, meaning "poisonous or dangerous creature"). Thus the classical name of the malady was derived from the vector itself even before its transmitting role was proved.

Kawamura cites early Chinese medical literature as suggesting occurrence in ancient times of a similar malady along rivers in China, but Japanese accounts supply the first accurate descriptions of the disease. Recognizable medical accounts appeared early in the nineteenth century in Niigata and Yamagata prefectures. The first Western accounts are given in a letter of a medical missionary, Dr. Theobald A. Palm, published in the Edinburgh *Medical Journal* in 1878, and in a German description the following year by Baelz and Kawakami in Virchow's *Archiv*. Both mention the beliefs of local inhabitants of causation by *shimamushi* ("island bugs"), associated with river fever.

Tanaka, a physician in northern Japan, was the first serious student of the mite vectors. In 1899, he published descriptions in German, and inaccurate figures of the accused mites, which he soon followed with reports that there was more than one kind on the local voles, including the same kind of "thin-haired red bugs" that were found on man. By the time of his final report in 1930, some six different kinds of these mites had been described on Japanese voles, or meadow mice, which required (and still do) investigation as to their relationship and relative importance in the epidemiology of the disease in the endemic areas along the rivers of northwest Honshu. It is generally conceded, however, that the original *akamushi* is the important vector to man in Japan.



Nowhere else in the countries of the Pacific-Asiatic region where the disease was subsequently discovered—Formosa, the Philippines, India, Burma, Indo-China, Thailand, Malaya, Indonesia, New Guinea and adjacent islands, and Australia—had the epidemiology involved the peculiar restriction to immediate river margins that it had previous to this year* in the classic areas in Honshu.

As the picture continues to unfold in these other regions, it becomes increasingly evident that the epidemiology of this disease is pre-eminently a reflection of the local ecology of the major mite hosts—*Microtus* and possibly *Apodemus* in Japan and various species of *Rattus* in the other areas, with birds probably playing a part in the introduction of infected mites into new localities.

Although similar infections were known under various colloquial names outside Japan, it was not until the past decade that their identity with the classical tsutsugamushi disease was confirmed following pioneer studies on "scrub typhus" in Malaya and "pseudotyphus" in Sumatra. The study of strains and cross-immunity in laboratory animals, and the Weil-Felix test, provided the chief laboratory evidence that settled the confusion caused by observed clinical variations in different areas.

As early as 1908, Ashburn and Craig had reported two cases on Samar, Philippine Islands, which showed similarities to both tsutsugamushi disease of Japan and Rocky Mountain spotted fever of Montana, but it is remarkable that incontrovertible evidence of the presence of scrub typhus in the Philippines was not forthcoming until American reoccupation in 1944–45 revealed foci on six of the major islands.

The causative agent. Scrub typhus is caused by a typical rickettsial agent (minute, bacterialike organisms), which invades the cytoplasm but not the nucleus of infected host cells. It is thus more closely related to the agents of endemic and epidemic typhus than to those of the spotted fever

* Only this past year has an endemic area been proved outside the classic ones, this on the slopes of Mount Fuji (*Historic Report, 1948,* 406th General Medical Laboratory, Tokyo).

group. It does not pass ordinary filters as does the agent of Q fever and has not been grown on artificial media. Rich growths have been obtained in the yolk sacs of embryonated chicken eggs and in the lungs of infected laboratory rats and mice, but vaccines prepared from these sources have been disappointing in meager field trials.

Beginning with a supposed gregarine, a number of "organisms" were accused as etiologic agents of scrub typhus by early Japanese investigators. The first unequivocal reference to its rickettsial relationship was discussed and clearly figured in 1924 by Nagayo and his colleagues, but they and others later admitted that Hayashi in 1920 probably saw rickettsiae in some of his preparations, from which he described *Theileria tsutsugamushi,* with a purported malarialike cycle in the blood. Hayashi's species is therefore transferred to the genus *Rickettsia* in the revised Bergey system, though it is difficult to accept the editors' belief in Ogata as the original reviser, since the latter still claims (correspondence 1949) independent discovery of the organism which he named *Rickettsia tsutsugamushi* in 1931. This and several other specific names are synonyms of *Rickettsia tsutsugamushi* (Hayashi), including the latest proposal of Hayasaka of "var. *tropica*" in a postwar printed military report of the disease in Japanese forces in Burma, Siam, Malaya, and Indonesia.

Photomicrograph of *Rickettsia tsutsugamushi* in infected cell of Giemsa-stained smear of infected animal tissue. (×1500.)

This rickettsia is more difficult to stain than most species, unless first fixed with a defatting agent—methyl or absolute alcohol. It is readily demonstrated in the cytoplasm (but never in the nucleus) of cells in Giemsa-stained smears from the body cavities of infected mice or from yolk sacs of infected chicken eggs.

Fortunately for diagnostic purposes, a fortuitous discovery in Malaya in 1926 revealed that serum of persons convalescent from scrub typhus agglutinated an accidental mutant of *Proteus* OX_{19} which was labeled *Proteus* OXK (after Kingsbury, who provided the strain that presumably mutated). This was a variation of the original so-called Weil-Felix reaction for certain other rickettsial infections. Although nonspecific as a serologic test, the OXK reaction is now considered to be diagnostic in endemic areas when high titer is obtained, or a rising titer occurs in serially drawn serum samples from a patient. In certain areas, such as the Kunming district of southern China, epidemiological considerations differentiate louse-borne relapsing fever, which may also elicit a rise in OXK agglutinins.

The mite vectors. Chigger mites of the family Trombiculidae occur all over the world and are peculiar among mites (which are really minute ticks) in that only the larval stage is parasitic, and then always on some vertebrate. The unfed larvae are so minute, on emergence from the egg, as to be scarcely visible to the untrained eye; they have been observed to penetrate the mesh of a coarsely woven sock. After completing engorgement on a host, the larvae drop off and continue development as free-living nymphs and adults in the soil.

Only recently have techniques been developed for mass rearing of any species. Crucial disease-transmission studies in the laboratory have thus been hampered but are now being undertaken. Preliminary tests with Dr. Dale Jenkins, of the Army Chemical Center, have not succeeded in demonstrating adaptation of the pathogen to transmission by North American chiggers, although actual ingestion of the organisms was accomplished.

Only two kinds of trombiculid mites have so far been incriminated in transmission of scrub typhus to man. Named *Trombicula akamushi* and *T. deliensis,* they are so closely related systematically, with morphological intergradation, that some tax-

The "dorsal topography" of *Trombicula deliensis*, one of the two known vectors of scrub typhus. The other, *T. akamushi*, differs microscopically only in having more spines posteriorly. (This photomicrograph was awarded honorable mention in the First Annual International Photography-in-Science Salon. Greatly magnified.)

onomists have considered them varieties of the same species complex. It is to be hoped that the new breeding techniques will provide evidence to clear up this moot question.

The first was shown by the Japanese to be naturally infected, and to occur on voles coincidental with human cases in the warm months in the endemic areas. Since the parasitic larvae customarily attach only to one host in a given generation, it was considered axiomatic that the infection passed through the subsequent nonparasitic nymphal and adult stages and via the eggs to the next generation, but the data substantiating this are very meager for either of the above mites. Furthermore, reports are conflicting regarding demonstration of infection in wild-caught adults and nymphs. Although infection was readily recovered from larvae off rats in recent Malayan studies, no infection could be demonstrated in considerable numbers of nymphs and adults from the soil of foci where the rats were trapped.

T. deliensis was named for Deli in Sumatra by a Dutch investigator who first discovered it there. During the recent hostilities, great impetus was given to determining the extent of chigger mite occurrence, and surveys have revealed that both this and *akamushi* are widely distributed in all the major endemic countries, though one or the other may predominate in a given locality—sometimes to the exclusion of the other, as on certain of the smaller islands. A few specimens indistinguishable from *T. deliensis* have only recently turned up in collections from Japan, where it had been thought that only *T. akamushi* was the vector to man.

At least seven other species of trombiculid mites have been found on Japanese voles; two of them occasionally attack man and have also been reported to have caused infection when injected into laboratory animals. Because of their minute size, even when engorged, it is, however, hardly possible to be certain that no *akamushi* were present in the injected samples. A relative of these mites, new to science, has just been discovered on voles in the new Mount Fuji focus in Japan.

Circumstantial observations cast suspicion on certain other species during the early studies in Sumatra, and in both the New Guinea area and northern Queensland during the war, where species (other than the above) that will bite man were the only ones found incidental to local human infection. These included certain species of mites that are capable of causing "scrub itch," an irritation of the skin appearing promptly after the bites, which is entirely unrelated to scrub typhus. This was encountered by troops in several Pacific areas, but appeared strangely absent from Burma and the Philippines.

The larvae of both vectors contain pinkish pigment even before feeding, which is the basis for the widespread illusion that attached mites are ingesting blood. Like other trombiculids, however, they feed only on lymph and tissue fluids, through a peculiar tube which penetrates the subdermal tissues, from the bladelike, embedded mandibles. Continuing its extension during feeding, this tube may become longer than the body of the mite itself, but its origin has never been satisfactorily explained.

About midway in the incubation period, or during the first mild symptoms, the site of an infected mite bite often ulcerates to form a so-called eschar. The percentage of patients showing this sign varies in different areas, for unknown reasons, but not in relation necessarily to the virulence of local strains of infection. Differenti-

ation of scrub typhus from tsutsugamushi disease on this basis is no longer accepted.

The vertebrate "reservoirs." Following the demonstration of natural infection in tissues of mite-infested voles in Japan, attention was focused on native murid species in other endemic areas. The indigenous rat fauna of the Pacific-Asiatic region is very rich in species. Recovery of strains from various local species has been reported in Formosa, Burma, India, Malaya, Java, New Guinea, and Queensland. Five species or varieties of rats were represented. Other species are undoubtedly susceptible also, and many have habits that make them good mite hosts.

The domestic, or commensal, forms of *Rattus* are apparently involved only occasionally. They are seldom found infested with vector mites except under special environmental conditions, as in the suburbs of Calcutta or the farmyards of the Pescadores Islands west of Formosa. Under special conditions, other local faunal factors may also intervene, as in parts of Burma and India, where tree shrews have been found infected in nature. The susceptibility of the marsupial bandicoots is still unconfirmed, but these animals have been accused and could be an important factor in the antipodes.

Raised, blisterlike reaction about a chigger attached for thirty-six hours at the base of a hair on a human forearm.

In view of Hayashi's report of the susceptibility and natural infection in certain native Japanese birds, it is surprising that known foci had remained confined to river margins in northwest Honshu until the Mount Fuji focus was encountered. Bearing on this consideration are data of the writer showing persistence of the agent in the blood stream for varying periods in sparrows and domestic fowl, the longest (twenty-six days) in turkeys. A captive Malayan jungle cock, which the writer had under observation in Kuala Lumpur in May 1948, had demonstrable infection in its brain nineteen days after injection with a laboratory strain.

Since ground-frequenting birds, in particular, become heavily infested with vector mites in endemic areas, the implication of birds as carriers is obvious for spread from a given focus, or to isolated South Sea islands such as Bat, Ponam, and Espiritu Santo, on which foci were encountered. During surveys in a 398-acre cutover rubber field in Malaya where infection was not known until after the war, quail were found to be much more heavily mite-infested than were indigenous rats, and infection was recovered from mites off two of these quail. These birds could, therefore, cause extension of the focus in this and neighboring fields more rapidly than local rats, which are much more restricted in their migrations.

For laboratory tests, white mice are the animals of choice, though North African gerbils and field mice of the genus *Microtus* are also readily and fatally infectible. Under the impetus furnished by combat problems with this disease, many strains of the rickettsia have been isolated from human, rat, and mite sources in most of the known endemic countries. It is apparent that there is a wide zoological adaptability of the agent. Nevertheless, it is generally believed at present that the rat-mite-rat cycle, fortified by transovarial continuity between mite generations, constitutes the fundamental mechanism for natural maintenance of infection in most tropical foci.

Unquestionably, the mites act as important reservoirs, but the demonstrated fact of rickettsiae persisting in tissues of experimentally infected rats for many months requires further investigation of murids in the reservoir role also. Dutch investi-

gators have recently reported recovering infection from experimental guinea pigs nearly two years after inoculation.

The epidemiological picture as brought out during hostilities. As has been implied, the ecology of the disease in the tropics was found to differ from the flood-river fever in Japan in that it involved environments harboring a variety of native rats rather than field mice. One of the tragedies of military operations is that they have always provided mass exposure of susceptibles to various endemic diseases in combat zones Knowledge of both the geographic and the ecologic distribution of scrub typhus was greatly augmented as a result of war activities and the occurrence of cases in troops in out-of-the-way places.

A type of coarse grass (*Imperator cylindrica*) known as *lalang* in Malaya was particularly suspected as dangerous before the war. Troops again encountered the disease in fields of this grass in New Guinea (where it was known as *kunai*), in the Philippines (*kogan* grass), and in parts of Burma and India. This grass is also found along the endemic river margins in Japan, where it is called *susuki* and *yoshi*. Local epidemics resulted in the troops and in postwar labor in other grass associations, particularly a forage grass (*Pasapalum conjugatum*) in Burma, Siam, and Malaya; the seed of this grass are probably attractive to the rats as food. The grasses in some of these fields were virgin stands; in others they comprised secondary invaders in abandoned rice, cane, and tapioca fields.

Coconut groves, neglected during the Japanese occupation, developed an undergrowth providing good rat harborage along the coasts of some of the Philippine Islands, New Guinea, and adjacent small islands, which also produced several major military outbreaks. These doubtless also will be the source of civilian cases in the postwar reclamation period. In Malaya, important foci of this type shifted from prewar oil-palm estates to cutover rubber and other previous war-emergency food-growing areas.

Although it was suspected in several areas that cases were contracted by soldiers in primary rain forest, there was usually a history of previous contact with more open terrain during their incubation period. A large proportion of cases occurs near sea level or in low inland country, but foci are known in mountainous terrain in New Guinea, Formosa, and India, where infection has been found as high as six to seven thousand feet in Kashmir and the Kumaon Hills There may be interesting ecological significance in the preponderance of the foci north of the equator. These foci extend well north of the Tropic of Cancer but do not even reach the Tropic of Capricorn to the south. The factors affecting the distribution of the vectors and their hosts are obviously concerned.

It thus has become increasingly evident that wherever the local environment encourages the rat-vector-rat natural association, there is potentiality of human infection. Such conditions have been shown to exist even in primary jungle in New Guinea and Burma, so that it is probable that "silent" foci exist in many areas as yet unvisited by the white man.

The most marked seasonal variation in incidence is in Japan and northern Formosa. In some parts of western Burma and India, the annual dry season adversely affecting the local mite populations is reflected in a drop in case occurrence also. Experience of the Australian and American forces, on the other hand, indicated that fluctuating exposure and troop movements rather than seasonal factors accounted for variations in incidence in Queensland, Indonesia, the Philippines, and northern Burma.

The disease. Scrub typhus is a severe, prostrating, typhuslike infection, often with an eschar at the site of the mite bite. It is typically characterized by swelling of the lymph glands, a skin rash, headache, loss of appetite, muscular aches and pains, and temporary damage to certain tissues and organs, particularly the circulatory system, including the heart. Permanent damage to the heart has been claimed as a result of symptoms observed during late convalescence, but this has since been discounted.

The incubation period was sometimes determined rather exactly when timed by invasion schedules. The average is around ten to twelve days, but instances as short as four to six days have been observed.

Fatality rates vary in different areas. In Japan, as high as 60 percent has been reported, whereas only 0.06 percent mortality occurred in the Owi-Biak epidemic of 1,469 American military casualties. The usual fatality rate in the tropics has been between 10 and 20 percent. Duration is prolonged, with a conservatively estimated average loss of sixty to seventy man-days per case in military personnel. Some convalescents are rehospitalized with weakness and heart and nervous symptoms; in such cases, convalescence is slow. Two or even three relapses during convalescence were recently

Chigger mites are minute. *Left*: A fully-fed specimen on the head of a pin. *Right*: The adult mite is bright red. It does not attack animals, but lives free in the soil. (Photoflash caused recoil.)

noted in Malaya. On the other hand, during epidemics, mild febrile episodes have been observed which were diagnosed as scrub typhus only by serological means two or three weeks after return to full duty, in some instances without requiring hospitalization.

The fact that second and even third attacks after varying periods have been seen suggests that immunity is not as solid as in some other typhuslike diseases. Recent discussion has even considered evidence that immunity is dependent on "premunition," or the persistence in the tissues of the host of asymptomatic infection such as envisaged for certain viruses.

The antibiotic chloromycetin, recently shown by Smadel, Woodward, and their associates of a U. S. Army team to be dramatically effective in treatment of this disease in field trials in Malaya, has proved to be the most effective of the drugs tried to date, and tests have indicated it is also of value in temporary suppression, or chemoprophylaxis against infection during and following exposure.

The drug can be administered by mouth, has been found not to be toxic for clinical use, and to result in defervescence in twenty-four to thirty-six hours after an administration period as short as twenty-four hours, as well as to shorten markedly the period of hospitalization. Laboratory tests with experimental mice also suggest that another new antibiotic, aureomycin, may be equally effective in treatment.

Control as practiced before and during the war. In the absence of a proved vaccine or, until very recently, of an effective drug against the agent itself, control measures, past and present, may be divided into two classes, both directed against the mite vector: temporary efforts to protect exposed persons from mite bite, and more permanent manipulation of the environment to reduce mite prevalence.

Until the advent of effective acaricidal impregnants, miteproof clothing was ineffective in Japan and, in addition, impractical for the tropics. Impregnation of uniforms with dimethyl phthalate by American forces, and with dibutyl phthalate by the Australian forces, both in the Southwest Pacific, was proved feasible, but not widely adopted in this or the Burma theater, owing chiefly to the lateness of use in the campaigns.

The Australian hand-treated uniforms were re-

ported to have reduced incidence and to have resisted several launderings, remaining lethal to invading mites. Other improved acaricides were soon developed. Much the most effective of these resistant chemicals is a now well-known insecticide, benzene hexachloride (BHC), but it has proved to have dangerous toxic properties to the wearer. The newest of the clothing impregnants on the U. S. Army Quartermaster supply shelf consists of 45 parts of benzyl benzoate (a well-known scabicide), 45 parts of dibutyl phthalate, and 10 parts of an emulsifier. The effectiveness of this acaricide as a clothing impregnant has been demonstrated incidental to field studies in proved hyperendemic foci in Malaya, where discontinuance of wearing of treated clothing during exposure resulted in infection among volunteers.

Barefooted Tamil laborers "ring-weeding" young rubber stumpage in a focus of scrub typhus. Postwar Malayan rubber estate near Kuala Lumpur.

The development of agriculture in the tropics, however, can hardly depend in practice on this means of protection for relatively primitive labor, owing to minimum of dress and the habit of going barefoot in the fields. To insist on the wearing of footgear and treated socks by Tamil labor on rubber plantations in Malaya, for example, would probably result in detrimental psychological reactions outweighing the advantages of protection afforded for purposes of production. A further difficulty recently demonstrated in practice in Malaya also involves the human equation—native wearers of treated clothing may become lulled into a sense of false security and become lax in their care to maintain proper miticidal barriers even under supposed close supervision. Each of two native field assistants, presumably indoctrinated, became infected after over two months of field work under equivalent conditions with two Europeans, in constant company with them in the same foci, who did not become infected. Furthermore, one of the latter became infected in the same areas when treated clothing was later intentionally omitted.

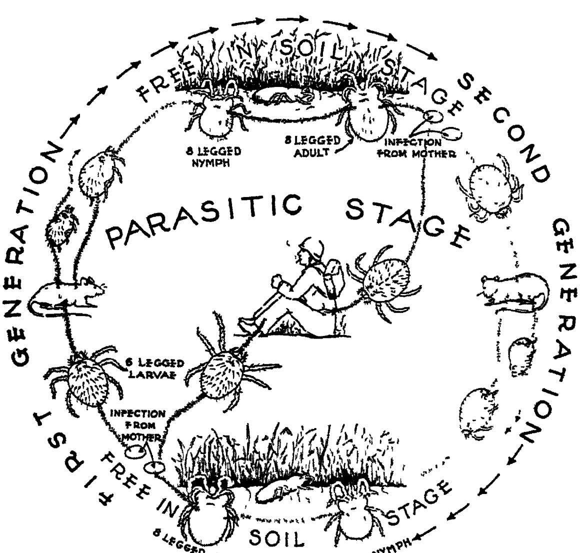
The other alternative, of reduction of indigenous mite population in a given local environment, is practical where heavy machinery is available, such as on a military beachhead or about the immediate environs of human habitation on plantations in endemic areas. The vector species have been found to be very sensitive to exposure of soil terrain to drying and to sunlight. Mites on trapped local rat hosts have been found to practically disappear where ground litter and other protective cover has been cleared. But again this is economically impractical where extensive acreage must be cleared by hand labor. Burning of mature grasses is feasible too late in the season in Japan, usually dangerous, and only partially effective. Under tropical conditions, rapid regrowth occurs, affording repopulation by immigrant rats. Rat control could aid, but it is hardly practical on the extensive scale that would be required for plantation areas.

Anglo-American and Asiatic volunteers exposed in a focus in Malaya during field trials of chloromycetin as a prophylaxis.

It also has been shown by the Orlando investigators of the Bureau of Entomology and Plant Quarantine that among other acaricides, crude benzene hexachloride is a superior and durable soil disinfectant against chiggers when applied at the rate of 50 and 25 pounds of toxicant per acre. Sulphur and DDT were much less effective. This again would be an economically feasible control measure only under very restricted circumstances.

Future lines of research. Some fundamental questions relating to the epidemiology and control of this disease remain to be answered. Are there strain differences in the etiologic agent of the order of the differences between epidemic and murine typhus as suggested by recent serological evidence? The answer to this question could bear on the feasibility of a practical vaccine. How satisfactorily can military and agricultural needs be met by the new drug chloromycetin when it is brought into adequate production, from points of view of both treatment and suppression? How effective, comparatively, in the field is another antibiotic, aureomycin? Even granting their effectiveness, will these drugs be economically desirable for use in large areas of the Pacific tropics where production is dependent on cheap, usually barefoot, native labor?

Much remains to be learned regarding the vector question. What, if any, species of mites other than *akamushi* and *deliensis* are capable of harborage and transmission of the rickettsial agents, and, if they exist, what is their importance epidemiologically? Present reports of other vector species need confirmation. Is there a qualitative or quantitative change in the passage of the disease agents through the nonparasitic nymphal and adult stages? What is the percentage of progeny that acquire infection from an infected female via the egg? Is this a sufficient explanation of maintenance of foci, or must there be a proportion of new lines of infection started from infected vertebrate hosts? Since rats are known to carry persisting rickettsiae for a year or more, the answer to the above questions and to whether rats can act as effectual asymptomatic carriers to infect successively attaching mites will have a bearing on the emphasis to be placed on local control measures. The adaptation of newer rearing techniques to laboratory studies of the vector mites appears imminent and will open up many profitable lines of research in spite of the hazards involved. The reduction of these hazards inherent in the experimental handling of these minute creatures is an important step.



The "rickettsial stream" of *R. tsutsugamushi* in nature through two generations of mites. (Courtesy U. S. Army Medical Museum.)

Problems of the vertebrate "reservoirs" also require much additional observation, particularly the role played by birds. The ecology of the vertebrate hosts probably has a vital relationship to the distribution of the vectors, and hence to explanation of the peculiar, and still enigmatic, spotty focal restriction of the disease in a given endemic area. Some differences in ecological factors are probably involved in explaining occurrence of foci in the mountains of Formosa, the Philippines, and India, and on isolated Pacific islands, while the disease has apparently failed to spread beyond the immediate river margins in northwest Honshu in Japan except for the focus near Mount Fuji. There are also important lacunae in our knowledge of the susceptibility of important mite hosts in various localities and of the epidemiological significance of such unusual hosts as tree shrews found naturally infected in parts of Burma and India. Almost nothing is known regarding the environmental and food requirements of the free-living nymphs and adults of the vector species.

These are some of the problems that serve to emphasize the need for much more work in elucidating the epidemiology of scrub typhus. Answers to at least some of them may be forthcoming from small teams of British and American investigators now in the field in Japan and Malaya.

Photomicrographs by N. J. Kramis.

ETHICAL SCIENCE

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BEING untrained in ethical theory, I think it very likely that all I have to say has already been said in more adequate and more pleasing ways. If this calls for apologies, I gladly offer them. What matters today is not whether ideas on ethics are original, but whether they work in our world. Democracy, as we understand it, has tended chiefly to *dissolve* old standards, and has shown itself thus far none too competent to create new basic norms. Moreover, it has set itself against accepting all authoritarian standards, because they violate our concept of freedom And it is becoming fairly evident that the idea of freedom alone, no matter how prolific of special "freedoms" it may become, is never going to settle in a positive way how we shall decide the most basic and the most intimate issues of our lives. Under these circumstances, "ethical science" has a strong appeal.

The purpose of this note is to illuminate the impressive parallelism that exists between the traditional problems of ethics and those of science. In many cases, the analogue of the ethical problem has been solved in science, and the question is here asked whether the known solutions may not contain suggestions useful for ethics. But before proceeding to this central theme, I shall restate briefly the position that makes such an inquiry possible.

"Ethical science" differs from "scientific ethics" as that term is often employed: it is not a method of solving ethical problems by reference to the *content* of science. Science, we hold, will never replace ethics; its subject matter is not continuous with moral actions, for the *ought* is foreign to the factual *is* of science. No attempt will, therefore, be made to revive Stoicism with its precept of living according to nature, or any more modern version of naturalism which makes believe that if one squeezes science hard enough it will give norms for action. The time for this illusion is past. Science has its vogue; its successes are impressive enough in its own field. Yet by itself it is powerless to mold the behavior of men So far as action is concerned, science never rises above *hypothetical* function: it will build houses and automobiles and planes, *if* man decides that such devices are worth having; it will feed millions, *if* it is decided that millions ought to be fed; it will destroy life, *if* men desire it to be destroyed. But the desire, the decision, rest on grounds that are inaccessible to scientific routine.

There is only an appearance of truth in every argument which denies this fact. Typical is the reasoning of hedonism, which goes as follows: "It is a general law of nature that all living things seek pleasure or happiness in some form. Therefore, to be in accord with scientific law, it behooves us to promote by our actions all conditions which enhance pleasure or happiness in ourselves or in others." And thus it seems that a normative maxim has been extracted from science. There are two important errors in this piece of reasoning. The first is inherent in the word "therefore" placed at the beginning of the second sentence. If we grant the premise that all living things seek happiness, then, being a living thing, I *must* seek happiness as a matter of fact, and there can be no question about the validity of this ethical doctrine. Yet we know that the conclusion does not follow in this simple syllogistic way, for it requires assent of a sort never forced by logic. I can, after all, promote unhappiness if I wish.

There is, in fact, an unstated premise whose inclusion would make the argument formally correct. It should run: "Most living things seek pleasure. I wish to conform to the character of most living things. Therefore I will seek pleasure." In this form it becomes apparent that the second premise, "I wish to conform," etc., is independent of the first, and can be independently denied. Only the first premise comes from science; over the second, science has no jurisdiction.

Leaving the second premise unstated is but one fault of the hedonistic argument. The other is the falsity of its major premise. To say that pleasure-

seeking is a scientific law is misunderstanding science. For if it is a scientific law no one can violate it, as has already been mentioned. The law of gravitation prevents no one from throwing stones upward and is not violated by this act, nor does one contravene it by walking off a cliff. Indeed if seeking pleasure were a law of nature, it could, because of that very circumstance, never function as a basis for ethics. Careful analysis shows that the statement in question, far from being a law, is but a definition of pleasure, or of happiness. For it amounts to making pleasure the motive of all actions: martyr and sensualist alike are driven by pleasure. This settles the matter—and at the same time it makes pleasure, or happiness, ubiquitous enough to be unimportant to ethical concerns.

The state of affairs described is well illuminated by a corresponding situation in physics. The relation between science and ethics is not unlike that between the first and the second law of thermodynamics. The first permits a very great variety of physical processes, many of which are never observed. No violation of the first law is involved in the prediction that a pail of water should boil when placed on a cake of ice, or that the cloud which rose over Hiroshima should reassemble itself and form the bomb from which it issued. It is the second law, not derivable from, but superimposed on, the first which makes thermodynamics into a reasonable discipline. Now replace the first law by the whole of science; you then need a "second law" to produce a reasonable state of human affairs. That second law is ethics.

But it is one thing to admit that science is impotent to sire ethics, and quite another to claim its irrelevance for ethics. For, after all, science has developed a methodology the success of which ethics may well envy. I propose that ethics "copy" those traits of scientific method that can be conducive to its own progress, not because these traits are scientific, but because they represent the best that human ingenuity has at present to offer. The inveterate humanist who regards such proposals as undignified and insulting has had his day; he will regain his stature only by reflecting seriously upon the risks involved in maintaining his aloofness from science.

To implement our suggestion, it becomes necessary to state briefly what the essential method of science appears to be and how its analogue would function in ethics, and to draw attention to the parallelism which exists between the fundamental problems of moral philosophy and those of the philosophy of science.

I

By science we shall mean *exact,* or deductive, science. This restricts all considerations to disciplines which have a clear mathematical or logical structure; it leaves aside such descriptive correlational sciences as the natural histories, sociology, and, to a large extent, economics. From the point of view here taken, this exclusion is not a neglect or an adverse judgment of their significance; it merely acknowledges that these sciences are probably not yet ripe for ultimate methodological analysis, that they have not yet reached the state of fertility and the power of prediction to which they quite obviously aspire.

It is widely believed that all sciences are correlational and inductive. A theory, it is often said, is merely a generalization of the facts of immediate observation: intelligent guesses from particular to more general situations, sanctioned by maximum probabilities, are the methods whereby science proceeds. I feel that this view cannot be maintained in the face of modern physics, but shall not argue the point in detail. The entities of the astronomic and of the atomic domain lay claim to a status in the scheme of reality which cannot be justified by declaring them to be inductive generalizations of immediate experience; they draw sustenance from metaphysical principles which give them strength beyond the trickle of vigor they derive from the concatenation of probabilities linking them with immediate facts. For the likelihood that an electron exists, or that the universe expands, is found to be practically zero when it is compounded by probability laws from the individual probabilities of the numerous specific propositions that compose these statements. Theory, it seems, is stabilized by two kinds of requirement: one, that the facts of observation be *deducible* from it without fail; and, two, that it obey certain postulated norms (such as continuity, causality, simplicity, etc.). By satisfying the second requirement—which is meaningless for the facts of immediate experience—as well as the first, theory comes to grips with reality in a more significant way than does mere observation.

If this appraisal of the function of theory in science is in error, the relevance of everything this note is meant to offer at once collapses. Ethics is then also a thicket of correlated behaviors which the anthropologist is expected to unravel. The reader who rejects the thesis that physics, for example, is both deductive and confirmative in its procedures, need not go on, for he will see in the subsequent remarks only the grotesque inflation of a basic error into a pretentious ethical theory.

But the sympathetic reader will recognize science as a great postulational enterprise forever adjusting itself to the demands of contingent facts We postulate the laws of arithmetic, construct number fields, and draw from them conclusions which conform to certain parts of our direct experience. The postulates—in this case the laws of numbers—are found so successful in a limited domain as to convey the impression among the uncritical that they have a kind of absolute truth, quite independent of their logical genesis; hence it came as a shock to many minds when the properties of atomic entities refused to obey the laws of arithmetic and required matrices, not numbers, for their description. With the recognition that electrons are not billiard balls rapidly gaining ground, we are prone to smile and think what fools we've been to think that the axioms of arithmetic should be valid for all possible experience! And, continuing to muse in this vein, we become aware of the unfortunate circumstance that even now we have not been able to invent a calculus for handling ideas, which, by their apparently spontaneous generation, their refusal to be additive, their disappearance through forgetting, defy the laws of all normal calculi.

In other fields of science, axioms of other sorts lead to metaphysical and empirical satisfaction. There are Newton's laws of motion, Schroedinger's equation of quantum mechanics, any one of the so-called relativistic cosmological models, Pauli's exclusion principle, the laws of genetics. At present all these are to be regarded as *different* hypotheses regulating diverse parts of our experience. Most of us hope, it is true, for an all-embracive future theory which will unify these various postulates, but we do not wait for it before proceeding with the important business of science. And it is remarkable that so much could be achieved with the use of rather tentative postulates which have no a priori sanction! Hence it should be noted how such success became possible.

Axioms,† that is, norms of thought, fully formulated and clearly understood, were accepted for methodological purposes as true, were consistently adhered to with utmost care in all deductive procedures. Yet the possibility was always left open for modifications of the initial premises when facts required them, and without undue concern over eternal verities the scientist frequently recast his axioms for greater conformity with the contingencies of direct experience. Nor did he see an inconsistency in this procedure.

† By axiom, or postulate, we mean here any basic (unproved) hypothesis which has deductive fertility.

What he regards as inconsistent is failure to honor his tentative commitment to a formulated norm of thought during the process of deduction and empirical verification He calls such inconsistencies errors of reasoning. Their avoidance is not always an easy matter; it often involves a degree of tedium and of determination rivaling the disciplined steadfastness of moral integrity. Also, there are often personal advantages to be gained through scientific error, which becomes a moral lapse when detected but not corrected. Indeed, there are places where the distinction between scientific and moral error becomes rather thin and where there is an overlapping of issues, as in situations governed by a scientific code of ethics. At any rate, scientific sin is not commensurate with *doubt as to the axioms* of a given discipline: it is the *failure to honor a commitment* to a set of maxims, whether they are ultimately tenable or not.

Having clearly formulated its basic principles, science goes forth to the second phase of its methodology and derives from these principles all conclusions which experiment is able to test. Naturally, of course, the formulation of principles is initially carried out with an eye upon their likely survival under test; the psychology of discovery draws heavily upon suggestions coming from unexplained facts. But this does not alter the logic of the procedure here under study. The second stage of scientific performance, deduction of specific theorems and prediction from postulates, is largely dominated by the analyst (the theoretical physicist, the applied mathematician), who prepares the scientific material for the guidance of the confirming and often discovering experimenter.

We have now arrived at the third phase in which the predictions issuing from the fundamental laws are confirmed or confuted. Confutation, even in a single significant instance, calls for rejection of the premises; confirmation, on the other hand, results precisely in the effect which this word describes: it renders the hypothesis firmer, though not indubitably true. The process of testing laws is intricate and beset with great complications; it is not always a matter of simply *seeing* whether a prediction is correct. For instance, to verify a law of quantum mechanics usually requires far more than an observation; it calls for numerous interpretations which are possible only by an appeal to the theory itself. But, here again, I do not wish to go into particulars; it is sufficient to emphasize that science was not born with obvious criteria by which it might demonstrate its truth. Indeed, the significance of tests had to be de-

veloped along with its own deductive formalism.

To summarize: science has three logical (not necessarily historical!) stages: (1) postulation of principles or laws; (2) prediction on the basis of laws; (3) confirmation of the predictions and thus validation of laws. The temporal sequence of scientific investigation may combine these stages in any order. It may start with a flagrant confutation of a prediction from accepted laws, and lead, by an inquiry that travels back and forth between all the stages, to the discovery of new principles. Cases in which discovery actually moves in the order of logical sequence, from 1 to 3, are citable, but rare.

For one who understands science in the manner here set forth, it is exceedingly difficult to see why ethics should not operate in a similar way. The elements of method which constitute a science are present in ethics as well: there is a moral code, or, shall we say, there are moral codes, in perfect analogy with scientific postulates; there is the task of explicating and expounding the code, of drawing from it all its consequences, and this is the exact counterpart of such scientific procedures as solving the laws of motion under special conditions. Finally, there arises in ethics very clearly the problem of confirming the code, of seeing whether it is the best attainable, or whether it is in harmony with the philosophy espoused by its practitioners. Let us follow further the suggestions contained in this parallelism. Let us postpone for once the ready conclusion that the scientist talks nonsense whenever he leaves the subject of his métier. For, although the specific conclusions I shall draw from these considerations may be entirely in error, it is nevertheless possible that a recognition of the tripartite structure of ethics, by revealing more clearly the problems that are to be solved, may prove beneficial to the progress of moral philosophy.

One need not be a partisan to the view that ethics has the structure of a science to see the harm which has been done by the persistent failure to respect the distinctions between the three phases. The first and the third are most frequently confused. Somehow, men do not wish to accept a moral code unless its validity can be demonstrated in an a priori manner to begin with. They act like the pseudo scientist who will never accept anything but the consequences of theories he already knows to be correct. They are like the physicist of the early century who rejected the quantum theory, because it violated what he called common sense. Now it is obvious that *empirical* validation cannot be had beforehand, whatever one's philosophy of the a priori may be. As a consequence, the person who confuses the first with the third methodological stage of ethics is driven to seek accreditation of moral doctrine by divine agencies, or at any rate to look for its documentation as an eternal verity deep in the nature of things. He does not realize that he is asking or looking for something absolutely unique, something which no other discipline requires as a starting point. The axioms of science, for example, can be accepted and are indeed accepted *prior* to their proof, for a variety of reasons. The genius who conceives them may regard them as inspirations; some have traced them to a divine origin. To others they are suggested by what they expect to happen; others again adopt axioms because of their logical simplicity or their mathematical elegance. Whatever the motive, one adopts them with complete respect for their integrity as guides in scientific conduct, even if one wishes ultimately to prove them wrong.

What I advocate is a postulational form of ethics, one that gets going before its code is definitely known to be "true."

Our schematism has the further advantage of calling attention to the need for verification of ethics, which is usually either confused with the other phases or forgotten. A moral system does serve a purpose, and clarity as to the general conditions under which the system could qualify as valid must at some time be attained. At present one finds concern over this problem predominantly in rather vague queries having to do with the effect of particular actions upon an individual's happiness, or upon society. Such issues, it seems, will have to be discussed in a much larger setting and probably on more abstract grounds. Perhaps Kant's categorical imperative, with its detached concept of duty, is the nearest approach to scientific ethics in its attitude toward confirmation at the present time. Furthermore, stage three calls upon us to envisage confirmation of ethical doctrine as a large historical venture, perhaps as a vast social experiment, ranking in scope and duration with astronomical enterprises whose completion extends over many years. Validity does not come the easy way, by soul searching or by miracles.

A word should be said about phase two, which I have badly termed "explication." In our time it seems to be very much the neglected phase (except among legal minds, where, however, it takes on certain aspects that remove it from the ethical realm). But we recall that *casuistry,* the established name for the activity under discussion, denotes what was once a flourishing and widely practiced subject. It has sunk into disrepute, prob-

ably as a result of the spread of relativism in ethical philosophy and of the attendant loss of force of all moral codes. But certainly the point of view here adopted confers upon casuistry an importance quite commensurate with the other phases. Explanation of the meaning of norms, in terms of individual behavior should, I believe, rate higher in public education than it does today.

II

Several powerful arguments stand against our view, which takes seriously the structural parallelism between ethics and science. Most damaging in its public effect is the thesis that the subject matters of science and of ethics are wholly different in kind, and that this difference is destructive to all comparisons between them. Science deals with objective facts, ethics with the subjective behavior of men.

If this is meant to assert that ethics is a far more difficult subject than any of the highly developed sciences, I can find no fault with it, for then the argument merely urges a more emphatic and forthright adoption of the methods which are known to be successful in the simpler fields. If, however, the argument implies that ethics will *never* be understood, then I think it is dangerous. Fortunately, it makes little sense when thus construed. The reason most commonly given for the belief in a fundamental disparity between natural science and the science of man is this: A natural object is unaltered by man's speculations about it. Whatever conclusions he draws from its presence do not effect it in the least. But knowledge is a primary and most important factor in man's behavior. There is what has been termed "a strong interaction" between subject and object in the science of ethics, and the objective description of natural science must fail in consequence.

But this argument loses its point, because it is no longer true that science must limit its scope to "objective" matters. Atomic physics and some parts of biology are domains where scientific procedures, measurements, and so forth have as severe an effect upon the observed system as a moral action has upon any behavioral situation. And yet these sciences have succeeded in developing exact deductive theories. Indeed, what seemed, in the classical stage of science, to set it apart from man's reasoning about his own volitions and actions is rapidly disappearing, and a new appraisal of the problem of objectivity is badly needed.

Then there is the bearded supposition that moral decisions matter greatly to the individual who makes and subjects himself to them, whereas scientific ones do not. The implication is this: If I decide not to steal I shall be out a certain sum of money (a most unpleasant prospect!), whereas in making an error in a calculation the effect does not concern me directly. I defy anyone who has had both experiences, and who says that the rigors of scientific discipline, especially to one as unskilled in science as most of us are in moral matters, are less arduous than the stamina it takes to resist temptation. When carefully inspected, such arguments invariably break down.

More important and perplexing is the problem of generating a code of ethics, which is of course the starting point of the entire enterprise. Shall we continue historical practice and wait until some great moralist, by the persuasive force of his personality, sweeps us in his train? Or shall we let the state impose its moral dicta in the manner in which it enforces its laws? Laws are, after all, a crustlike moral code near the periphery of ethical concerns. Or shall we stage a convention of elected moralists and appoint them to the task of formulating a code? The fact that such possibilities, when thus concretely phrased, sound silly is indeed an indication of our immaturity in the field of ethics. I am not endeavoring here to make a contribution to these difficult problems, except to say that they must and can be faced.

III

In the following, we shall avail ourselves of the facilities offered by our parallelism, to comment upon some of the traditional paradoxes of moral philosophy. It will be seen that each of them has as its counterpart a certain problem in the philosophy of science, which in most of the instances to be recorded has been solved. It seems plausible to assume that the solution offered on the side of science, if it has a correlate on the side of ethics, is at least suggestive of a way in which the ethical problem can be attacked. The procedure will be illustrated under five queries.

1. *Is ethics an autonomous discipline?* This question has already been answered in part. The subject matter of ethics cannot be wrung from science. Though both have similar structure, each has its own postulates or norms, and each requires specific acceptance or, if it seems more adequate to use a moral term, personal dedication to norms. This disposal of the problem does not preclude the possibility that ultimately the axioms of ethics may be reducible to those of science, just as chemistry and astronomy have become branches of physics. The point is that they are not thus reducible at

present. In my own opinion, the reduction is not likely to be achieved, and the worst attitude would be to wait for it.

2. *Is ethics to be based upon a theory of values?* Ethics and the theory of values are closely linked by tradition, and the weight of much authoritative writing is behind this tradition. To brush it aside without careful consideration seems unbecoming, nay, frivolous—yet the verdict of scientific ethics is very clear on this point. For the problem at issue has an exact corollary in the dispute over the relevance of *facts* in science. Translated, the question becomes: Is science to be based on facts?

Science has often been defined as a systematization of facts. But the bluntness, the inadequacy of this formulation, become apparent as soon as the meaning of fact is examined. If fact is considered synonymous with all that is found to be true on inspection, the definition is empty and valueless, because it includes too much that is trivial. A complete catalogue of mere matters of fact is not a science. And if the meaning of fact is restricted to *significant* fact, the definition begs the question. For it is science itself which confers significance; prior to the adoption of principles, no fact of observation, however trustworthy, has claim to importance. A person who painstakingly weighs every grain in a large pile of sand makes no contribution to science unless he is perchance verifying or refuting some hypothesis or law. In the accurate sense, then, a fact of science is a datum with relevance to postulates. And it can easily happen that a datum takes on relevance long after it has been discovered, although this is by no means assured. Hence a scientist would be misguided if he went about collecting facts indiscriminately in the hope that laws will be found someday to reward his labors.

As principles are prior to fact, so are ethical norms prior to values. Psychologically, anything can take on value. There is nothing intrinsic in human behavior, aside from anthropological statistics, that allows a qualitative distinction to be made between good actions and bad. The psychology of a dope addict's craving is quite the same as normal hunger. Furthermore, our belief in the efficiency of moral education is a clear demonstration of the conviction that things which appear good can be made to seem bad, that motives can be strengthened or weakened, that things of little value to the untutored can take on great value with understanding and with use. All this implies the logical priority of *principles*, of *codes* of behavior.

What the theory of values is patently meant to do is to persuade people that certain standards of behavior are more acceptable than others. Values are offered as prizes, neatly wrapped, for the acceptance of norms. But upon analysis, the circularity of this procedure emerges every time: Values are merely products of some code of behavior which the advocate of the code cherishes or wishes to propagate. To recognize the secondary nature of values and to concentrate upon the primary issues, namely, the norms which generate values, cannot but be of benefit to ethics.

But surely, the reader will say, a code of ethics is not something one offers for acceptance in its own right, as an abstract thing from which all blessings flow. Are we not treating this so-called code too much like a mathematical formula? Here I feel that perhaps we are not treating it enough like one. At any rate, we are losing sight of the ominous fact that, when it is so treated, enormous consequences for good or evil can follow. Dictators have seen this point. Good teachers know it, for they rely on personal example (which always sets a norm directly), rather than upon the secondary use of values. In fact, the moralist who is reduced to reasoning about values, has usually lost his grip on the pulsating flow of human behavior

Although unreasoning acceptance of norms is possible—indeed, is far more widely practiced than we citizens of a democracy like to believe—it is not advocated here. Nor is it what the parallelism between science and ethics demands.

A postulate cannot be separated from its consequences, and it is because of these consequences that it is introduced. This must be true in ethics as well.

What we need, however, is greater emphasis on these postulates and less on values. Too few of us know what are the guiding principles of our behavior. Yet we all know our likes and dislikes, our values.

3. *Are ethical standards relative or absolute?* In answer to this question the scientist is expected to sing the praises of the theory of relativity, which, as many writers see it, has invaded the field of ethics. This belief rests on a misunderstanding of the physical lawfulness summed up in the theory of relativity, which asserts a kind of *in*variance for the workings of nature that is completely falsified by the customary phrase: all things are relative. The postulate of relativity has an air of absoluteness, of independence, shared by very few generalizations in any science. One cannot draw upon it in an endeavor to prove that standards are relative.

Science does in fact not give comfort to the relativist in ethics, for the laws of nature remain

unchanged as we pass from one situation to another. The so-called principle of uniformity of nature, assumed in one form or another throughout science, operates to deny the type of relativism usually envisaged in ethical applications. If we follow its lead, we arrive at the clear decision that norms do not permit variform interpretations under different circumstances.

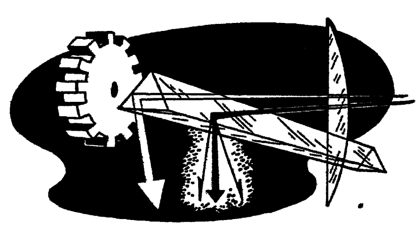
But axioms do change in time. As the scientist learns more about his universe, he takes occasion to review his axioms and his laws, bringing them into better conformity with his experience. Older formulations are seen to be only approximate, or indeed cease to be valid. This self-regulating dynamism is one of the wholesome features of scientific progress, and if we read its implications aright, it teaches the lesson that ethics, too, might well temper its insistence on eternal truth and consider an occasional review of time-honored standards. If this is what we may understand by relativity in ethics, then the success of science affirms it.

4. *Are ethical standards derived from more elemental and primary considerations?* We have already indicated the answer to this question. As norms are the counterparts of axioms, they are themselves necessarily primary. Human acquiescence to them can be wholehearted and effective despite their logical parthenogenesis, and one can become convinced of their validity as thoroughly as one longs for an ideal. In a practical sense, we must of course acknowledge the psychological force of auxiliary motives, be they as base as hope for reward and fear of punishment, or as refined as the love of fellow-men. To enlist such motives is the task of the practical moralist, who, even as an ethical scientist, does not violate his integrity by their use. For does not the teacher of arithmetic, though he be fully aware of the postulational nature of the number system and of its limitations, appeal to instances (marbles, fingers, etc.) in which it is helpful? He finds it quite unnecessary to warn the pupil against employing the multiplication table with Heisenberg's matrices.

5. *Do we accept a standard of behavior because of the inherent qualities of the standard, or in anticipation of its practical consequences?* In other words, do we perform good acts for their own sake, or because of their effects on ourselves and others? Both alternatives have been fervently avowed by different schools of ethics. More common, perhaps, is adherence to the latter thesis, for every form of eudaemonism, every proposal to seek the highest good, is an affirmation of it.

Science is afflicted by a very similar controversy between *causal* and *teleological* description. On its outcome a final verdict cannot be rendered. Causality seems to be winning the race inasmuch as teleological principles, like Fermat's, can be translated into differential equations which imply causal relations. In conclusion, then, we may probably say that scientific ethics, as we have defined it here, favors the view that moral action is compulsory on man for its own sake, and this is in satisfactory accord with the postulational attitude demanded by our parallelism on other grounds.

The loosely coordinated remarks of this note were not intended to develop a systematic theory of ethics. They are particularly defective in their omission of almost all reference to the problem of confirmation, whose treatment is by far the most difficult on the side of science, and therefore presents challenging aspects to ethics as well. My purpose was to sketch an approach which appears to have some promise.



THE PROBLEM OF CORAL REEFS*

H. S. LADD and J. I. TRACEY, JR.

Dr. Ladd, who has studied coral reefs in the Pacific for the Bishop Museum and the University of Rochester, has been with the U. S. Geological Survey since 1940. Mr. Tracey, who is also on the staff of the Survey, was, with Dr. Ladd, one of the geologists who did research on the coral reefs of Bikini (Operation Crossroads).

DURING the past ten years much new interest has been aroused in the ancient and controversial problem of coral reefs. Some of this interest may be traced to the publication of papers describing prewar researches, part of it to work carried on during the war when reefs and related structures were intensively studied, and part to postwar investigations; also, in this decade additional deposits of petroleum were discovered in a number of ancient reefs, and this aroused the interest of petroleum geologists in the subject of reefs in general. A number of recent papers have been directly concerned with the origin of existing reefs. Some of these studies dealt primarily with reef surfaces and submarine mapping, others reported on deep drilling into existing reefs, and still others dealt with geophysical data on reef foundations not yet reached by the drill. Plans for additional work in all these fields are being made, and it seems appropriate to review the general problem very briefly, stressing recent developments and pointing out what appear to be promising lines of approach for future investigations.

THE REEF PROBLEM IN EARLY DAYS

No doubt the first travelers ever to see living coral reefs observed some of their peculiar features and speculated on their mode of formation. Actual records of this kind go back at least to 1821 when Chamisso[4] published his observations; this was sixteen years before Charles Darwin[9] presented the first statement of his famous theory. Pre-Darwinian speculations aroused little interest, though they may now be interpreted as preliminary rumbles of an activity that was to reach volcanic proportions some decades later. Darwin's observations led him to suggest that a fringing reef growing on a slowly subsiding foundation could be transformed into a barrier and finally into an atoll by organic growth. Seldom, perhaps, has a new idea been more quickly or more widely accepted than was Darwin's. Additional investigations by J. D. Dana[8] gave considerable support to Darwin, and it was not until 1863 that the first real opposition was brought forth by Carl Semper,[36] who had worked in the Palau (Pelew) Islands. Semper concluded that atolls could be formed in areas of elevation, their lagoons being formed by solution as the reefs grew outward. He took account of other factors also, but his ideas on the whole were the antithesis of those held by Darwin and Dana. Later, others followed Semper's lead; the period from 1880 to 1910 was a particularly prolific one, and many able investigators—John Murray, J. J. Rein, H. B. Guppy, W. J. L. Wharton, J. Stanley Gardiner, Alexander Agassiz, and others—took issue with Darwin and proposed a variety of alternate theories. The chief support of Darwin's theory in this period came from the deep boring at Funafuti[16] in the Ellice Islands, but the results of that boring were given very diverse interpretations.

In 1910 R. A. Daly[6] called attention to the speculations of A. Penck on the relations between Pleistocene glaciation and the forms of coral islands and outlined in some detail the glacial control theory. In this and many later papers Daly explained how coral reefs could have originated on the banks left close to sea level at a time when sea level was low because of the removal of waters to form glacial ice on the land. Later, as the glaciers melted, the reefs grew upward with a rising sea level. This theory, like Darwin's, won many supporters from the start, and it seems fair to state that it has had at least as many adherents as its older adversary. Indeed, since the glacial control theory became established, no student of coral reefs has felt justified in ignoring its postulates entirely.

Many of those who have studied coral reefs—including some that antedate Darwin—believed that a pre-existing platform of some sort was essential to the development of reefs. Of these, some postulated a change of sea level as necessary, others felt such a change was not essential.

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Theories calling for antecedent platforms have not been as widely held or as vigorously defended as either of the two mentioned earlier.

THE THIRTY YEARS' WAR

During the next thirty years—1910–39—the subsidence theory of Darwin and Dana was vigorously defended, particularly by William M. Davis, and Daly developed the opposing glacial control theory. In 1928, six years prior to his death, Davis[10] published a volume entitled *The Coral Reef Problem* which, though it may be characterized as "a full and unprejudiced account of the coral reef problem from the Darwinian point of view," brought together nonetheless a tremendous amount of very valuable factual information. Three years later J. Stanley Gardiner[17] published his *Coral Reefs and Atolls,* the volume being based on a course of lectures given in 1930 at the Lowell Institute at Boston. Although not documented in detail as was Davis' work, this volume shows a more objective approach and is probably the finest general presentation of the subject of existing reefs yet written.

In this interval T. Wayland Vaughan[46] wrote a great many papers about corals and coral reefs, based primarily on his studies of Tertiary and later structures in the Caribbean and Central America. Vaughan, like Gardiner, was familiar with reef organisms and their requirements. He recognized the necessity of studying each reef in relation to its local environment and was aware of the dangers of generalizations based too largely on physiographic form.

M. A. Howe[23] and, later, W. A. Setchell[37] called attention to the importance of algae in the building of "coral" reefs. A. G. Mayor[31] made detailed studies of the reefs of Samoa, later followed by R. T. Chamberlin.[3] H. A. Brouwer,[2] W. H. Hobbs,[20] P. Marshall,[29] G. A. F. Molengraaff,[32] H. Yabe,[48] and others studied the elevated reefs of parts of the Pacific. L. J. Chubb[5] published a number of papers giving the geological results of the St. George Scientific Expedition to many island groups in the Pacific in 1924–25, and Ph. H. Kuenen[24] as geologist on the Snellius Expedition in 1929–30 reported on many reefs in the East Indies. Hoffmeister[21] and Ladd[26] published the first accounts of their work in Tonga and Fiji.

During this period most workers had very definite ideas about reef origin and lent their support either to the subsidence group headed by Davis or to the glacial control group led by Daly. In his book on the coral-reef problem Davis lists thirty-five papers written by himself and a dozen published by Daly. Twenty years have passed since Davis published his volume, and Daly is still actively producing papers and books on the reef problems. These two indeed led the field in productivity.

THE PAST DECADE

During the first half of this period, which covered roughly the years of World War II, only a few papers appeared and these dealt mainly with prewar investigations. In 1939, prior to the outbreak of hostilities, J. H. F. Umbgrove[44] published an account of the atolls and barrier reefs of the Togian Islands in the Dutch East Indies Clearly recognizing the importance of sedimentation in reef development, and unable to find any evidence to support glacial control, he described instead the evidence for crustal movements during the Pleistocene. He stated that the elevated reefs in the area were formed in late Tertiary or early Pleistocene time, then uplifted and eroded, the existing reefs developing later on a subsiding fault block. In areas where subsidence was slow, reefs grew to the surface, fringing reefs being transformed into barriers in true Darwinian fashion.

In the twenty-year period that separated World Wars I and II, the Japanese were in control of all of Micronesia, and foreign scientists had little opportunity to study in the area. The Japanese themselves carried on a great deal of work, but much of this was not published, or at least was not generally available until the end of World War II. One of the most active workers on Micronesian coral reefs—both the existing reefs and the elevated limestones—was Risaburo Tayama, of the Tohoku Imperial University at Sendai. A number of Tayama's preliminary papers have recently become available, but his final report on the character and distribution of coral reefs in the South Seas is still in manuscript form. It covers all his investigations in Micronesia from 1932 to 1943. In 1940–41 the Japanese issued a two-volume Jubilee Publication to commemorate the sixtieth birthday of Professor H. Yabe. Copies of these volumes were not available in this country until the end of the war in 1945. Volume II contained a description by Shoshiro Hanzawa[18] of cores from a deep drill hole on Kita-Daitô-Zima (North Borodino), a small island east of Okinawa. The hole was drilled in 1934–36 to a depth of 1,416 feet. (Reports on the drilling were published in Japanese by Rokuro Endo in 1935 and by Toshio Sugiyama in 1936.)

In 1922 H. C. Richards[34] gave a talk to the Royal Geographical Society of Australasia

(Queensland Branch), entitled "Problems of the Great Barrier Reef." This reef has long been recognized as the greatest of them all, extending as it does more than 1,000 miles off the northeast coast of Australia through 15 degrees of latitude and 11 degrees of longitude. Richards outlined a program to determine the origin, growth, and natural resources of the reef area. His recommendations led directly to the formation of the Great Barrier Reef Committee, a group of some sixty members representing leading scientific societies. The first six volumes of the Committee's reports were issued between 1925 and 1948 and contain many interesting data, particularly the results of drilling at two points on the reef.

The scientific director of the Great Barrier Reef Committee was Charles Hedley. His death in 1926 was a severe blow to the committee's program, particularly in the field of marine biology. Consultations with J. Stanley Gardiner led to the formation of an influential committee in England that organized the Great Barrier Reef Expedition of 1928–29, under the leadership of C. M. Yonge.[49] This group spent a year on the Great Barrier, and the results of their investigations are appearing in England in a valuable series of quarto volumes. These reports deal with physiology of reef organisms, the biochemistry of their environment, and the processes of erosion and sedimentation on reefs and low islands.

In 1943 T. Wayland Vaughan and John W. Wells[47] published a revision of the Scleractinia. This volume, though dealing primarily with the systematic classification of the stony hexacorals, included a summary of the distribution of reef-building types both geologically and geographically.

In 1944 Hoffmeister and Ladd[22] summarized the evidence for the antecedent-platform theory. This paper was the last of a series of three, the first two having dealt with the subsidence theory and the theory of glacial control. Their observations and conclusions were based almost entirely on field work done in Tonga and Fiji. Both of these island groups are inside the Melanesian continental area where, as most workers will agree, uplift rather than subsidence characterized late Tertiary and post-Tertiary time. There are thick sections of elevated limestones in both island groups that earlier workers had cited as evidence of subsidence. It was found, however, that these limestones represented several periods of deposition, and furthermore that many of them contained no reef corals or other evidence of deposition in the shallow waters of the reef-coral zone. As they found no sections of unquestioned elevated reefs thicker than 300 feet, these investigators concluded it was unnecessary to call on subsidence to explain the elevated reefs. Likewise, since many of the elevated reefs were found to be Tertiary in age, they could not accept glacial control as a general explanation, though they stated that changes of sea level had certainly stimulated the growth of reefs in Pleistocene times. In 1945 a full account of their work in Lau (eastern Fiji) was published.[27]

Since 1945 Hoffmeister and Ladd have studied both existing and elevated reefs in various parts of Micronesia and now feel that they were too restrictive in their identifications of "reef limestone." They failed to recognize that "reef structure" (imbricating colonies of flat reef corals in positions of growth such as characterize the marginal zones of existing reefs) forms a very small percentage of the entire reef. It is an important part, to be sure—as important as the sides of a pail that holds water—but it may make up only 5 or 10 percent of the reef mass, and furthermore may be the first part to be destroyed when the reef is elevated and eroded. By thus recognizing the quantitative unimportance of reef structure, the occurrence of scattered reef corals that are *not* in position of growth assumes greater significance. Such occurrences may indeed suggest a talus slope or deposition on a submarine bank, only small parts of which projected into the zone of reef growth, but it is perhaps more likely that such scattered corals were deposited on the wide reef flat behind the marginal zone or in the shallow waters of a lagoon.

A second factor overlooked by Ladd and Hoffmeister was the significance of the texture of sediments.[25] Fine sediments do not accumulate on unprotected banks, and, therefore, the occurrence of thick sections of such materials is evidence of the existence of a protecting rim at the time of accumulation, even though such a rim may no longer exist.

In 1945 Harold T. Stearns published a summary of the "Late Geologic History of the Pacific Basin,"[40] which was followed the next year by his "Integration of Coral-Reef Hypotheses."[42] Both of these are very stimulating papers that offer solutions for most of the problems of the Pacific, including those involving coral reefs.

Stearns expressed the belief that great eustatic shifts of sea level occurred during Pliocene and Pleistocene time as a result of changes in the configuration of the ocean basins. On a map accompanying each of the articles, he drew the "Sial Line" around the continental islands of the west-

ern Pacific (this line is a little east of the "Andesite Line" of earlier writers) and stated that during Tertiary and early Pleistocene time reefs developed on rising foundations (continental, or sialic, islands) to the west of this line, whereas to the east reefs developed on subsiding islands (oceanic, or simatic, islands). He also expressed the belief that only the glacial control theory could explain the vast majority of living reefs on both sides of the Sial Line.

One of the attractive features of Stearns' integration is his conclusion that the merits of the leading coral-reef hypotheses vary according to the geologic age of the reef and its location relative to regions of uplifting and subsidence. Such recognition gives to each hypothesis both a time and a place to operate. If Stearns' basic structural interpretation is correct, it offers an explanation for the puzzling regional relations in the Pacific, particularly the concentration of low islands in the simatic area. In describing reef development Stearns starts with the Pliocene and cites Andrews' conception of a great continental area creeping by undulations against a subsiding central Pacific block. According to Stearns, submarine folds were formed, and, as they rose toward the surface of the sea, reefs developed on their crests. He recognizes that many of the atolls are very broad—too broad to have been made by marine plantation during the glacial epochs of the Pleistocene. He feels that such wide atolls are crowns on broad anticlinal crests. This idea is graphically presented in a series of sections through banks and atolls. These diagrams, however, are greatly exaggerated vertically, and for that reason tend to be misleading. Kuenen[25] points out (p. 14) that the exaggeration obscures the fact that, whereas an atoll may be a few to many kilometers wide, the range in depth between the lagoon floor and the maximum depth for coral growth is very small, if not negative. Kuenen concludes: "One cannot assume a topographic high, as flat and horizontal as a billiard table to have been formed by diastrophic agents before the reef grew." If the explanation diagrammed by Stearns is to be applied to many atolls, a lot of narrow-crested folds and quite a number of broad ones would have to be stopped at critical levels very close to the surface—the banks within 600 feet of the surface, the potential atoll foundations within 300 feet. During a time of lowered Pleistocene sea level the banks would develop reefs while the atolls were being truncated; with a return of high level both types of structures would become atolls. These and the repetitive stages to follow in succeeding glacial stages call for the accumulation of coral talus slopes. This is a slow process, and, as Kuenen points out, there can hardly have been sufficient time for such accumulation during the low levels of the Pleistocene—at least not in the manner indicated by Stearns' diagrams.

It is perhaps unnecessary to point out that Stearns' diagrams, probably for the sake of brief presentation, greatly oversimplified conditions in the sialic part of the Pacific. In certain island groups the distribution of islands and atolls in broad arcs fits a fold pattern very well, but in other groups the arrangement is much more complicated.

In his paper on the integration of reef hypotheses Stearns refers to recent negative shifts of the sea, and states that most modern reefs are decadent as a result of these shifts and that many are practically devoid of living reef organisms. Stearns suggested, in a paper published some months earlier on a decadent coral reef on Eniwetok Island,[41] that a very late local change of sea level of a few feet in the Marshall Islands caused the decadence observed there. The stretch of reef specifically referred to is on the southeast side of the atoll, a section that was subjected to heavy bombardment a short time before Stearns visited it in June 1944. This bombardment and the spreading of fuel oil from ships were probably largely responsible for the condition noted by Stearns. When the same reef was examined by Tracey in June 1946, the reef was still dead, and much of the limestone was spongy and rotten from the action of boring organisms. Live corals and algae, however, were beginning to repopulate the marginal zone, and everywhere else on the atoll the reef was healthy, with algae and corals flourishing in the marginal zone. The reefs of nearby atolls (Bikini, Rongelap, Rongerik) were also examined by Ladd and Tracey and found to be healthy.

In 1946 H. H. Hess[19] described a series of seamounts—which he calls *guyots*—under the intriguing title "Drowned Ancient Islands of the Pacific Basin." He located about 160 flat-topped peaks between the Hawaiian Islands and the Marianas. These structures resemble truncated volcanic cones rising 9–12 thousand feet above the ocean floor, their tops lying 3–6 thousand feet below sea level. Some of the structures are isolated, but others adjoin atolls, two at Eniwetok in the Marshall Islands appearing to pass partly under the atoll in a curious relationship that suggests for the seamounts an age greater than that of the atoll. These structures are still but little known. Half a dozen short cores obtained by Emery from the seamount adjoining Bikini consisted only of

Globigerina sand. In 1947 the Navy and the Geological Survey flew a magnetometer over the seamount adjoining Bikini, finding a magnetic anomaly approximately twice the magnitude of that of Bikini itself.[1]

In 1946 Kuenen gave a series of special geological lectures at London University. The subject matter of two of these lectures was published the following year as a very interesting paper entitled "Two Problems of Marine Geology: Atolls and Canyons."[25] Kuenen is a skilled and experienced worker both in the field and in the laboratory. He was the geologist on the Snellius Expedition, where he gained firsthand information on reefs in parts of the Dutch East Indies. His full reports on the geology of the coral reefs[24] and his interpretation of the bathymetric results were published in 1933 and 1934. In his 1947 paper, leading coral-reef theories are critically reviewed, and a merger involving the two leading theories is proposed under the name "glacially controlled subsidence." Under glacial control he stressed solution of exposed preglacial reefs, concluding that by such means alone elevated rims were cut down partly or entirely to glacial sea level.

He believed that the proposed combination could meet the main objections raised against either subsidence or glacial control when applied separately, and that it accounted for some obscure points not previously stressed. Thus, denudation by chemical means yielded no smothering sediment and permitted reefs to flourish along the edges of platforms at glacial levels, and as a result limited postglacial reefs to the peripheries of such platforms. Kuenen argued for extremely slow preglacial subsidence as compared with rapid postglacial rise of sea level. He stated that very deep passages and deltas in front of gaps in atoll and barrier rims are rare or absent in coral-reef areas because preglacial subsidence was so slow that any gaps formed could be closed by coral growth. Postglacial rise of sea level, on the other hand, was held to be much faster, and hence many shallow gaps were left unfilled because the corals were not able to keep up with the rising waters.

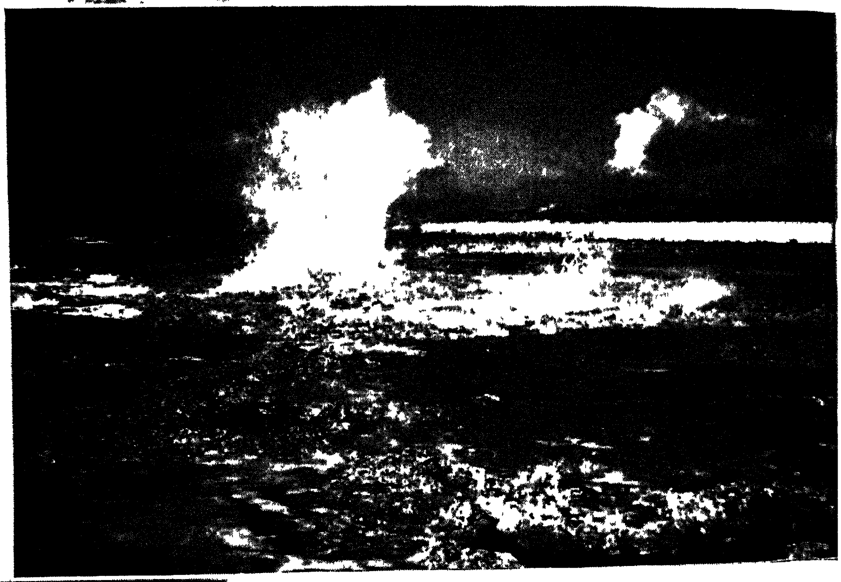
Geological evidence of widespread solution that is limited to the intertidal zone and particularly effective at low tide level, has been recently cited by Kuenen,[25] Umbgrove,[45] and Fairbridge and Teichert.[15] Solution phenomena had been observed by earlier workers, especially by Murray[33] and Gardiner,[17] who claimed that lagoons were hollowed out by this means, but oceanographers have pointed out that normal sea water is saturated with calcium carbonate and, therefore, incapable of dissolving limestone Kuenen thought that solution might truncate large areas, but Daly questions this possibility.[7] Evidence showing, however, that water on reef flats varies considerably in its composition from that of normal sea water has been presented by biochemists working on the Great Barrier Reef[49] and at Bikini. The possibility of solution on a large scale as a result of this variation must be considered. In 1946 K. O. Emery[12] attributed the development of tide pools in calcareous sandstone at La Jolla to the diurnal variation in CO_2 of the pool water caused by the biologic processes of organisms inhabiting the basins. In the daytime, animal life uses up oxygen and respires carbon dioxide, whereas marine algae use up carbon dioxide and give off oxygen by photosynthesis, resulting in a marked increase of oxygen in solution. During the night, when photosynthesis does not operate, both forms of life give off carbon dioxide. At night, therefore, the increase of carbon dioxide in the water increases the amount of calcium ion that can be held in solution, and a dissolving of the rock walls of the basins would be expected. Conversely, during the daylight, the drop in carbon dioxide in solution, by this process as well as by the increased temperature of the basin water, results in deposition of calcium carbonate as a fine precipitate. As the precipitate would be flushed out by waves at high tide, the resultant of the daily cycle would be an enlargement of the basin by solution of the rock. Such a process is probably of great significance in the development of reefs, both because there it acts effectively over a broad flat that may be covered by only a few inches of water at low tide, and because of the abundance of organic life on the reef.

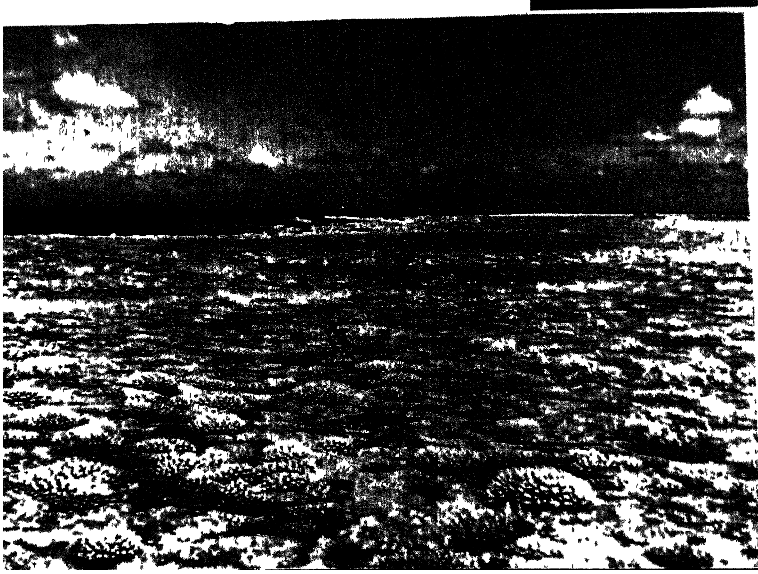
In 1947 J. H. F. Umbgrove[45] published a comprehensive paper reviewing all work on the coral reefs of the East Indies. He pointed to evidence for widespread subsidence and again expressed the opinion that the shifting of sea level in the Pleistocene was a factor of only minor importance in the development of East Indian reefs. He stressed the importance of sedimentation in lagoon filling and in the retardation of reef growth.

A brief report on the drilling done in 1947 during a resurvey of Bikini Atoll (Operation Crossroads) was published by Ladd, Tracey, and G. G. Lill[28] in 1948. In this paper it is noted that shallow-water assemblages were found at great depths, and the Bikini findings are compared with the results of drilling done on coral islands and reefs in other parts of the Pacific. It is clearly shown that we are not yet ready to generalize or to predict what the next deep hole on a coral

Part of an atoll reef View looking northwest from Bikini Island, taken at about five hundred feet. The island is about half a mile wide, to the right is the open ocean, to the left the lagoon.

Reef at Enyu Island, Bikini Atoll, showing fissures and open pools in the reef flat. The blowhole, fed by surf, is spouting water twenty-five feet into the air.





Coral growth on reef near Enirik Island, Bikini Atoll. Most of the flat is covered by only a few inches of water at low tide; the exposed coral colonies in the foreground are approximately a foot in diameter.

island will reveal. Thus, of the three deep holes drilled on islands in the open Pacific—on Kita-Daitô-Zima (a small island east of Okinawa), on Bikini, and on Funafuti in the Ellice Islands—the hole on Kita-Daitô-Zima was consolidated in its upper part, the one on Funafuti was consolidated in its lower part, and the one on Bikini, the deepest, was not consolidated at all. The Kita-Daitô section was dolomitized in its upper part, Funafuti in its lower part, Bikini not at all. The ages of the rocks penetrated likewise varied considerably.

Late in 1948 appeared the preliminary reports of Emery[14] and Tracey, Ladd, and Hoffmeister[43] on surveys carried out in 1946 in connection with Operation Crossroads. Emery, who was primarily responsible for the charting of lagoons and submarine slopes, showed clearly in the case of Eniwetok that the lagoon floors of atolls are not the smooth, saucerlike depressions many conceive them to be. When the 180,000 soundings obtained by the Navy in Eniwetok's 24-mile lagoon were plotted and contoured, they revealed terraces, depressions, and coral knolls of varying height. In this small lagoon more than 2,000 coral knolls were located.

Francis P. Shepard's[38] pioneering volume *Submarine Geology* appeared in 1948. It includes a chapter on coral reefs that is in effect a very good summary of the problem. He cites many of the most recent discoveries, relates the reefs to other submarine features, and wisely avoids the temptation to make final interpretations. In an article summarizing the evidence of world-wide submergence, published late in 1948, he concludes[39] that some of the indicated submergence was Pleistocene and some Tertiary or even earlier. His reason for extending the data into pre-Pleistocene time is largely the evidence from drilling on coral islands in the Pacific (Bikini and Kita-Daitô-Zima) and in the Atlantic (Bahamas).

The latest contribution to the resurgent coral-reef problem is one by Milton B. Dobrin, Beauregard Perkins, Jr., and Benjamin L. Snavely[11] describing geophysical work carried out on Bikini. In 1946 a seismic refraction survey was made by firing a series of depth charges on the lagoon bottom along four lines extending across the atoll, the resulting seismic waves being picked up by water-coupled microphones near shore. The time-distance curves indicated the existence of three zones of different sound velocity. The first of these was about 2,500 feet thick and showed a speed of 7,000 ft./sec.; the second extended to 10,000 feet with a speed of 11,000 ft./sec.; the third, a zone of undetermined thickness presumed to be the igneous basement, had a speed of 17,000 ft./sec. As a vertical velocity survey carried out in the deep hole drilled in 1947 showed a continuous transition from a velocity of 7,000 to 11,000 ft./sec. from the surface to 1,800 feet, the authors conclude that probably no essentially different rock materials appear in the atoll down to the foundation rock with the 17,000-ft./sec velocity. The authors also state that under this interpretation the change in velocity in the upper layers would be due mainly to progressive compaction and cementation of calcareous sediments, but other possibilities involving a change in texture, dolomitization, or a change in type of material are mentioned. They point out that the seismic data call for relative subsidence in thousands of feet.

FUTURE INVESTIGATIONS

It should be apparent from the brief review here presented that there is a widespread and growing interest in reefs and in the oceanic environments in which they are developed. Some of the questions raised in earlier days have been answered, but as our knowledge of reefs has increased, new questions and promising new lines of investigation have appeared. With the help of improved methods and new techniques developed in the fields of submarine geology and geophysics, we may hope to settle some of the outstanding questions in the near future, and almost all of them eventually. Meanwhile, there are certain promising fields that may be pointed out; in most of these there is already some activity.

Island geology. Reef-encircled islands of all types should be mapped geologically, the maps being parts of an over-all survey in each island group. The U. S. Geological Survey is now engaged in such a program in Micronesia and the Ryukyus. Geological mapping is done on air photographs at scales of 1:10,000 or less, with a view to publication at a scale of 1:50,000; as a part of the study, reefs and lagoons are examined in some detail. Surveys of this type have recently been completed in Okinawa, Palau, Yap, and Saipan.

Areal studies should include detailed investigations of the effectiveness of solution: (1) on limestones above high tide; (2) on limestones exposed between tide levels; and (3) on limestones beneath the island but within the lenticular mass of fresh water (Ghyben-Herzberg lens) that, on the larger coral islands, extends to appreciable depths. There is evidence pointing to effective solution in all three of the above-mentioned en-

vironments, but supporting analyses are lacking, particularly for limestones below sea level. Quantitative data on existing conditions would be very valuable in appraising the importance of solution during the low levels of the Pleistocene.

Reef studies. Additional ecological studies such as those carried out on the Great Barrier[49] and at Bikini[35] on organic productivity on reefs are needed. These and related studies indicate the amount of organic matter produced, the amount available for burial in reef sediments, and set limits to the rate of reef growth under existing conditions.

Lagoon studies. Mapping and coring of lagoon sediments yield data that are of great value in the interpretation of elevated limestones and of the cores and cuttings obtained from drilling. When such information is combined with data obtained from dredging on the outer reef slopes, it is possible to make sound paleoecological interpretations and to determine the significance of the several types of limestone that occur in reef areas.

Recently completed detailed charts of a number of lagoons have revealed numerous coral knolls rising from the floor to varying levels, some of which are very close to existing sea level. A little has been learned about the surface features of knolls, but their internal constitution is unknown; no structures of this type have ever been drilled.

Submarine geology. Something of the abundance and distribution of seamounts in part of the Pacific is known, but, to date, as previously noted, very little else has been determined. Dredging and additional coring of these structures should give more clues to their origin and, perhaps, their age. Specifically, it is essential to learn if they have a hard rim with a central depression now filled with sediment; to determine if the coating of Recent sediments is thick, and if it contains pebbles of hard rock suggesting an origin by wave erosion.

Studies of isolated banks in the coral seas at 300–1,000 feet below sea level would give needed data on the types of sediments that accumulate and the kinds of organisms that live in reef areas at depths below the limit of reef-coral growth. Interesting comparisons could be made between accumulations on such banks and Tertiary sediments that are known to underlie reefs and coralliferous limestones on many Pacific islands. Emery[13] made such a study of the Ranger Bank that lies at 67 fathoms off the coast of Mexico, but in an area where reefs are sparingly developed.

Island foundations. The origin of coral reefs will probably never be settled to the satisfaction of many investigators until a great deal more is learned about the foundations of existing reefs. To date a considerable amount of information has been obtained from drilling and from geophysical investigations. The latter, of course, give only strong indications of the nature and distribution of the materials of the foundation, but these indications in themselves are quite valuable and such drilling as has so far been done bears out geophysical predictions fairly well. The seismic and air-borne magnetometer surveys in the northern Marshall Islands have already been referred to. Additional seismic studies are planned in that area, and it would be desirable to make some gravity studies. A pioneering effort in this field was carried out by the Japanese[30] on Jaluit Atoll in 1918. With little effort, a few measurements could be made in the lagoon and on the islands, but if such a survey is extended beyond the lagoon a submarine will be needed because of the steepness and the possible irregularities of the reef slopes.

One or more holes drilled through a coral atoll will certainly be required for a final solution of the reef problem. Plans for such a project have been made and will be carried out if financial support can be obtained to supplement that already offered by the Geological Society of America and the Office of Naval Research. Bikini has been selected for the proposed operation because at the present time more is known about that atoll than about any other. Geophysical data indicate that the hole would have to be 7,000–10,000 feet deep (depending on the site chosen). The operation would give valuable information on many fundamental geologic problems other than the origin of the atoll itself. Among these are stratigraphic, paleontologic, and paleoecologic data on an unknown geologic section; siesmic velocities and other geophysical information needed to interpret surface surveys; data on the Tertiary and earlier history of the Pacific Basin; information on the post-depositional alteration of carbonate sediments by compaction, cementation, recrystallization, dolomitization, and by differential solution; and data on the effects of the above-mentioned diagenetic processes on porosity and permeability of limestone reef rock. Much of this information would be useful not only in studies of other existing reefs, but also in studies of ancient reefs, including the oil-bearing structures of the Paleozoic.

After the present article was sent to press, the writers learned of a brief review of coral-reef theories published in New Zealand (COTTON, C. A. The Present-Day Status of Coral-Reef Theories. *New Zealand Sci. Rev.*, 1948, **6**, (6), 111–13.).

REFERENCES

1. Alldredge, L. R, Keller, F., Jr, and Meuschke, J. L. *Aeromagnetic Map, Bikini Atoll.* Open file release, ONR and U. S. Geol. Survey, June 1948.
2. Brouwer, H. A. The Origin of Coral Reefs and Reef Caps with Special Reference to Mountain Building within the Netherlands East Indies. *2nd Pac Sci. Cong, Proc.* **2**, 1926, 1164–67.
3. Chamberlin, R. T. The Geological Interpretation of the Coral Reefs of Tutuila, American Samoa. Carnegie Inst. Wash., 1924, Pub. 340, 147–78
4. Chamisso, A. von. Bemerkungen und Ansichten auf einer Entdeckungsreise unternommen in den Jahren 1815 bis 1818 . . . . 1821, **3**, Weimar.
5. Chubb, L. J. The St. George Scientific Expedition *Geol. Mag.*, **62**, 1925, 369–73.
6. Daly, R. A. Pleistocene Glaciation and the Coral Reef Problem. *Amer. J. Sci*, 1910, Ser. 4, **30**, 297–308.
7. Daly, R. A. Coral Reefs—A Review. *Amer J. Sci*, 1948, **246**, 193–207.
8. Dana, J. D. *Geology.* U. S. Exploring Exped. (Wilkes), 1849, **10**.
9. Darwin, C. On Certain Areas of Elevation and Subsidence in the Pacific and Indian Oceans, as Deduced from the Study of Coral Formations. *Proc. Geol. Soc. London,* 1837, **2**, 552–54.
10. Davis, W. M. *The Coral Reef Problem* Amer. Geogr. Soc., 1928, Spl. Pub. 9.
11. Dobrin, M. B., Perkins, B., Jr., and Snavely, B. L. Subsurface Constitution of Bikini Atoll as Indicated by a Seismic-Refraction Survey. *Bull. Geol. Soc. Amer.*, 1949, **60**, 807–828.
12. Emery, K. O. Marine Solution Basins. *J. Geol.*, 1946, **54**, 209–228.
13. Emery, K. O. Submarine Geology of Ranger Bank, Mexico. *Bull. Amer. Assoc. Petrol. Geol.*, 1948, **32**, 790–805.
14. Emery, K. O. Submarine Geology of Bikini. *Bull. Geol. Soc. Amer.*, 1948, **59**, 855–60.
15. Fairbridge, R. W., and Teichert, C. The Rampart System at Low Isles. *Reports of the Great Barrier Reef Committee*, 1948, **6**, 1–16.
16. [Funafuti report], *The Atoll of Funafuti,* Roy. Soc. London, 1904.
17. Gardiner, J. S *Coral Reefs and Atolls.* Macmillan & Co., Ltd., 1931.
18. Hanzawa, S. *Micropaleontological Studies of Drill Cores from a Deep Well in Kita-Daitô-Zima (North Borodino Island).* Jubilee Pub. in Commemoration of Prof. H. Yabe's 60th Birthday, 1940, **2**, 755–802.
19. Hess, H. H. Drowned Ancient Islands of the Pacific Basin. *Amer. J. Sci.*, 1946, **244**, 772–91.
20. Hobbs, W H. Reef Formations as an Index of Mountain-Building Processes. *2nd Pac. Sci. Cong., Proc.*, **2**, 1120–28.
21. Hoffmeister, J. E. Geology of Eua, Tonga. *Bull. Bernice P. Bishop Mus.*, **96**, 1932.
22. Hoffmeister, J. E., and Ladd, H. S The Antecedent-Platform Theory, *J. Geol.*, 1944, **52**, 388–402.
23. Howe, M. A. The Building of "Coral" Reefs. *Science,* 1912, **35**, 837–42.
24. Kuenen, Ph. H. Geology of Coral Reefs. *Snellius Exped., Geological Results,* **2**, 1933.
25. Kuenen, Ph. H. Two Problems of Marine Geology: Atolls and Canyons. *Verh. Koninklijke Ned. Akad. Wetenschappen,* 1947, **43**, (3), 1–69.
26. Ladd, H. S. Geology of Vitilevu, Fiji. *Bull. Bernice P. Bishop Mus.*, **119**, 1934.
27. Ladd, H. S, and Hoffmeister, J E., *et al.* Geology of Lau, Fiji. *Bull. Bernice P. Bishop Mus.*, **181**, 1945.
28. Ladd, H. S., Tracey, J. I., Jr., and Lill, G. G. Drilling on Bikini Atoll, Marshall Islands. *Science,* 1948, **107**, 51–55.
29. Marshall, P. Geology of Mangaia. *Bull Bernice P. Bishop Mus.*, **36**, 1927.
30. Matsuyama, M. Determinations of the Second Derivatives of the Gravitational Potential on Jaluit Atoll. College of Sci, Kyoto, Mem. 3, 1918, 17–68.
31. Mayor, A. G. Structure and Ecology of Samoan Reefs. Carnegie Inst. Wash, 1924, Pub. 340.
32. Molengraaff, G A. F. The Coral-Reefs in the East Indian Archipelago, Their Distribution and Mode of Occurrence. *4th Pac. Sci. Cong., Proc.* **2A**, 1929, 55–89; **2B**, 1930, 989–1021.
33. Murray, J. On the Structure and Origin of Coral Reefs and Islands. *Proc. Royal Soc. Edinburgh,* 1880, **10**, 505–18.
34. Richards, H. C. Problems of the Great Barrier Reef. *Queensland Geog. J.*, 1922, **36–37**, 42–54.
35. Sargent, M. C., and Austin, T. S. Organic Productivity of an Atoll. *Trans., Amer. Geophy Union,* 1949, **30**, 245–49.
36. Semper, C. Reisebericht (Palau-Inseln). *Zeitschr. für Wiss. Zool.*, 1863, **13**, 558–70.
37. Setchell, W. A. Nullipore versus Coral in Reef Formation. *Proc. Amer. Philos. Soc.*, 1926, **65**, 136–40.
38. Shepard, F. P. *Submarine Geology* New York: Harper, 1948.
39. Shepard, F. P. Evidence of World-Wide Submergence. *J. of Marine Research,* 1948, **7**, 661–78.
40. Stearns, H. T. Late Geologic History of the Pacific Basin. *Amer. J. Sci.*, 1945, **243**, 614–26.
41. Stearns, H. T. Decadent Coral Reef on Eniwetok Island, Marshall Group. *Bull. Geol. Soc. Amer.*, 1945, **56**, 783–88.
42. Stearns, H. T. An Integration of Coral-Reef Hypotheses. *Amer. J. Sci.*, 1946, **244**, 245–62.
43. Tracey, J. I., Jr., Ladd, H. S., and Hoffmeister, J. E. Reefs of Bikini, Marshall Islands. *Bull. Geol. Soc. America,* 1948, **59**, 861–78.
44. Umbgrove, J. H. F. Atolls and Barrier Reefs of the Togian Islands. *Leidsche Geol. Mededeelingen,* **11**, 1939, 132–87.
45. Umbgrove, J. H. F. Coral Reefs of the East Indies. *Bull. Geol. Soc. Amer.*, 1947, **58**, 729–78.
46. Vaughan, T. W. Corals and the Formation of Coral Reefs. *Ann. Rept. Smithsonian Inst. for 1917,* 1919, 189–276.
47. Vaughan, T. W., and Wells, J. W. Revision of the Suborders, Families, and Genera of the Scleractinia. *Geol. Soc. Amer.*, 1943, Spl. Paper 44.
48. Yabe, H., and Hanzawa, S. Raised Coral-Reefs of the Riukiu Islands and Taiwan. *Sci. Repts. Tohoku Imp. Univ.*, **7**, 1925, 29–56.
49. Yonge, C. M., *et al.* Great Barrier Reef Expedition 1928–29, British Mus. (N.H.). *Sci. Repts.*, 1930–39, 1–6.

PHOTOGRAPHY AS A BASIC RESEARCH TOOL*

BANNER BILL MORGAN and DEAM HUNTER FERRIS

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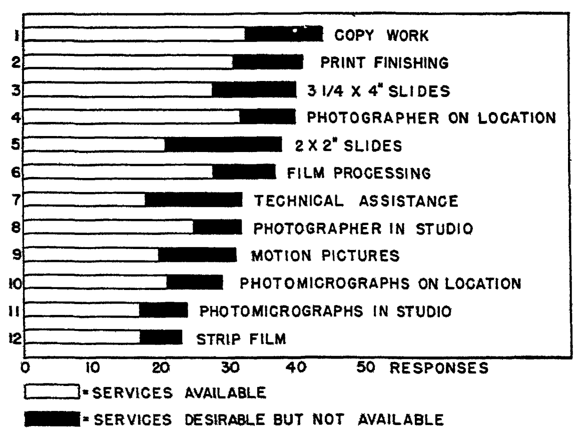
AT THE time the Annual International Photography-in-Science Salon was being organized by THE SCIENTIFIC MONTHLY and the Smithsonian Institution, Morgan and Ferris were conducting a survey to determine the extent to which colleges, universities, and commercial research laboratories were providing adequate photographic facilities for their scientists. The following situation appears to be typical in most institutions:

There are a confusion of small darkrooms on the campus and a hard-working but poorly equipped, understaffed, and inadequately housed central photographic laboratory. Various departments, dissatisfied with the services, build more darkrooms, which add to the confusion. Often the more vocal groups on the campus utilize a large share of the photographic department's time; since their photographic budgets are larger, an organization pursuant to these needs is built up. Thus, a vicious circle is established in which the scientist is driven more and more to meet his own needs, to the detriment of real service, economy, and efficiency.



Availability of photographic services in 48 educational institutions.

* All photographs from the Annual International Photography-in-Science Salons, sponsored by THE SCIENTIFIC MONTHLY and the Smithsonian Institution.

Our survey was designed to determine what situations were confronting the scientist that limited his use of photography in his research problems. Seventy-five questionnaires were sent out to scientists in the biological, medical, and veterinary fields. Fifty-three responded, representing 48 institutions in 28 states, Hawaii, and 2 provinces of Canada.

The survey was not intended to give exact statistical answers; owing to the complexity of the problem and the individuality of the scientists and laboratories represented, this would be difficult. It is believed, however, that a general picture of methods used by scientists for photographic services has been obtained.

How often are photographs or motion pictures made for scientific purposes and at what intervals? For what purposes are the pictures utilized? Are professional services available and at reasonable prices? What type of photographic organization serves the scientific laboratory? With what equipment are scientists doing their own photographic work? How much money was available for photographic budgets? How well are scientists satisfied with present services? What type of organization do they consider best for their needs? These and other questions were covered by the survey.

Seven laboratories reported the availability of special techniques or equipment for photography as follows: Phase contrast microscope, 1; electron microscope, 2; radioactive tracer photography, 1; motion photomicrography, 3.

Fewer than two thirds of the institutions reporting made available to scientists even the most basic photographic assistance, such as film processing, print finishing, or a photographer in a studio. Fewer than half furnished photomicrography, motion-picture photography, or projection-slide facilities. Only a third provided any technical assistance.

From the results of the survey it is clear that the demand for photographic services by scientists is

great, and that these demands are not now being adequately met.

FREQUENCY OF DEMAND FOR PHOTOGRAPHIC SERVICES

A summarization of the replies showing the frequency of the demand for photographic services by scientists is as follows: a few each year, 10 percent; at various intervals, 39 percent; every week, 25 percent; daily, 25 percent; no answer, 1 percent. All the services that were in demand are shown in the diagram The production of 2″×2″ slides, technical assistance, photomicrographs to be taken on location, 3¼″×4″ slide production, copy work, and motion-picture photography, in that order, were considered most desirable among the unavailable services.

Many laboratory directors who believe that photographs are not within their means always have a few taken each year for the annual report or to illustrate papers. This is possibly reflected in the response of those using pictures at irregular intervals and a few each year. The high esteem placed upon the photograph as objective evidence is shown by the number indicating such services were desirable but unavailable.

Although rigid conclusions are difficult to draw, certain trends may be noted It is obvious that the great majority of those surveyed appreciated the value of photographs. Approximately 50 percent took photographs each week, and of this group 50 percent took some pictures every day.

Many scientists were forced either by inclination or in desperation to do their own photographic work. The scientist in his own laboratory, without the help of a professional photographer, may do all the work (29 percent); more than half (21 percent); less than half (16 percent); none (33 percent).

PURPOSES FOR WHICH PHOTOGRAPHS WERE TAKEN

The purposes for which the pictures were taken revealed some interesting information: illustrations, 88 percent; visual aids, 77 percent; research records, 65 percent; research data, 52 percent. Of those surveyed, 38 percent used all four types; nearly all indicated at least two uses. Apparently most scientists try to have pictures taken by professional photographers if they are available at the institution, as shown by the following figures (percentages indicate the proportion of scientists using such services): commercial, 7 percent; departmental laboratory, 10 percent; central laboratory, 51 percent; none, 32 percent.

As might be expected, the traditional use of photographs as illustrations in books and other publications ranked highest, 88 percent using them for this purpose. About 77 percent used photographs for visual aids in teaching. Of the services most desired and now available, "copy work" ranked highest; of the services most desired, but unavailable, production of 2″×2″ slides ranked first. Certainly, institutions that do not offer these services need to reconsider the value of their present photographic organizations.

Although the number using pictorial methods for records and to obtain data was not as great, these figures indicated a strong interest. In this area lies much of the opportunity for future growth and development. Photography holds the answer to problems of greater accuracy, timesaving, and undisclosed truth from data obtained by high-speed photography, time-lapse motion-picture studies, and similar methods that remain at present largely untried. As yet, satisfactory methods for the routine use of such photography are somewhat undeveloped.

Cattle heel fly (*Hypoderma lineatum*) depositing eggs. (Banner Bill Morgan and Deam Hunter Ferris.)

In an effort to determine the value of photomicrography to implement and even supplement conventional drawings, trials were conducted in our laboratory during the summer of 1947. We were engaged in making autopsies of various animals in order to collect parasites for ecological and incidence studies. Descriptions and detailed morphology are most important in studies of this nature, for purposes of identification and because of

To study mechanism of detergent entering solution, a tergitol droplet was allowed to fall into a transparent cell of water in a slide projector. (D. C. Whitmarsh, Ordnance Research Laboratory, Pennsylvania State College.)

the ever-present possibility of new species being encountered. Since it is desirable to make some observations and measurements on living specimens, this created a real time problem. It was not always possible to delay "posting" the animals long enough to make detailed drawings. An Argus C-3 and a Leica camera, each equipped with copy attachments and extension tubes, were set up on an improvised copy stand in such a manner that they could be dropped over the microscope and a photograph taken in a matter of seconds. Considering only its minimum value as a method of obtaining accurate measurements, this was found to be far more rapid than making sketches and measure-

ments. It was more rapid than even the camera lucida. An ocular micrometer used to superimpose a scale upon the figure made a permanent record In addition, it was found that some detail, especially in flattened specimens, was quite satisfactory; however, the inherent problems of depth of field make it improbable that photographs will ever supplant drawings completely. Clarity and freedom from distracting detail make drawings essential.

There is the possibility that the phase contrast microscope will help in the recording of data. Living, unstained tissues are revealed with all the detail of stained specimens where the material is suitable and appropriate instrument and technique are combined. With this microscope it is unnecessary to "stop down" the condenser diaphragm to obtain greater detail, making possible use of the objective at its greatest resolution. With proper photomicrographic equipment it will be possible to photograph and study living material at the same time. The inclusion of a Speedlamp in the illuminating system would easily allow photographs to be taken of the most rapidly moving specimen.

Such methods, where they would fit the research, are now partly available at reasonable cost. The inexpensiveness of 35-mm film should offer opportunities for data and record work. Even in these times of high prices, bulk 35-mm film was obtained at one-half cent per foot for "movie ends" (usually very satisfactory) or five to eight cents a foot for the very best quality bulk film sold exclusively for miniature use. Equipment is now available to convert 35-mm cameras to such uses as photomicrography, copying, and microfilming.

In the field of visual aids, the 2″ × 2″, 35-mm slide, and strip film are rapidly replacing the larger 3¼″ × 4″ slide. Especially for color slides, this

A commercial grade of tri-sodium orthophosphate. (T. G. Rochow and E. J. Thomas, American Cyanamid Co.)

results in economy and space saving. It should be mentioned that the larger film sizes yield better detail and are much superior for illustrative purposes.

Nearly 40 percent of the scientists surveyed used photographs for all four purposes (records, illustrations, data, and visual aids). This showed that a large proportion of scientists are alert to the possibilities of photography and that we should expect increased use in the future.

USE OF PROFESSIONAL SERVICES

That only 7 percent of the responding scientists used the services of commercial photographers indicated how far the average professional photographer is from the field. Scientific photography not only requires special photographic knowledge, but also presupposes some scientific background; often a knowledge of microscopy and related techniques is essential. From the standpoint of the commercial photographer the field is restricted. In large centers the specialist in medicine will no doubt have an excellent though rather limited opportunity.

Only 10 percent made use of or had available departmental photographic laboratories. From the standpoint of the department as a unit this is often a very satisfactory method of organization. In the case of a large department, such as a hospital, medical school, or research unit with a volume of photographic work, it is a sound system from the standpoint of efficiency and economy. In some cases the type of research work done makes this imperative.

The fact that more than 50 percent had central laboratories available would indicate this is the preferred method of organization. Although nearly all had photographic equipment (76 percent), 32 percent did no photographic work of their own. About 84 percent of this group had a central laboratory available. Many took color photographs and simply mailed the film to the processor.

About 29 percent did all their own work; only 26 percent of this group had the use of a central laboratory. It is obvious that a satisfactory central laboratory will be used. The large percentage of scientists doing most of or all their own work (53 percent) and the frequent expressions of dissatisfaction with the present system (but accompanied by a desire for professional photographic services) should give administrators and photographic directors some food for thought. Scientists want good photographic services, but our survey indicated that as a rule these services are not provided.

BUDGETARY ALLOWANCES FOR PHOTOGRAPHY

Nearly all replies indicated the need for larger photographic budgets. Photography is often regarded as a luxury. Many people think of it largely in connection with vacation trips or a Sunday afternoon excursion with the Baby Brownie. Most of the scientists used photographs, yet only 57 percent had photographic materials furnished in an unqualified way as a part of departmental supplies. Many scientists purchased their own supplies, yet only 21 percent were able to obtain supplies at a discount—which is certainly the least that could be expected!

Amounts available to scientists for photographic work were somewhat discouraging. Only a few more than half revealed any information regarding their annual budget, some giving reasons such as bookkeeping problems, or that such information was regarded as confidential. Twenty-three replies indicated a distribution of from $50 to $1,000, which was perfectly reasonable and surprisingly uniform. The next two jumped to $5,000 and $6,000. Two laboratories in commercial pharmaceutical houses spent as much for photography as one entire school. It would be noted that university budgets include many nonscientific uses for photographs. Surely the disparity here is glaringly apparent.

NEED FOR IMPROVEMENT OF PHOTOGRAPHIC LABORATORIES

A large majority of the replies showed dissatisfaction with the present photographic organization. A central laboratory, under the direction of an alert, competent director, and well equipped to do scientific work, is the most desirable. The mere existence of a central laboratory, however, is no assurance that scientists will receive adequate service; in fact, a large number of complaints were received from many who have access to such facilities. Although about 29 percent of the scientists did all their own work, even those who had a central laboratory, this was not entirely from choice, as shown by the number who checked the services they desired, and from statements of dissatisfaction. A few of these were: "Service slow but adequate;" "Central lab too busy;" "Not adequate because not immediately available; available on interdepartmental order [in other words, too much red tape];" "Service almost useless;" "Photographer, although good, is rushed and no train-

ing in scientific work;" "Professional photographers are interested in the artistry of photography and do not have a scientific or illustrative viewpoint." (This criticism has been heard repeatedly.)

Sound, color, time-lapse study, or high-speed motion pictures will be beyond the reach of the small laboratory. With a strong central organization, such projects could well be provided for most scientists because of the increased personnel strength and more efficient use of funds, equipment, and staff. With a large organization the time of its members can be utilized to better advantage and opportunity afforded men to specialize, permitting a much higher standard of services. Labor-saving equipment and competent assistants should free the photographer from routine duties and work requiring little skill. The smaller laboratories will find the use of mechanical washers, print dryers, and other labor-saving equipment profitable. A good central organization should have equipment to lend and rent. Portable photomicrographic equipment, especially, should be available.

Photomicrographs on location are much in demand and should be provided for. Preparations are more easily made in the scientist's own laboratory where he has all the accessories required at hand. Scientists experience difficulty in using equipment at the studio. It is often difficult to find fields on the large photomicrographic cameras of the horizontal type. Properly set up and equipped, the scientific photographic unit should provide equipment where the scientist can sit down and find the microscopic fields as comfortably as in his own laboratory. Often the scientist will be unable to use the type of illumination necessary for photomicrographs.

The photographic department of a large institution should be given some opportunity for research. This could possibly be carried out in conjunction with the physics, chemistry, and other departments, as well as the larger photographic companies, who have excellent research departments but who may find it more profitable to concentrate on problems other than those of the scientist. It is hoped that in the future scientists will use photography more and more in their research as they become aware of the ways in which photography can improve their work.

MOUNTAINS

A sphere in space
At sun-up's stretching time
In satisfaction belched
And squeezed some splinters
From its skin,
Then went about its business.

Thus came the orphaned ones—
To face the floggings of the wind,
The water's acid burns,
The searing etching of the ice,
The senseless scrubbing of the sands,
The torments of the damned.
Yet, they stand,
As when they first were fire-forged,
And mark the graves
Of pampered and of weaker ones.

JOSEPH HIRSH

THIRD ANNUAL INTERNATIONAL PHOTOGRAPHY-IN-SCIENCE SALON*

Prize winners in the Third International Photography-in-Science Salon, an annual competition for scientists and photographers, sponsored by THE SCIENTIFIC MONTHLY and the Smithsonian Institution, were announced on September 25.

Judges were Merle A. Tuve, of the Carnegie Institution, for the physical sciences; Walter F. Jeffers, of the Department of Botany, University of Maryland, for the biological sciences; A. A. Teeter, of Charles Pfizer & Co., New York City, for chemistry; Emanuel Krimsky, of Polyclinic Hospital, New York City, for the medical sciences; and Alexander J. Wedderburn, of the Graphic Arts Division, Smithsonian Institution, for photography.

The 200 prints were on exhibition at the U. S. National Museum, October 3–31, and will be shown at the Annual Meeting of the AAAS, New York City, December 26–31. Afterwards the show will go on tour of important museums and scientific institutions in this country and abroad.

Prize winners in the Black-and-White Division:

First: L. L. MARTON, Chief, Electron Physics Section, National Bureau of Standards, Washington, D. C.: "Electron-optical shadow method."

Second: BERNARD HENRY MOLLBERG, University of Houston, Houston, Texas: "Ventrosinistral view of dried chick embryo, plated with aluminum."

Third: S. B. NEWMAN, EMIL BORYSKO, and MAX SWERDLOW, National Bureau of Standards, Washington, D. C.: "Electron micrograph of thin section of cells in onion root tip."

Honorable Mention: JOSÉ OITICICA, JR., Rio de Janeiro, Brazil (Guggenheim Fellow at the U. S. National Museum, Washington, D. C.): "Male genitalia, ventral view, of *Citheronia mogya* Schaus 1920, (Lepidoptera, Citheroniinae).

CLYDE T. HOLLIDAY, Applied Physics Laboratory, The Johns Hopkins University, Silver Spring, Maryland: "Cloud formations." (Photograph made from a V-2 rocket at White Sands.)

T. G. ROCHOW, American Cyanamid Company, Stamford, Connecticut: "Commerical sample, tri-sodium orthophosphate ('TSP')."

CHARLES J. SALAT, Armour Research Foundation, Chicago, Illinois: "Calibration of a ball bearing by means of optical flats."

CLEE O. WORDEN, Laboratory of C. A. Zapffe, Baltimore, Maryland: "Fractograph of piezoelectric single crystal."

In the Color Division, the following won awards:

First: CHARLES D. OUGHTON and EUGENE C. RICKER, Battelle Memorial Institute, Columbus, Ohio: "Xerographic developing process."

Second: CHESTER F. REATHER, Carnegie Institution, Baltimore, Maryland: "Implantation of twelve-day human ovum."

Third: THOMAS C. POULTER (third-time prize winner) and WALTER LAWTON, Stanford Research Institute, Stanford, California: "High-speed movies of colored meteorological balloons used with Poulter Seismic Method show interaction of shock waves in the third dimension."

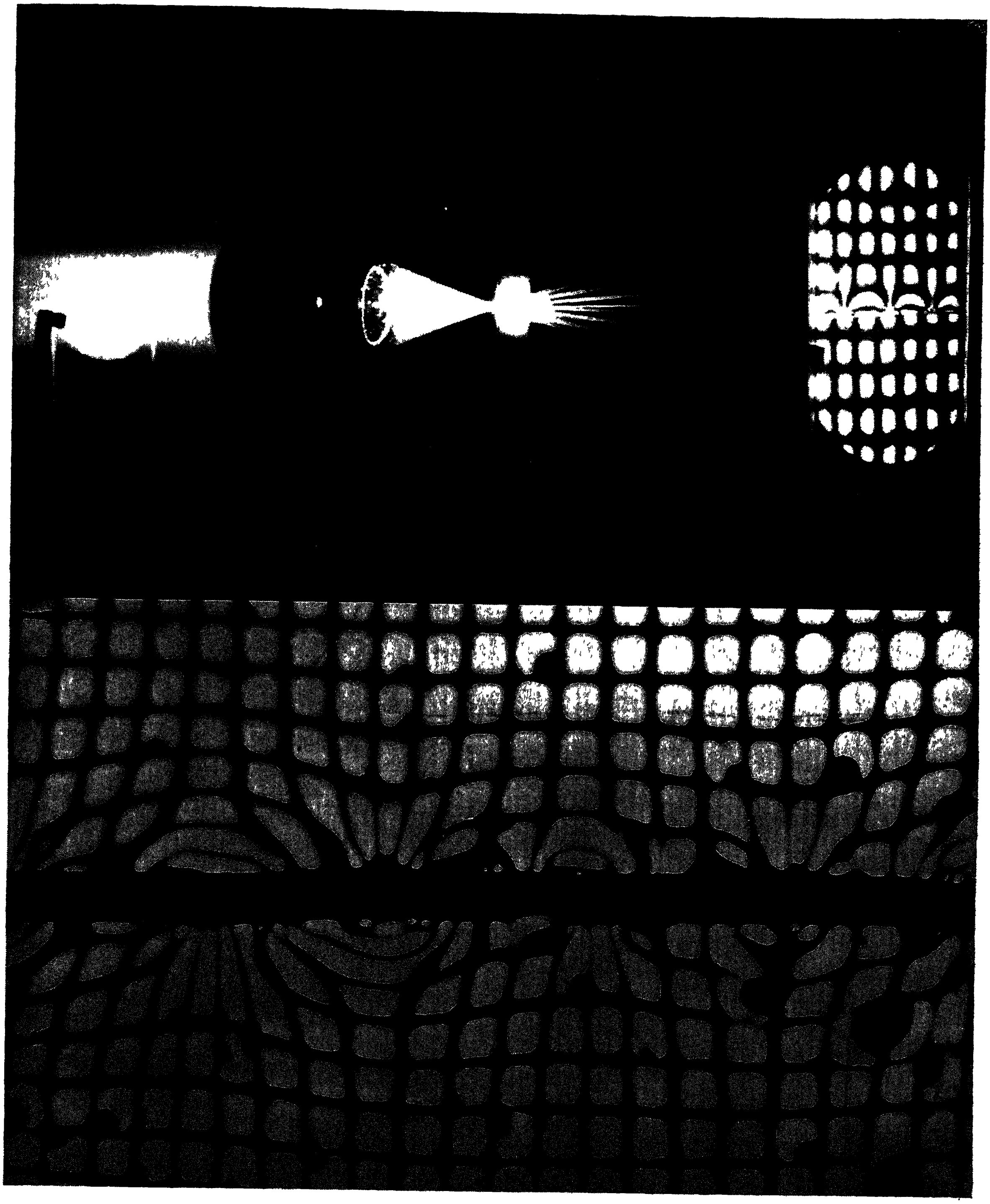
Honorable Mention: D. H. ROWLAND, Carnegie-Illinois Steel Corporation Research Laboratory, Pittsburgh, Pennsylvania: "Polished and etched cross section of experimental galvanized coating on low carbon steel."

ROWLAND B. STRADLING, U. S. Cast Iron Pipe and Foundry Co., East Burlington, New Jersey: "Photomicrograph at 200 diameters of titanium nitride in cast iron."

ALBERT C. WALKER, Bell Telephone Laboratories, Murray Hill, New Jersey: "Quartz crystal grown at Bell Telephone Laboratories."

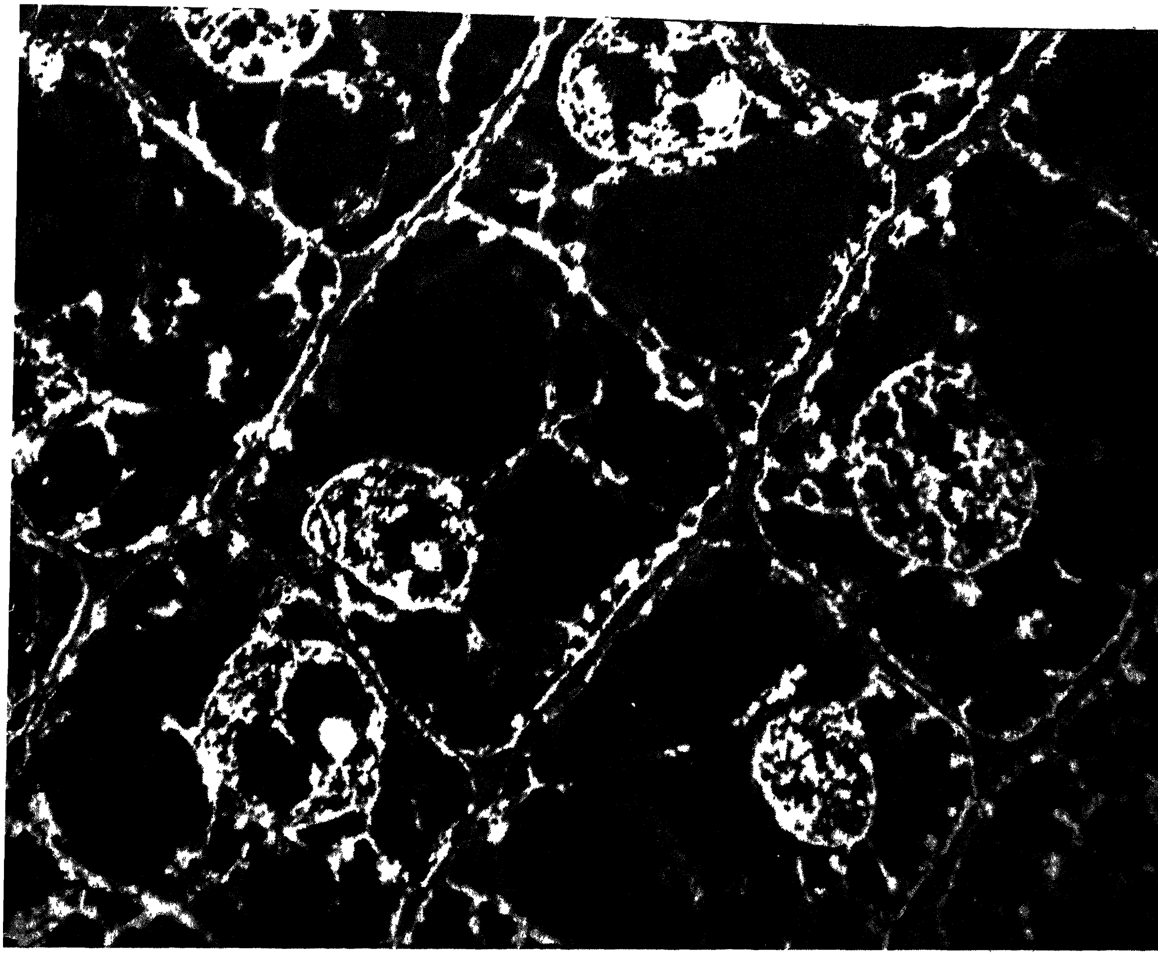
Established to encourage and extend the use of photography as a basic research tool, the contest will be continued in 1950. All entrants shall be actively engaged in scientific research, and all photographs must be taken for scientific purposes. Entries may be sent to the Editor, The Scientific Monthly, 1515 Massachusetts Ave., N. W., Washington 5, D. C., November 1–27, 1950. Prize-winning and other accepted entries will be shown at the Annual Meeting of the AAAS in Cleveland, Ohio, December 26–31, and at the U. S. National Museum, Washington, D. C., January 3–31, 1951.

* Prints in the 1949 Salon are booked through April 1950. Dates for showing these pictures may be arranged by writing to the Editor, THE SCIENTIFIC MONTHLY.



First Prize went to L. L. Marton, chief, Electron Physics Section, National Bureau of Standards, for this illustration of the new electron-optical shadow technique, which makes it possible to photograph and study quantitatively electrostatic and magnetic fields of extremely small dimensions. *Above:* An analogous experiment in light optics. In the electron-optical shadow method, the glass lens system is replaced by an electron lens, and the distorted plastic by a magnetic or electric field. *Below:* Photograph of a typical pattern. Superposed on image of a magnetic recording wire is the electron shadow of a fine wire mesh placed just beyond the back focus of an electron lens. From the displacement and reduced magnification of a selected part of the mesh, the absolute value of the magnetic field intensity at a corresponding point in the field can be accurately computed.

Photomicrograph showing orientation structure of an annealed 70–30 nickel-copper alloy that had been deformed at room temperature. At the National Bureau of Standards it was found that the proper etching technique will produce an optically active film, which, when photographed under polarized light, will show variations in orientation. Print entered by D. H. Woodard, metallurgist, National Bureau of Standards.



▲ Electronmicrograph of an extremely thin cross section of onion root tip in the zone of elongation, showing cell walls and nuclei. Prepared for study by a new method in ultramicrotomy recently developed at the National Bureau of Standards by S. B. Newman, Emil Borysko, and Max Swerdlow. Awarded third prize.

Grenz ray picture of a rose. Part of an investigation to study plant nutrient movements. Entry from Herbert R. Isenburger, St. John X-Ray Laboratory, Califon, N. J. ▶

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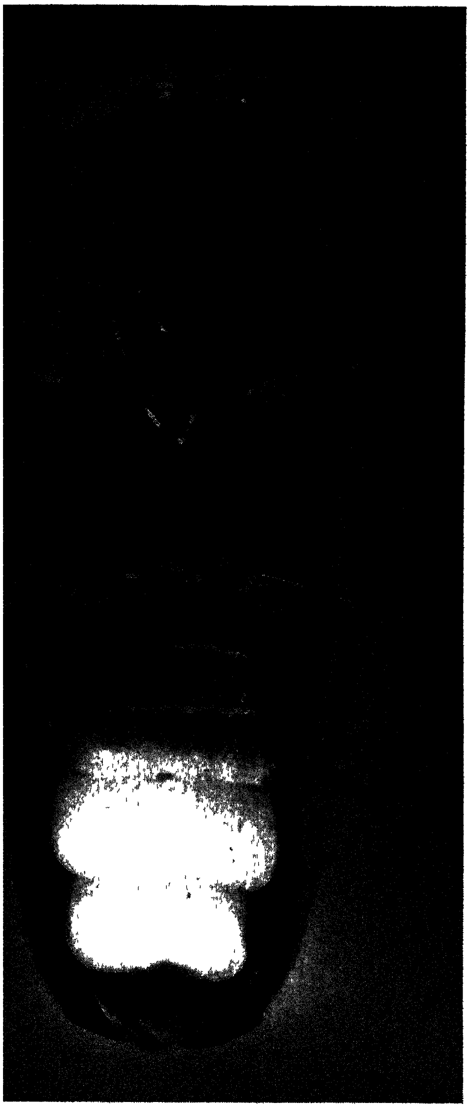
José Oiticica, Jr., of the Museu Nacional, Rio de Janeiro, temporarily at the National Museum, Washington, D. C., under a Guggenheim Fellowship, took this ventral view of the male genitalia of *Citheronia mogya* Schaus 1920 (Lepidoptera, Citheroniinae), which won Honorable Mention in the Black-and-White Division.



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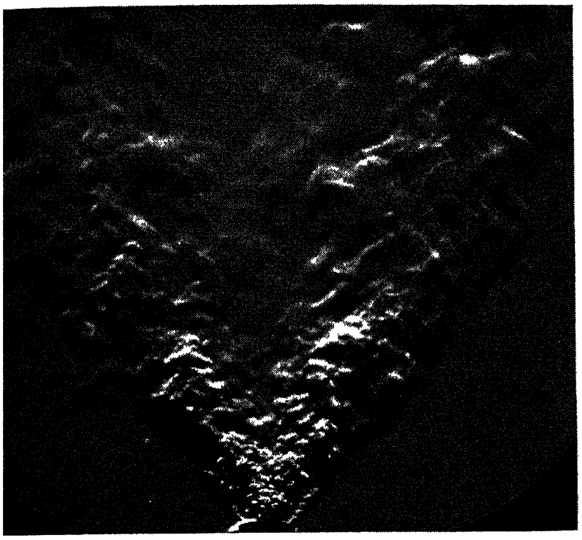
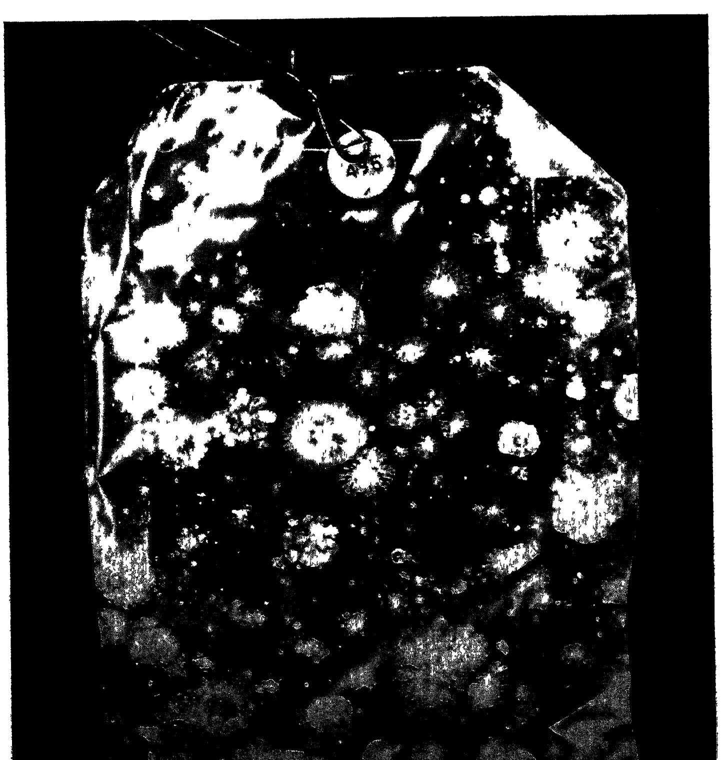
Among the interesting zoological subjects was this photograph of a firefly taken by its own flash. Submitted by A. M. Winchester, of Stetson University.

This ventrosinistral view of a dried chick embryo which has been plated with aluminum won Second Prize for Bernard Henry Mollberg, of the University of Houston. The plating process reveals details of surface anatomy that would otherwise remain obscure. ▶



▲ Schlieren photograph of helium discharging from injector of a V-2 rocket motor. Print entered by Walter R. Keagy, Jr., of Battelle Memorial Institute.

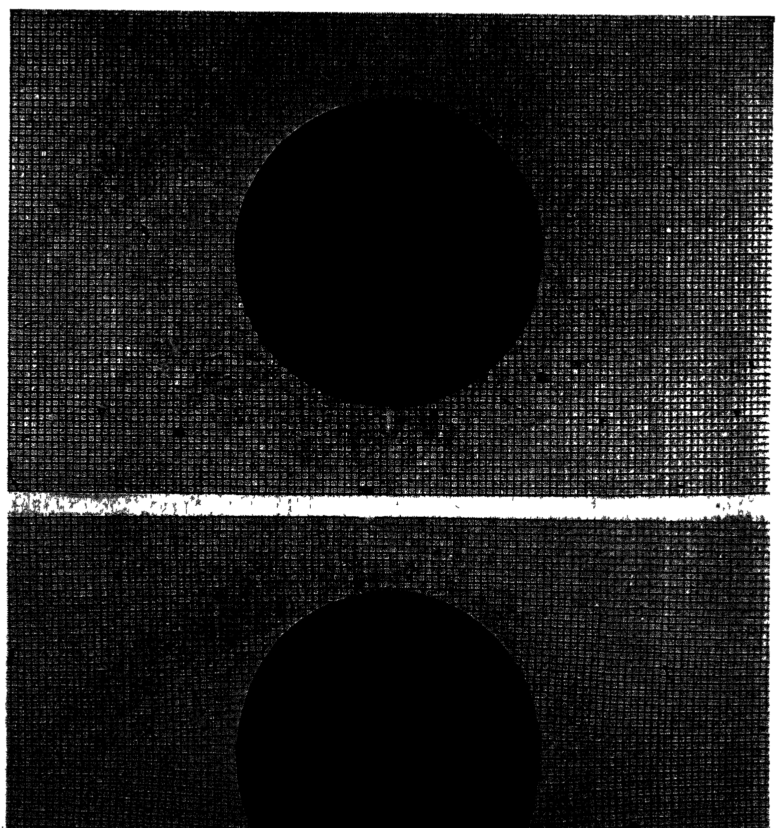
Calibration of a ball bearing by means of optical flats. Photograph provides permanent record of the calibration of a half-inch ball bearing. Honorable Mention awarded to Charles J. Salat, of Armour Research Foundation. ▶



◀ Mold growth on cellophane exposed in the tropics. Print entered by Leonard Teitell and Marie Reeder, of Frankford Arsenal's Pitman-Dunn Laboratory.

◀ Honorable mention went to Clee O. Worden, of the Laboratory of C. A. Zapffe, research metallurgist, Baltimore, for this fractograph of a piezoelectric single crystal. "Fractography" is a photographic technique that surmounts the long-standing difficulties of observing jagged surfaces at high magnification.

By means of a new photo-grid technique ▶ developed at the National Bureau of Standards, plastic deformations in the vicinity of holes may be conveniently investigated. Method involves use of a master grid ruled with lines 0.015 mm wide and nominally spaced 0.25 mm apart. Submitted by James A. Miller, metallurgist, Chester I. Pope, chemist, and Benjamin L. Page, physicist.



SAMOA—SHELL-COLLECTOR'S PARADISE

R. TUCKER ABBOTT

Mr. Abbott was malacologist on the Harvard-Archbold Expedition to Melanesia in 1940–41, collecting mollusks in Fiji for three months aboard the junk-yacht Cheng-Ho. *In World War II he saw service as a Navy dive-bomber pilot and later as malacologist for Navy Medical Research Unit No. 2, based on Guam He has done research on disease-carrying snails in China and the Philippines, and since 1946 has been assistant curator of mollusks at the U. S. National Museum, Washington, D. C.*

AMONG the many Polynesian collecting grounds for mollusks, Tutuila Island, Samoa, stands foremost as a rich and diversified area for ecological studies. The coastal barrier reefs present ideal collecting conditions, but it is no less rewarding to search for sea shells along the rocky shores, on the sandy beaches and mud flats, and in the mangrove swamps. The beach collector soon finds that certain groups of marine animals inhabit special areas that suit the needs or desires of the individuals. Fagaitua Bay, on the southeast coast of Tutuila, is as varied in its types of habitats as New York City is in its wards. From the shallow mud flats to the rock-bound headlands, from the semistagnant waters of the mangroves to the turbulent surf on the reefs, every kind of footing and resting place is available for the hundreds of species of marine shells found in this region.

Frequent visits to Fagaitua Bay enabled me to observe the quite obvious differences between the way of life on the sand flats and the rocky shores, and also to see a constant change in each one of the littoral environments. In fact, I found it impossible, for instance, to make a detailed census of the shell population on the sand flat just inside the reef barrier, for on each visit I found the sand bar had been moved by the tides and currents. One section of the reef bordering the lagoon would be covered by sand one day and bare the next. Some changes in the bay were slight and occurred at regular intervals, such as the tidal flooding and draining of the mud flats; other changes were more violent but less frequent, as for instance when the elements played rough during stormy moods. After a heavy gale the deep, soft sand of a beach would be replaced by a mass of coral rubble, and sometimes sections of the inner coral reef would be half covered or isolated by a miniature Sahara.

In a week or so of more normal weather—gentle northeast breezes and a calm sea—the lagoon would slowly change back to its original condition. Inch by inch the sands would move back to the beach, and eventually the reef would recover its old boundaries. All these changes the mollusks of the bay had to endure. A few succumbed in exceptionally long periods of change, as was evident from the dead and empty shells cast up on the beach. In the main, however, the populations survived the ordeals which, to us, would have been equivalent to violent earthquakes, devastating floods, and dust-bowl storms.

It was not until my fourth or fifth visit to Fagaitua Bay that I became acquainted with a molluscan sand lover. Directly in front of Alofao Village is a square half mile of sandy flats. For the Tom Thumb conch shell, *Strombus gibberulus* Linné, these flats are paradise. My West Indian collecting had long made me class the conch shells among the Gargantuas of the shell world. The pink-and-ivory flushed queen conch of Florida and the Antilles, *S. gigas* Linné, grows in most cases to a foot in length. Here on the Fagaitua flats the adults of this diminutive conch were no more than an inch and a half in length. I was as much surprised as if I had found an adult elephant the size of a dog.

The first few times I searched the flats, I had seen neither the Tom Thumb conchs nor any trails in the sand that might have indicated their presence. Then one afternoon at low tide, as I walked out from the beach, I discovered them by the thousands. For the next week I raided the colony for specimens, without seemingly reducing their numbers, until one day I came down to the flats to discover every single conch had disappeared. Twice again, during my occasional visits for the next three months, the Tom Thumbs appeared for a few days to enjoy what apparently was a lengthy period of mating. Each time they moved off as mysteriously as they had come. I was not long

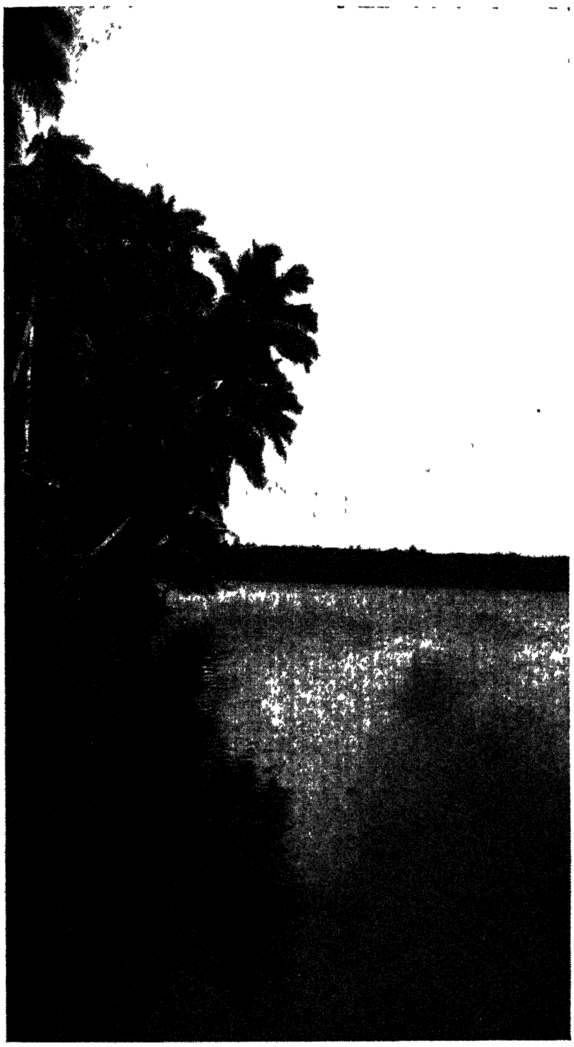
enough in this vicinity to find out whether this appearance was a quarterly or even monthly occurrence. Nor was I able to ascertain positively that the conchs had left for deeper waters. Perhaps they had retreated to a less active life among the corals of the reef or had dug themselves into the sand at the bottom of the lagoon.

My tiny conch animal may have been of Lilliputian proportions, but he certainly made up for it in strength and agility. The chitinous trap door, or operculum, has been modified into a long, sickle-shaped weapon. Most mollusks use this operculum as a door to slam shut in the face of the enemy, but Tom Thumb uses it as cutlass and vaulting pole. The small, saw-toothed operculum is attached to the end of the powerful foot, where it can be brandished effectively against attacking crabs. The foot muscles are powerful enough to force open a boy's fingers when the shell is held in his clenched fist. A rock ten times a conch's own weight can be pushed aside with ease.

My respect for Tom Thumb increased even more when I saw him try out for the pole-vaulting record. Apparently bored at times, just inching along on the sand flat, the conch whips up his operculum, jabs it into the sand, and suddenly hurtles himself a full two inches off the ground. If a one-legged man with a crutch could jump twelve feet into the air, it would be no less amazing.

The eyes of Tom Thumb are something marvelous to behold! In a glass jar of sea water, the tiny conch loses all shyness, and soon comes out of his shell. At first, two round orbs peek out between the half-extended foot and the shell—two shiny agates marked with black and orange circles. Having those eyes peering directly at you tempts you to believe that high intelligence is in the brain they serve. Presently the bulbous eyes slide for-

The giant and the dwarfs. A study in contrast between the diminutive conchs, *Strombus gibberulus* Linné, and the massive helmet shell, *Cassis tuberosa* Linné.



A Samoan bay, where clear waters and a calm sea make ideal collecting conditions.

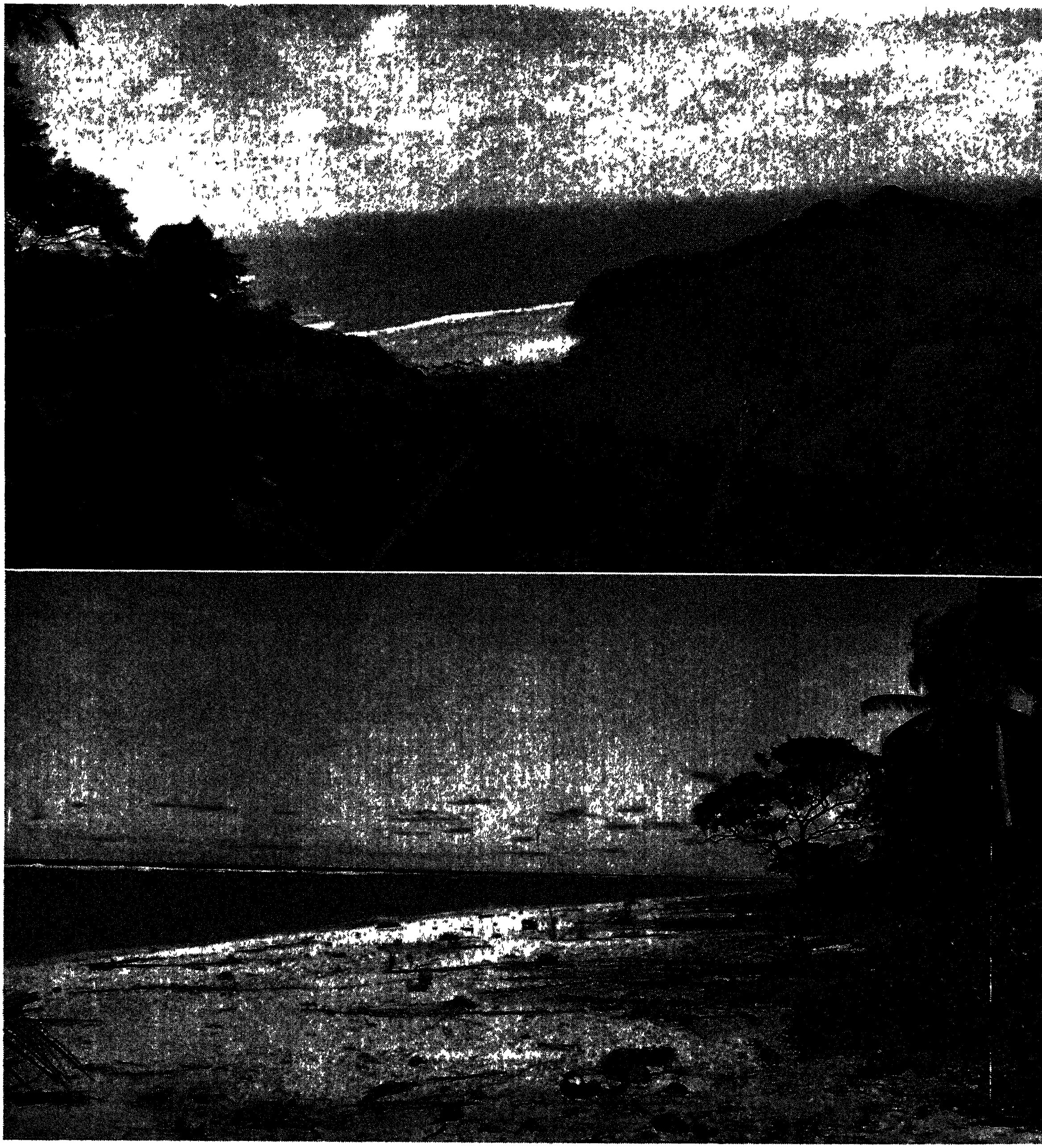
ward, away from the rest of the head, on long gray tubes. The rubbery, hoselike stalks extend so far that the comical manipulation of the eyes almost seems to be accomplished by remote control. But Tom Thumb's eyes are more for show than use. They are built in the form of the old-fashioned peep-show, and probably the molluscan brain can register only changes in light intensity and dim outlines of near-by moving objects.

The flats at Alofao are ideal living grounds for other sand- and mud-loving mollusks. The water is relatively quiet, but kept sufficiently fresh by changing tides. When the tide draws slowly back from a long, level sand bar, the sun readily warms the water. Microscopic organisms thrive, and seaweed growths become attached to every rock and nearly every shell. Here I found a splendid colony of nassa snails, *Nassarius globosus* Quoy and Gaimard. This half-inch, minutely corrugated gray shell spends its more active moments in sliding along the mud flats in hot pursuit of the falling tides, and again in returning with the rise. Nassas are not as sluggish in disposition as their muddy environment might indicate. They seem to be continually inching over the surface as fast as their one foot will carry them, and not one specimen could I find guilty of gathering moss. Their apparent rush through life is motivated by their appetite for dead flesh, and in their small way they help keep the beaches clean.

Eventually your eyes grow tired of the monotonous gray tone of the mud flats, and the tiny mollusks you seek fail to register in your brain. In front of Alofao Village you can desert the mud stretches and step a few yards to firm, sandy bottom. The white sand and clear lagoon* are in startling contrast. The sunny brilliance of the crystal clear water can hold you with almost bewitching fascination, and even the most indifferent collector is impressed by the possibilities in the white sand carpet. Hours of soul-satisfying swimming often become an irresistible overindulgence in collecting, ended only by the warning of oversoaked, wrinkled hands.

Animal of the small conch, *Strombus canarium* Linné, showing the eye (*e*), foot (*f*), mouth (*m*), and operculum (*o*). (From *Voyage de La Bonite.*)

Bathing trunks, water goggles or a face mask, and a collecting bag tucked under the belt are the open sesame to a new kingdom of profitable hunt-



Above: A tiny bay set in a matrix of Samoan hills and Pacific seas. *Below*: A salty breeze and shallow reef waters invite the shell collector.

ing. William Beebe has long extolled the advantages of a diving helmet, and for purposes of strolling through the coral gardens under thirty feet of water, this type of diving is ideal. But in a shallow lagoon, in most places no deeper than four or five feet, helmet diving, of course, is impractical. Even in deeper waters, helmet diving is not suitable for collecting sea shells. Those helmet divers who have attempted to leap in sprightly fashion over a jagged coral head know from experience the futility of rapid motion. If they lean over to pick up an object, the water splashes up inside the helmet to blot out all vision. But for lagoon collecting goggles allow almost unlimited freedom

of action. You can make your way about in fish fashion, exploring any part of the bay in three-dimensional travel. It is true that each submergence is limited by the diver's lung capacity, but it is surprising in how few days of continuous diving one's underwater endurance can reach the two- or even three-minute mark. The experienced goggler spends most of his time cruising by means of breast strokes over the surface, remaining buoyed up by a large lungful of air. Using leisurely strokes, the swimmer may continue his cruising tactics for hours without tiring, lifting his head out of water occasionally to gulp in fresh air. The lungs may be used like the ballast tanks of a submarine. Emptying the lungs of air, the swimmer can drop to the bottom like a rock. A quarter lungful will permit cruising just over the sandy bottom. When an interesting shell is discovered, a complete emptying of the lungs will settle the observer down on his elbows on the sand.

Just as woodsmen make use of their knowledge of animal trails over the snow, marine collectors can reconstruct the drama of life on the lagoon floor. I remember cruising one morning along the surface in search of shells when I came across a neatly made track—a miniature and exact copy of a tractor trail. The tracks of sea shells in the sand are usually short, no more than six to ten feet in length, but this one led off into the hazy distance across the lagoon desert. As I followed along its course, I noticed another, similar trail converging toward the first one. Then still another appeared. All were apparently headed for a rendezvous at the other end of the lagoon. Finally I reached the mecca, and found the ruins of what had once been a large, long-spined sea urchin, *Diadema*. Little of the body was left, only a scattered patch of broken, purple, needlelike spines. Between the spiny rubble and a near-by section of the coral reef lay a wide, hazy trail, indicating that the urchin had wandered away from the protecting reef sometime during the night. A few broken spine tips at the base of the coral wall were evidence that the urchin had been dislodged by wave action or by some unknown creature, and had been injured in its fall to the lagoon floor. There were signs of a tussle near the ruins, where possibly a large fish had attacked the helpless urchin.

But the mystery of the origin of the tiny tractor trails was still not solved. I saw that the trail-makers had been enticed to join the carnage and, having had their fill, had moved off again. At last I tracked down the agents, discovering at the end of each trail a large hermit crab scurrying along over the sand with an old, weather-beaten shell on its back. By far the longest tracks are left by hermit crabs, which seem to be engaged continually in important missions at the opposite end of the lagoon. The great length of their trails is due primarily to the speed at which they scamper across the bottom I say "speed" because they travel ten times as fast as the average gastropod, and to walk around the world without rest the crab would take only forty years, the snail perhaps four hundred.

I also saw near the urchin ruins a short wide trail not uncommonly found on the lagoon bottom. At the end of it, I found an adult moon shell, *Polinices mamilla* Linné, whose glossy white exterior is like the most perfect of well-finished chinaware. The foot and head of the animal, when its tiny internal ducts are filled with water, are quite easily twice the size of the shell. The creature is equipped with an additional propodium, or forefoot, which it uses to shovel and push the sand up on top of its shell, Quite often the moon shell remains dormant for a day or two at a time, but a small mound of sand will always reveal its presence.

No mollusk trail is indelible on this sandy slate of the seashore, but the gastropod with the widest foot, or largest shell, will always make the most lasting impression. On a normally calm day, a well-impressed track will not be obliterated by drifting sand within twenty hours, but during squally weather, when the lagoon's face is clouded with suspended particles of sand, such a track will be covered in one or two hours.

One of the prettiest and daintiest shells of the South Seas is the olive-shaped mitre, *Imbricaria olivaeformis* Swainson. The shell is a little larger than a bean, and is brightly hued with an enamel coat of golden yellow. Its smooth rounded tip, or spire, is lacquered over with a splash of deep royal purple. I am tempted to dub this little mitre the "Who-done-it?" shell, for its short, yard-long trail is nearly always in the form of a question mark. The hook of the question mark is fine and distinct, as if made by the tip of a very sharp pencil. In the spot where the mitre shell is busily inching along, a tiny hump of sand sticks up to form the dot of the question mark. Many shells have a characteristically curved or meandering trail, but why "Who-done-it?" should start to make a circle and then straighten out its course to complete the form of a question mark is one of nature's riddles.

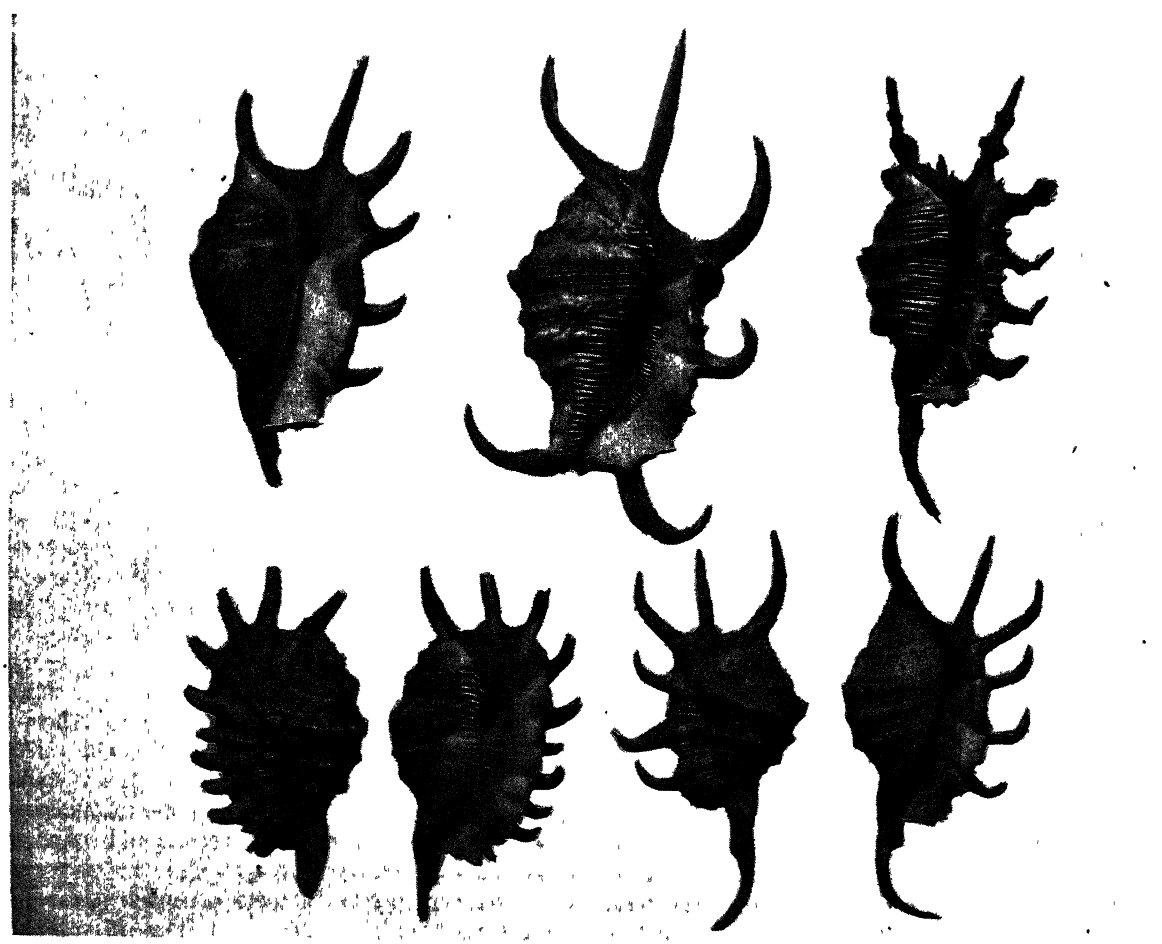
The greatest thrill I had while hunting mollusks in the lagoon area of the bay was in plucking huge

marlinspike shells, *Terebra maculata* Linné, out of the sand. It was much like pulling deep-rooted carrots out of the garden soil. The marlinspike is four to eight inches in length and shaped very much like a long, drawn-out cornucopia. The smooth, heavily enameled whorls are colored a creamy white, with a series of red-brown splotches spiraled around the entire shell. The animal digs down deep into the sand, so that only an inch or so of the spire projects above the surface.

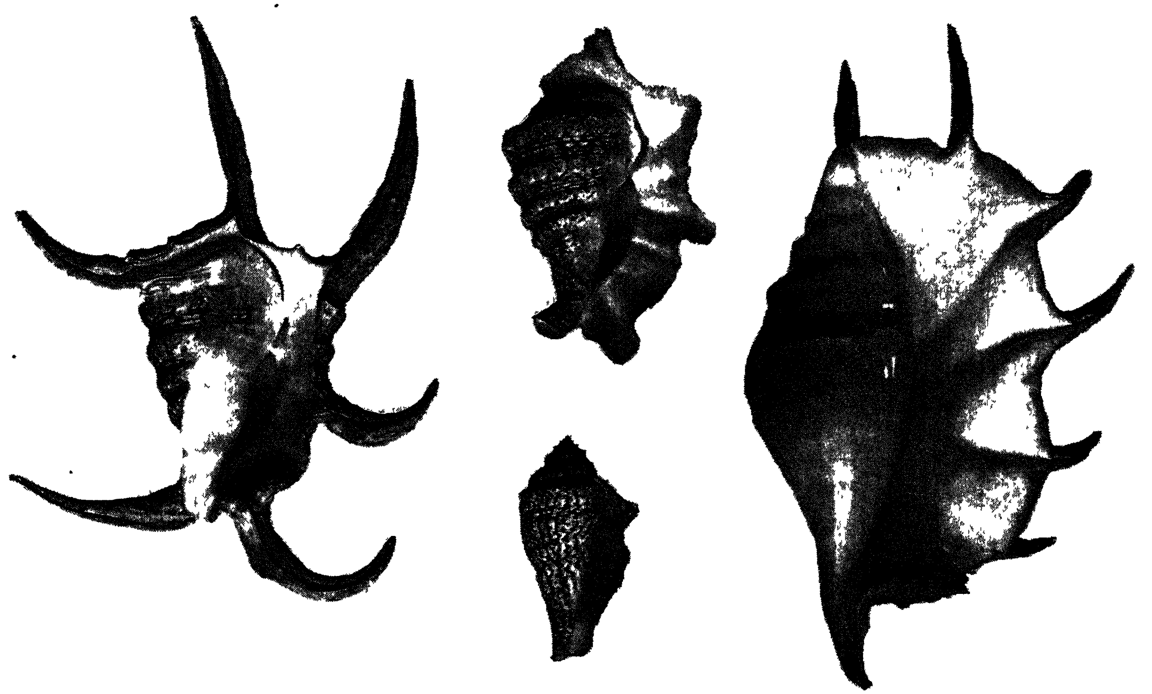
After weeks of searching for mollusks in mud and sand, on rock shelves, under coral heads, on mangrove stumps, even in fish stomachs, I thought that every collecting possibility had been exhausted. Then one day, rummaging in the water near the mangroves, I picked up a white, short-spined sea urchin (*Tripneustes*), to inspect the delicate system of tubular feet on its underside. Nestled down between the tubular feet and very short spines was a pair of thin, nut-brown clams about the size of the nail on my little finger. I checked with the other sea urchins and confirmed my suspicion that the small fingernail clams were not accidentally adhering to the urchin but were in fact symbiotic clams found nowhere else in the bay nor on any other creature.

A billion years ago, perhaps, the survival rate for such tiny clams was greatest in the immediate vicinity of the white sea urchin where vegetable debris was richest. Teleologically speaking, we might say that some bright fellow, an evolutionary-minded bivalve, eventually decided not to wait for the creeping sea urchin to come, but instead clambered aboard the lumbering, seaweed grist mill. Today, a fingernail clam is in danger of starvation if once detached from its host. Such is the price of luxurious specialization.

The five-armed starfish, *Ophidiaster,* in the mud



Five species of Indo-Pacific spider conchs, *Lambis*. *Top, left to right*: *Lambis lambis* Linné, *L. arthritica* Röding, and *L. scorpio* Linné. *Bottom,* first pair: *L. millepeda* Linné; second pair, *L. crocata* Link.



The two largest species of spider conchs, *Lambis. Left*, and two young specimens, *center: Lambis chiragra* Linné; *far right*: Bryon's spider conch, *Lambis truncata* Humphrey.

regions, also turned out to be an unexpected source for a rare parasitic gastropod. On the bottom sides of the arms, wedged between the hundreds of small tubular suckers, were to be found some white shells, each about the size of a grain of rice. Specialization has gone to extremes in the case of this tiny *Eulima,* or "suck-shell." The head and mouth of the snail have been drawn out into a single slender tube for the purpose of tapping the rich body fluids of its host. Some species of "suck-shells," such as *Stylifer,* have taken on the strange habit of burying themselves deep in the flesh of the starfish as a precaution against being rubbed off. The great holothurian, or sea-cucumber, which resembles a black, oversized cucumber, has a much more uncomfortable time with its parasite. In this case, the "suck-shell" crawls right into the sea cucumber and jabs its proboscis into the intestinal wall. The highly nutritional waterworks are tapped for food, and enough sea water passes through the holothurian to give adequate aeration to the snail.

Mollusks do not always act as the villainous parasite in these strange associations. Sometimes the snails themselves act the gracious host, as in the commensal association between the large queen conch, *Strombus gigas* Linné, and the small conch fish of the West Indies. The first conch fish to take refuge within the folds of the conch shell was indeed a desperate or a very brave animal. When the conch is extended, there are no more than three or four cupfuls of sea water between the conch's foot and mantle. When the animal withdraws into its shell, this volume is reduced to about a single cupful. In time of trouble, the inch-long fish flits into the protective folds of its molluscan host. Passing sharks or barracuda rarely give the hulking shell a second glance, much less attempt to crack it open. Sealed in the dark chamber of flesh, the fish must wait until the conch sees fit to protrude its massive foot once more and liberate its guest.

A few days after my arrival in Samoa, I heard of Frank, a native renowned for his diving ability, who had been with Roy Waldo Miner on the *Zaca* expedition in the Polynesian Islands. When word reached Frank that I planned to go diving in the deep channels of Fagaitua, he immediately recessed from his construction job with the Navy at Pago Pago, and returned to his native village of

Alofao. He loved fishing and diving almost as much as he did his wife and children; and he understood the urge that impelled naturalists to spend their time collecting shells and fish.

On our first day of diving we chose the middle channel directly opposite Alofao Village, because it was wide, deep, and relatively quiet. Frank struck out with great powerful breast strokes until he reached a spot midway in the channel. On either side of us, a dozen yards away, were the two reef walls of the channel. It was low tide, and a great section of the reef was out of water. The current in the channel at the time was almost negligible, and the swell of the ocean was little more than a gentle heave that sighed against the dripping sides of the reef. The floor of the channel sloped very gently down from the sandy bottom of the lagoon to a depth of forty feet where we were now treading water, and thence on out to the hundred-foot depths of the outer bay.

From the distant cliff road the waters of the channel are a beautiful blend of greens, blues, and purples—delicate shades determined by the type of bottom, whether sandy or rocky, and by the depth of water. But here, bobbing on the surface with my begoggled head under water, the view was of a totally different kind of beauty, possessing something of the grandeur we find in a canyon when we stand on its rim and look far across to the other side. I felt as if I were looking down into a gigantic aquarium. Behind me, the sandy floor sloped upward into the opaque yellowish haze of the lagoon; in front, the subdued light of bluish water blended off into purple nothingness. Yet the water in the immediate vicinity was so clear I could see every detail of the bottom and of the coral sides.

The utter silence was as much a part of this strange kingdom as the softened light. All sound was blotted out beneath the surface—only the senses of sight and touch could be used in observing this new world. It was annoying at first to be so limited, for in a new and strange environment we humans like to sniff the air for odors and cock our ears for sounds. We wish to associate new experiences with familiar sounds and smells.

To Frank, all this was as familiar and commonplace as subway sights to a New York commuter. Before my untrained eye could pick out the main object of our search—a mollusk worth collecting—Frank had already started his first dive. I felt rather puny in physique and ineffectual as a swimmer as I duck-dove at the surface and attempted to follow. Within half a minute my breath was beginning to shorten and my strokes to weaken. I saw now that Frank was headed for a large shell of some sort, but my interest in reaching the bottom turned into a panic-stricken urge to get back to the surface for air. I turned around and shot for the surface, emptying my lungs on the way, and the instant I splashed through the channel ceiling, I was thankfully gulping fresh air.

In a few moments, Frank also surfaced with a splashing explosion, and triumphantly waved above his head a large specimen of the spider conch, *Lambis lambis* Linné. This was the first species of its genus, with live animal, that I had seen so far on my Pacific sojourn, although it is a fairly common shell of the conch family, both in museum collections and in its natural reef haunts. Personal introduction to an old friend of museum cabinets is a thrilling experience among all museum workers who go into the field. The most staid of fish picklers will register delight upon seeing his first dolphin or sea horse in action.

Private shell collectors strive to obtain at least one example of these handsome spider conchs for their collections, for the shell is large, brightly colored about the mouth and grotesquely ornate, with long, curved spines that resemble spider legs. There are about half a dozen species of *Lambis* in the tropical areas of the Pacific, and each kind possesses a characteristic type of spine and disstinctive color pattern. One species may bear a set of six gracefully curved and slender spines, another may carry eight thick, contorted spines. The adults of Bryon's spider conch, *Lambis truncata* Humphrey, are massive and nearly a foot in length, with the under surfaces glazed with a white enamel, whereas the orange spider conch, *Lambis crocata* Link, rarely exceeds four or five inches in size and is brilliantly painted underneath with a cream orange. Yet all spider conchs in their young stages look very much alike and, indeed, can scarcely be distinguished from the young of their cousins, the *Strombus* conchs. It is not until they approach maturity that the shells of *Lambis* begin to lose their normal conchlike appearance, and continue to develop the lip of the shell into long, drawn-out fingers, or spines. It is a matter of conjecture whether these shelly appendages are of importance as a form of camouflage or as a protection against predatory fish. For that matter, they may have no survival value at all.

Frank and I continued our exploration of the

channel. By confining my diving activities to depths of less than twenty feet, I was able to reach the few shells that could be seen from the surface. Additional small but choice mollusks came to light by turning over dead slabs of coral rock on the bottom. It was on one of these longer dives that I chanced to spot, off to one side of the channel in a small coral alleyway, a huge helmet shell, *Cassis tuberosa* Linné. Apparently, the helmet shell had wedged itself into this tight corner on a recent foray for sea urchins, and in its slow molluscan way had not yet explored the possibilities of backing out. On the next dive, I swam directly over the trapped mollusk, using both hands to wriggle it loose.

An armful of *Cassis* such as this necessitated a trip back to the beach, where it was added to our growing pile of spoils. Frank had already come ashore several times, and I now saw him returning for the fifth time with a number of large shells tucked under the cloth around his waist. Two hours of diving in deep water is rather strenuous, and we were both glad that the incoming tide and the accompanying currents in the channel were bringing our operations to an end for the day.

One of my favorite Samoan families lived in the first large hut just beyond the beach, and invariably I headed for the cool shade of their *falé,* where I could sit at rest on the grass mats with my back to a wooden pillar. While I munched cold fish and hot, freshly baked breadfruit, Samana, the head of the family, would look over my catch and make solemn comments on the rare or common occurrence of each species. "Ah, yes, this is a *papatuka,*" he would remark, holding up the spotted cone shell, *Conus pulicarius* Hwass. "I know a place where there are as many as the leaves on a coconut tree. See, over there." By then, my strength would be back and my hunger satisfied, and, as I followed Samana's pointing finger in the direction of the open sea, I would begin to plan tomorrow's collecting trip.

Samoan scenes reproduced by courtesy of Leonard P. Schultz, Smithsonian Institution.

INSTRUMENTATION AND CYBERNETICS

JOHN D. TRIMMER

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IT IS necessary to define, not only the new domain of cybernetics, but also the older domain of instrumentation. It remains to be seen whether this need can be fully met. One might expect that the newer term, having been coined *ad hoc*, would be more clear-cut than the older one, which "just grew." This expectation is hardly borne out. In fact, in probing after an understanding of these terms and the relations between them, it becomes evident that the basic concepts needed to establish satisfactory definitions of either one have not yet been too well clarified.

The following discussion, therefore, may not always be in closed form; but it will represent an honest effort to bring out into the open some of the questions that seem to need answering. The first of these concerns the nature of instrumentation.

WHAT IS INSTRUMENTATION?

The history of twentieth-century technology shows that one of its most rapid developments has been that of measuring instruments and automatic controls. This development followed a certain natural course of evolution, beginning with emphasis on indicating instruments, which left on the human observer both the burden of remembering the information provided by the instruments and the burden of acting upon that information. Next came emphasis on recording instruments, which might be described as instruments with built-in memory. The third and final stage of this sequence gives emphasis to the automatic control, in which the instrument takes some action, presumably more or less similar to that which the human observer would take if he were acting upon the information furnished by the instrument—that is, action toward an end desired by the user of the machine. According to present terminology, the device is called a *regulator* if the action is directed toward a goal that remains fixed for relatively long times, and a *servomechanism* if the goal changes somewhat rapidly.

The early stages of the growth of instrumentation were related to the various branches of engineering: instruments such as the engine indicator came to be a subject of study in mechanical engineering; the various electrical instruments were incorporated into electrical engineering; somewhat later, the problems of aircraft instrumentation found their place in aeronautical engineering. This early diversification of indicating instruments was gradually modified by two important influences One was the increasing emphasis on automatic control; the other was a synthesis of the features common to various kinds of instruments—electrical, mechanical, etc.—into a single unified body of ideas. Although these influences, particularly the second one, have not yet had their full effect, there is today widespread agreement that instrumentation is considered to deal in general and in detail with all kinds of instruments—indicators, regulators, and servos.

Such a definition of instrumentation appears to be quite satisfactory in giving to the field of instrumentation internal coherence—that is, in stating what instrumentation should include. There remains, however, the question of external coherence—that is, to what does instrumentation belong? Of what is it a part? This question has not been answered. It deserves careful consideration.

One approach has been worked out by the author during the past several years in teaching a course on instrumentation in the Department of Physics at the University of Tennessee, and it is expounded in a forthcoming textbook written for this course ("Response of Physical Systems," to be published by John Wiley & Sons). A brief statement of this point of view will be given here. It is based on a certain pattern of experience (Fig. 1). A system, of nature unspecified, is subject to an input, or "forcing," and gives an output, or "response." A general study may be built up around this pattern, including in its domain not only physical systems, such as instruments, regulators, and servos, but also biological and sociological entities. This general study will here be called "system response." (Basic principles of system response are outlined in the first chapter of "Response of Physical Systems.")

Thus, one answer to the problem posed above is to say that instrumentation is a part of the subject of system response, for the subject matter of instrumentation fits neatly into the pattern of Figure 1. Instruments, regulators, and servos are systems having forcing and response. For instruments, the forcing is the true value of the quantity being measured and the response is the reading obtained. For regulators and servos, the forcing is the desired value of the quantity being controlled and the response is the actual value.

Any pleasant feeling of accomplishment at having arrived thus far is short-lived, however, for one is immediately led to ask: Of what is system response a part? And it is at this point that it is convenient to refer to the beginning of this paper and to ask the second principal question, concerning the nature of cybernetics.

WHAT IS CYBERNETICS?

The English word "cybernetics" was formed from the Greek word (*kybernetes*) for "steersman," and was presented to the public in a recent book by the mathematician Norbert Wiener (*Cybernetics*. New York: Wiley, 1948). The subtitle of this book, *Control and Communication in the Animal and the Machine,* suggests a simple way of displaying the domain of cybernetics (Fig. 2). The chief difficulty in attempting this display is in finding single words, or even single terms, to describe each of the four quadrants of the diagram, particularly the lower two.

Taken in a somewhat modified and generalized sense (indicated in the figure by the quotation marks), the words "thought" and "action" may be considered to refer to the totality of problems arising, respectively, in animal communication and in animal control. The word "instrumentation" in the sense defined above seems to represent admirably the machine-control quadrant. Although the term "communication engineering" appears to be a suitable synonym for machine communication, it is worth noting that Professor Wiener puts considerable emphasis on computing machines, or, more generally, "thinking" machines. One is therefore faced with the alternatives of including such machines under communication engineering or instrumentation, or else of replacing "communication" by some more general word such as "thought." One feels here a certain awkwardness which suggests that the contents of cybernetics might profitably be grouped and arranged somewhat differently. A suggested way of doing this is presented at the close of this paper.

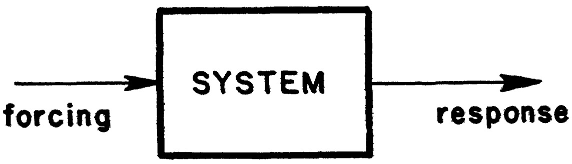

FIG. 1. A pattern of experience.

	COMMUNICATION	CONTROL
MACHINE	communication engineering	instrumentation
ANIMAL	"thought"	"action"

FIG. 2. The domain of cybernetics.

Both the derivation of the word "cybernetics" and the contents of Wiener's book put much emphasis on feedback. This corresponds closely to what has happened in the narrower field of instrumentation. In the twentieth-century evolution of instrumentation discussed above, an outstanding feature has been the increasing prominence of feedback. This has gone so far that many people accept feedback as the defining aspect of regulators and servos. With this viewpoint the author cannot agree. However convenient and widespread the use of feedback may be in the practical execution of actions directed toward desired ends, to make it the *sine qua non* of such devices seems to be a logical anomaly, comparable to defining mammals as "animals with four legs." Similarly, in the broader domain of cybernetics, the role of feedback appears to be more incidental than the initial interpretation has implied.

Referring to the question of what is instrumentation a part, it is clear that two answers have been found. On the one hand, instrumentation fits into the subject matter of system response; and, on the other hand, it is clearly a part of cybernetics. This situation must mean either that one of the two subjects, system response and cybernetics, is larger than the other and so includes the other, or else that both may be regarded as more or less distinct parts of a still larger discipline, not yet specified. Before attempting to decide between these alternatives, it may be well to look more closely at the way instrumentation actually fits into the two larger fields of system response and of cybernetics. This may be done by considering a particular problem in instrumentation.

THE PROBLEM OF INSTRUMENT EFFICIENCY

When a measuring instrument is to be designed, or is to be selected for purchase or for use, the question arises as to just how many distinct speci-

fications are involved. For present purposes, one can draw a somewhat arbitrary line to rule out economic and aesthetic factors, such as cost, convenience, appearance, ease of maintenance, etc. Then for instruments of a given class—that is, for instruments that measure any one given physical quantity—there appear to be three principal specifications. One of these is accuracy, and a second one is range. On these two there is doubtless wide general agreement. But the third specification is less familiar and, perhaps, more controversial. It may be called "instrument efficiency," or, in context where it will not be confused with the familiar thermodynamic term, simply "efficiency."

Instrument efficiency may be described as a measure of the magnitude of reading obtained per unit of energy exchanged between the instrument and the system being measured. Thus an instrument would have infinite efficiency if it could furnish a reading without having any interaction (energy exchange) whatever with the system on which the measurement is made. It is a complicated problem to find the best way of putting this idea of instrument efficiency into terms of an exact definition. (This problem is discussed elsewhere and will not be treated here.)

More pertinent to present purposes will be an effort to regard the concept of instrument efficiency in the general light of system response, and in the general light of cybernetics. From the point of view of system response, an instrument, as noted above, may be regarded as a system subject to the forcing of the true value of a physical quantity and giving as response a reading of the value of that quantity. Good accuracy requires that these two values agree as closely as possible, and wide range requires that the agreement hold over a large interval of values. These two specifications, therefore, seem rather simply related to the pattern of system response. But instrument efficiency does not seem so simply related, since it calls into the picture the hitherto extraneous element of energy or power. (The choice between energy and power in the exact definition is one of the complications mentioned above.)

A little reflection shows, however, that all three specifications are extraneous—none of them is a necessary part of the pattern of Figure 1. These desiderata together constitute something imposed on the general pattern to make it fit the case of measuring instruments; they constitute the design criterion for instruments. The general agreement—that accuracy, range, and efficiency are desirable attributes of the relation between forcing and response—is indeed a most necessary condition for advance in the art of instrumentation. By contrast, in the design of economic, political, or sociological systems, to all the other difficulties of design and execution there is added the overwhelming primary difficulty of obtaining general agreement on what constitutes desired performance of such systems. Yet all detailed design discussion of any system is surely somewhat inane unless it can be based on a clear expression of the performance expected of the system.

For any system, physical or otherwise, one can easily envisage concepts of "accuracy" and "range." Accuracy is simply a measure of how closely the actual response of the system conforms to the desired, or standard, response. Similarly, range can be defined in almost the same words as for instruments. But what is in general analogous to instrument efficiency? One comes here again to a suggestion of strangeness, of not being quite satisfied with this concept of instrument efficiency.

Suppose now that the concept is regarded in the general light of cybernetics. Communication, which according to Figure 2 is half the domain of cybernetics, may be described as the transfer of information; so that the idea of information is a very fundamental one, cybernetically speaking. The general problem of defining "amount of information" is discussed at some length in Wiener's book. Roughly speaking, the amount of information in any given item is taken to be proportional to the number of decisions (between alternatives) required to specify the item. Applying such a definition of information to the process of measurement, one may think of the output of an instrument as so much information about the quantity being measured. Then it is natural to define instrument efficiency as amount of information yielded by the measurement per unit energy interchange with the measured system.

But the amount of information is related to the number of significant figures obtained and so to the accuracy of the measurement; hence the concept of efficiency thus defined would not be independent of the concept of accuracy. Moreover, this definition of efficiency does not give any weight to the magnitude of the reading. Thus a d.-c. voltmeter reading of 100 volts to two significant figures would represent the same amount of information as the reading of 1 volt to two significant figures. Let I denote this amount of information. Suppose the 100-volt reading is taken at full scale on a meter having resistance of 200,000 ohms and the 1-volt reading at full scale on a meter of 20 ohms resistance. Then the efficiency for the two meters is the same, since I is the same and the input power is the same, namely, 50

milliwatts. This situation violates the intuitive feeling one has that a 2,000 ohm-per-volt instrument (the 100-volt meter) should be credited with higher efficiency, according to any definition, than one with 20 ohms per volt (the 1-volt meter). This "ohm-per-volt" specification is identical, for the special case of voltmeters, with the first general definition given above—reading per unit energy—evaluated at full-scale reading

In summary of this brief discussion of instrument efficiency, it must be admitted that no fully satisfactory definition has been presented. The definition as the ratio of reading to energy exchanged seems to be a useful one, but the problem needs further study. In any case, it is hoped that this discussion of the problem has served to illustrate some of the relations among instrumentation, system response, and cybernetics.

THE ULTIMATE SUPERINSTRUMENTATION

At the outset of this article it was implied that more questions would be raised than would be answered. A fundamental question, which was brought up but not answered, is the one concerning the relation between system response and cybernetics. Perhaps some sort of answer can be found. What has happened is clear enough—instrumentation has grown, and is still growing, into some kind of superinstrumentation. Or, expressed another way, instrumentation is the core around which a much larger structure of ideas is crystallizing and taking shape. In endeavoring to promote this crystallization, the author, interested primarily in measuring instruments, arrived at the mode of thinking described above as system response; Wiener and co-workers, preoccupied primarily with communication, servomechanisms, and computing machinery, arrived at the concepts of cybernetics. Are these simply two ways of regarding one and the same thing? Or are they two different cross sections of a larger entity, which transcends and encompasses them both? It may be that this question must be left to the future for final answering. But further speculation on the matter will not be amiss here.

Can system response be said to include all of cybernetics? Communication systems and feedback systems fit well enough into the pattern of system response, but computing machines and "thinking" machines do not fit so readily. There seems to be need here for distinguishing devices which *transfer* or *transform* information from devices which *generate* information. Efficiency of the former would be expressed in terms of how little information they *lose*: that of the latter by how much information they *gain*. Since a measuring instrument may be thought of as generating information, the problem of fitting computing machines into the pattern of system response may be closely related to that of fitting instruments into the pattern; and the problems of instrument efficiency and of computing machine efficiency may have much in common.

If there is some doubt as to whether system response can include all of cybernetics, there is also some doubt about the converse. Even when cybernetics is taken in the broadest sense of covering communication and control in the animal and the machine, with no restriction traceable to anyone's emphasis or interpretation thus far given, it would seem that the pattern of system response can be readily applied to many situations which are quite outside the domain of cybernetics. Any number of physical, chemical, and meteorological phenomena could be mentioned as examples—in fact, any situation where actions run their course, without the communication of information or the control of the actions toward a desired end. But there is one tour de force capable of reversing this situation. That is to note that all *knowledge* of the pattern of system response is achieved by the generation of information by measurement or observation; so that if the generation of information is made part of cybernetics, then system response might well be considered a part of, or a method in, cybernetics.

This suggests that the ultimate superinstrumentation taking shape might be a broadened cybernetics, as follows:

1) *Observation*—generation of information by:
   - instruments
   - sense organs
   - computing machines
   - brains
2) *Communication*—transfer of information by:
   - devices of communication engineering
   - nerves
   - language
3) *Control*—transformation and use of information by:
   - regulators and servos
   - organisms
   - societies

Such a threefold cybernetics—the science of observation, communication, and control in the animal and the machine—would seem to be a unified, coherent discipline, capable of standing on its own feet and taking its own place, a science among the sciences.

SCIENCE ON THE MARCH

GRASSLAND RESEARCH IN BRITAIN

BRITAIN'S agricultural program aims at raising food production by 20 percent within five years, to enable her to feed four million more people from her own resources It is planned to increase home production of cereals, in order to reduce the demand for imported grain, and to step up considerably the output of meat and dairy products.

To achieve the increase in meat and dairy produce, more animal feeds must be found. To avoid a heavy increase of imported feeds, an intensive program of home grassland development is being aimed at, with a considerable increase of silage making and grass drying. The climate and soils of Britain combine to make grass the major crop and one which must be exploited to meet the changing needs of agricultural production.

Britain's experts, reporting last April on the best means of applying research to the problems of increasing productivity, advised that the achievement of the agricultural program was well within the bounds of technical possibility. Under good management the nutrient value of an acre of grass could be increased two or three times, and the foodstuff value of the product could be equated with that of the best coarse grains. By concentrating on a program of intensive development of grassland, it should be possible to obtain within the next four years a 20 percent increase in the total yield.

The achievement of this program depends to a great extent on the work being carried out at the Research Establishments of Britain's Ministry of Agriculture. Among these is the Grassland Research Station at Stratford-on-Avon, Warwickshire, which is concerned with the problems of seed mixtures and leys in relation to soil fertility, sward management, and crop yields; the grass-legume ley is the main crop under study.

Recent researches have shown that the energy value of the grass-legume ley, when farmed at high level, is greater than almost any other crop that can be grown on United Kingdom farms. In terms of total dry matter, a productive ley will produce upward of three tons per acre of nutritious herbage in a normal season. Such herbage may have a starch equivalent of 65–70 percent, which means that the total starch equivalent per acre will be of the order of two tons. This compares favorably with a well-farmed crop of potatoes yielding, say, ten tons per acre, or with a 30-cwt. grain yield of wheat or oats. These comparisons are shown in Table 1, where it will be seen that none of the cereals and few of the root crops bear comparison with the starch equivalent and dry matter yields of good grassland.

The figures shown in Table 1 indicate:

a) High yield of starch and protein equivalents in grass compared with other crops.

b) The figures for oats, beans, kale, and potatoes are reasonably high averages for the better class of farm. Even kale is not as high in protein equivalent as is grass, although it may be higher in starch equivalent.

In making the comparison, the starch equivalent of the grass has been taken at 60 percent, which is low for a normally well-managed ley. The oat crop has been taken at 24 cwt. of grain for 36 cwt. of straw, but even if these figures were based on a 30-cwt. crop of grain and a 2-ton crop of straw it would still mean that oats are far lower in starch equivalent per acre than even a moderately good ley.

When we turn to the figures for protein equivalent the differences in favor of the grass-legume ley are found to be even wider. None of the cereals, pulses, or root crops approaches in protein equivalent per acre that produced by a ley in full production. Kale is the only crop which gives a protein equivalent production figure of the same order of values as the grass crop. These facts are of first importance, for they show that well-managed grassland can be made to produce more animal feeds than the normal run of arable crops used on mixed and livestock farms.

We can now turn to the question of the deficiencies and the *contra* aspects of the grass crop. The major point *contra* lies in the fact that grassland is extremely seasonal in its productivity. There is a long dormant period in winter from October to March or April, followed by a spring flush during May and June, when aggregate production from grasslands is considerably in excess of current requirements. The problem of the spring flush, therefore, is one of efficient conservation rather than of production. Traditionally, this

TABLE 1

YIELD PER ACRE OF DRY MATTER, STARCH EQUIVALENT, AND PROTEIN EQUIVALENT FROM LEYS AND OTHER FODDER CROPS IN BRITAIN

	Yield Per Acre		
	Dry Matter	Starch Equivalent	Protein Equivalent
	cwt.	cwt	cwt.
Perennial ryegrass (S.23) and white-clover (S.100) ley (G R.S. E.42, 1946)	74	44.4	8 9
Perennial ryegrass (S.24) and white-clover (N.Z) ley (G.R.S E 73, 1947)		29 0*	5.0*
Average good leys in Britain	60	36.0	7 2
Oats: 8 qr. crop=24 cwt. grain			
grain	24	19.4	2 1
straw	36		
Beans: ton crop=20 cwt. grain			
grain	20	18 9	4.4
straw	30		
Kale	25 (tons)	43 9	6.5
Potatoes	10 (")	36 0	1.2

* In 20 weeks starting May 20, 1947—a dry year—6 cwt.' starch equivalent and 1.0 cwt. protein equivalent per acre would have been produced before May 20.

presummer peak production has been conserved as hay designed for winter fodder. One of the major problems is to provide for better quality in the annual hay harvest. On a small scale hay is made on tripods and with good effect on hay quality. In many districts, as in the north of England, hay is made into pikes on the field—later to be brought into the stackyard. Most of the methods of haymaking are expensive of labor, and the research problem is to find efficient means of mechanization in harvesting weather that is traditionally wet and uncertain.

In recent years, following the researches of S. J. Watson and others, there has been renewed interest in the production of grass silage. Silage makes the farmer less dependent on harvest weather, has cheapened considerably the cost of the grass harvest, and has also produced grassy material of higher protein content. An obvious point in favor of silage is that losses due to maturation of the crop are lessened as compared with hay. Serious losses have frequently occurred in the harvesting and storage of silage, but with improved techniques these losses can, in large part, be minimized.

More recently, the drying of grass by artificial means has gained prominence in Britain. The process is still largely experimental but shows considerable promise. The dried grass problem may be divided into two closely allied subproblems: (*a*) the production of grass for the specific use of the drier, and aimed at keeping the drying plant fully occupied with drying high-quality material over a prolonged period; and (*b*) the engineering problem of reducing not only the capital outlay on the plant, but also the costs of drying as such. There is little doubt that the artificial drying of grass has come to stay, particularly since protein-rich animal feeds are likely to be in short supply over the world as a whole for a long time to come.

In her grass crop Britain has potentially an abundant source of protein which, if properly handled and effectively conserved, could easily make her self-sufficient insofar as the requirements of a greatly extended livestock industry are concerned. In this connection the grass drier will play an important part, because losses due to both maturation of the crop and to harvesting can be deleted entirely. Britain has, then, the potentiality not only to produce good grassland but to conserve any excess over current seasonal requirements when the product is at a high level of quality as a protein-rich concentrate.

The proper conservation of the spring flush of grass production can help immensely to solve those problems inherent in the extreme seasonal distribution of grass growth. There are, however, other approaches to the problem which are the subject of much detailed investigation. For example, the low production period after midsummer on many of Britain's pastures can be dealt with by the use of specific seed mixtures coupled with appropriate grassland management and

manuring In the drier parts of England, the normal ryegrass ley will usually enter a period of almost complete dormancy from early July until the end of August. On the other hand, leys based on lucerne with cocksfoot can be brought to a peak of production during this period. Another seed mixture that will produce an abundance of herbage at this time (July and August), if appropriately managed and manured, is that based on cocksfoot, meadow fescue, and timothy with white clover.

The winter (October to March) period of low production on our grasslands presents a much more difficult problem. Under most conditions in the United Kingdom, the well-managed ley will remain green throughout a normal winter but will be largely unproductive during that period. If especially managed, however, growth can be maintained fairly easily through October into November. The winter-green strains of cocksfoot, timothy, rye grass, meadow fescue, and some other species have assisted in an appreciable measure in lengthening the grazing season into the autumn months. These same seed mixtures are capable of producing also early growth in the spring, so that the winter period is fairly easily shortened; but there remains from late November to the end of March a serious gap which has to be filled.

During this period, quite obviously, the material conserved as hay, silage, or dried grass at the spring flush can be (and generally is) fed to livestock. Current researches, however, suggest that the present gap in winter grass (harvested either *in situ* by the animal or dried artificially) can be still further reduced. Here full use of the knowledge of the behavior of the winter-green grasses available must be made. An eye must also be kept open for any new material that may appear, whether exotic or as a result of indigenous plant breeding. Up to date, the greatest promise is shown by leafy strains in timothy, meadow fescue, and cocksfoot. The pasture strains of timothy (Aberystwyth S.48 and S.50), meadow fescue (S.215), and cocksfoot (S.143) show considerable promise in this respect and will produce green herbage in the period December to February. When these grasses are cultivated in wide drills (two-foot spacing) and effectively manured during the early autumn (August and September), they produce a wealth of leafage which remains tolerably winter-green, of high nutritive value, and very palatable, well into the early months of the year. Other grasses that show promise in this same connection are tall fescue (*Festuca arundinacea*), foxtail (*Alopecurus pratensis*), tall oat (*Arrhenatherum avenaceum*), *Phalaris tuberosa, Poa* species, and *Festuca rubra.*

To sum up, grassland research and its application to practice in Britain will tend on the one hand to close the gaps in seasonal production from grasslands so far as this is practicable. At the same time it is also concerned with the need to conserve material at the peaks of production during the summer and to harvest high-quality material as dried grass and silage, while at the same time preserving hay of medium to high quality. Grasslands can therefore be made to provide the bulk of rations for livestock, and at the same time full use can be made of the arable residues, both roughages and grain.

Clearly, arable and grassland farming are closely complementary in relation to the livestock industry; but grass will always remain Britain's major crop, not only because of the large proportions of land occupied by grass swards but also because well-farmed grass is the most productive crop in terms of energy values—starch and protein equivalents.

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PETROLEUM AND THE GROWTH OF THE PACIFIC COAST*

UNDER the American system of free enterprise, population and economic activity are able to expand in areas that are rich in natural resources. The Far West has been abundantly blessed with such resources, of which the principal ones are fertile soil, timber, water in considerable quantities, gold and other metals, and petroleum and natural gas. In addition, a favorable climate is a major asset and has been responsible for attracting people and industries in ever-growing numbers to the region. With energy and foresight, the Far West has made intelligent use of its many natural resources. The early impetus came primarily from the mining of gold and cultivating the soil, and the present century has seen the expansion or continuation of these activities, together with the intensive development of our other wealth.

* Based on an address presented at the Annual Meeting of the American Institute of Mining and Metallurgical Engineers, San Francisco, February 13–17, 1949.

It is hardly necessary to quote extensive statistics on the growth of the area, for the story is well known. A few population figures—covering the five states of California, Oregon, Washington, Nevada, and Arizona—will suffice. The present population is 15,000,000. Just before the war, it was less than 11,000,000; five years hence it is expected to be about 17,000,000. Our population is already more than double that of Sweden, greater than all of Canada, and ultimately should equal the present population of Mexico (Fig. 1).

The total value of the soil and subsurface products in the five states in 1947 was between four and five billion dollars. Superimposed upon these natural-resource industries are the products of our diversified manufacturing activities, which have a total value of about six billion dollars. When an area has great natural resources and fast-growing population and industry, its people are obviously in an economic position to purchase larger than normal amounts of many products. Such is the case here, as demonstrated by the fact that the average per capita income in these five states is about 20 percent higher than for the remainder of the country.

The activities represented by 15,000,000 people and the industries in which they are engaged require mechanical energy in vast quantities. It is interesting to compare the "source-of-energy pattern" in the Far West with that for the rest of the United States, and then to see how our large supplies of petroleum fit into that pattern. If the various fuels are converted into terms of British thermal units, it will be found that approximately 50 percent of the total energy delivered to consumers in the United States, exclusive of the five Western states, is supplied by coal. Petroleum supplies 35 percent, natural gas about 14 percent, and hydroelectric power only 1 percent. In striking contrast, the Far West is essentially an economy without coal, with only 5 percent of its total requirements being supplied by that commodity. About 4 percent is obtained from hydroelectric power, 22 percent comes from natural gas, and almost 70 percent from petroleum (Fig. 2). Thus the proportion represented by petroleum is just double that in the rest of the country.

It is readily apparent that, without large indigenous supplies of oil and natural gas, the growth of the Far Western states would have been much less dynamic than its history shows. With our other natural resources, a rapidly expanding population and industry, and with our lack of coal, the increase in demand for oil has been exceptional. Not only has it been necessary for Western petroleum to meet the fuel and lubrication needs of our

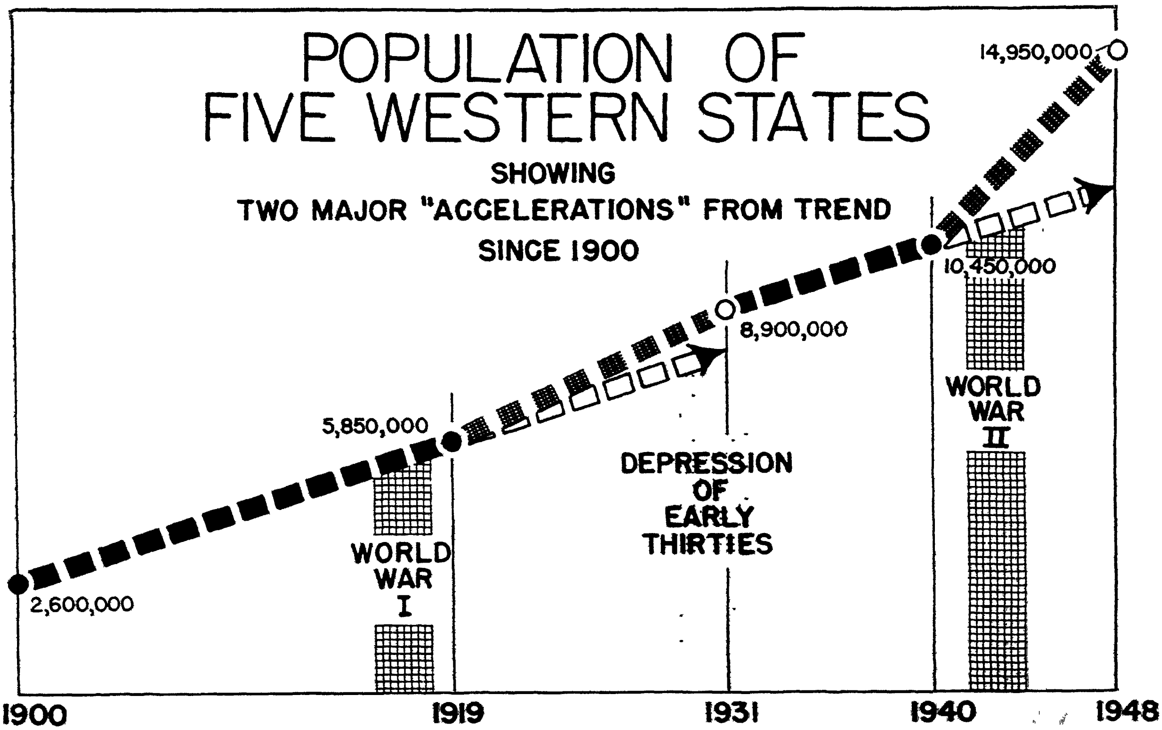

FIGURE 1.

millions of automobiles, trucks, buses, tractors, and other internal-combustion equipment, but, along with natural gas, it has assumed the burden of supplying our extractive and manufacturing industries, furnishing fuel for heating and cooking in our homes, and providing the fuel for the network of railroads that cover the broad expanses of the Far West. Recently, the electric utilities have made a heavy draft upon oil to operate their expanding steam-generating facilities. The large coastal and offshore shipping business is an important source of demand for fuel and Diesel oils; fuel oil bunkers the tankers that carry petroleum around the perimeter of the Pacific Ocean; the military requires approximately 100,000 barrels each day of various oil products; about 80,000 barrels of petroleum are required daily to meet our export demands; and Alaska and Hawaii draw upon this area for their liquid fuel requirements. Over all, a million barrels per day of raw materials must be produced, transported, refined, and distributed. In 1941, this figure was only 700,000 barrels.

These demands are expected to increase further in the future, although in less substantial proportions than in the past. One of the factors contributing to this situation is the rapid dieselization of the railroads, a trend that is becoming nationwide. Outside the Western part of the country, locomotives use principally coal; in the West, however, little coal is available for rail use, and, since one barrel of Diesel oil will do approximately the same work as four barrels of heavy fuel oil, the replacement of steam locomotives by Diesels results in very large declines in the consumption of heavy fuels in our area. In 1946, in the five Western states alone, the railroads daily consumed 112,000 barrels of heavy fuel. In 1948, they consumed just over 90,000 barrels, a decline of almost 20 percent in two years, despite high levels of railroad activity. Dieselization is not complete and, as it progresses, should cause further material reductions in rail use of heavy fuel oil, with relatively much smaller increases in the use of Diesel oil.

California in the past has produced ample supplies of crude oil and natural gas to meet the full requirements of our Far Western economy. In recent years, fears have been expressed that the supply may become inadequate to meet anticipated demands. Despite the fact that few large new fields have been discovered in the past decade, California increased its crude oil production from 631,000 barrels a day in 1941 to 913,000 in 1947. Concurrently, estimated proved reserves in the ground

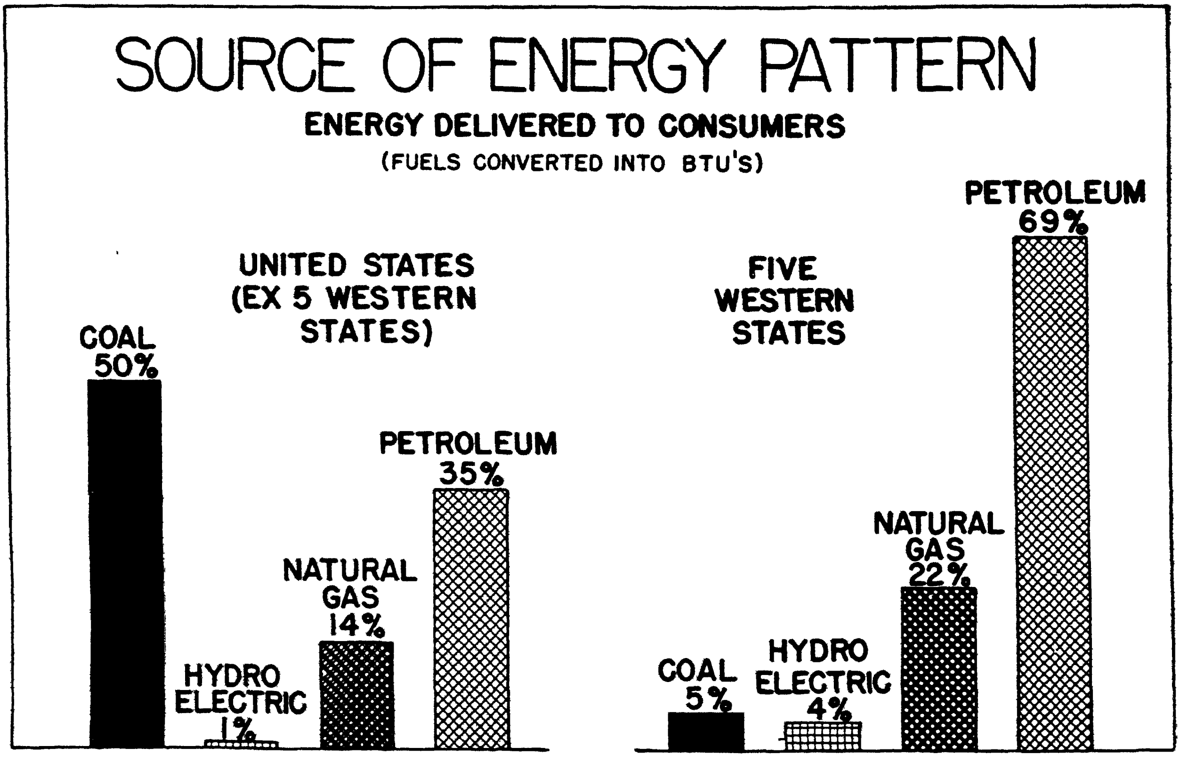

FIGURE 2

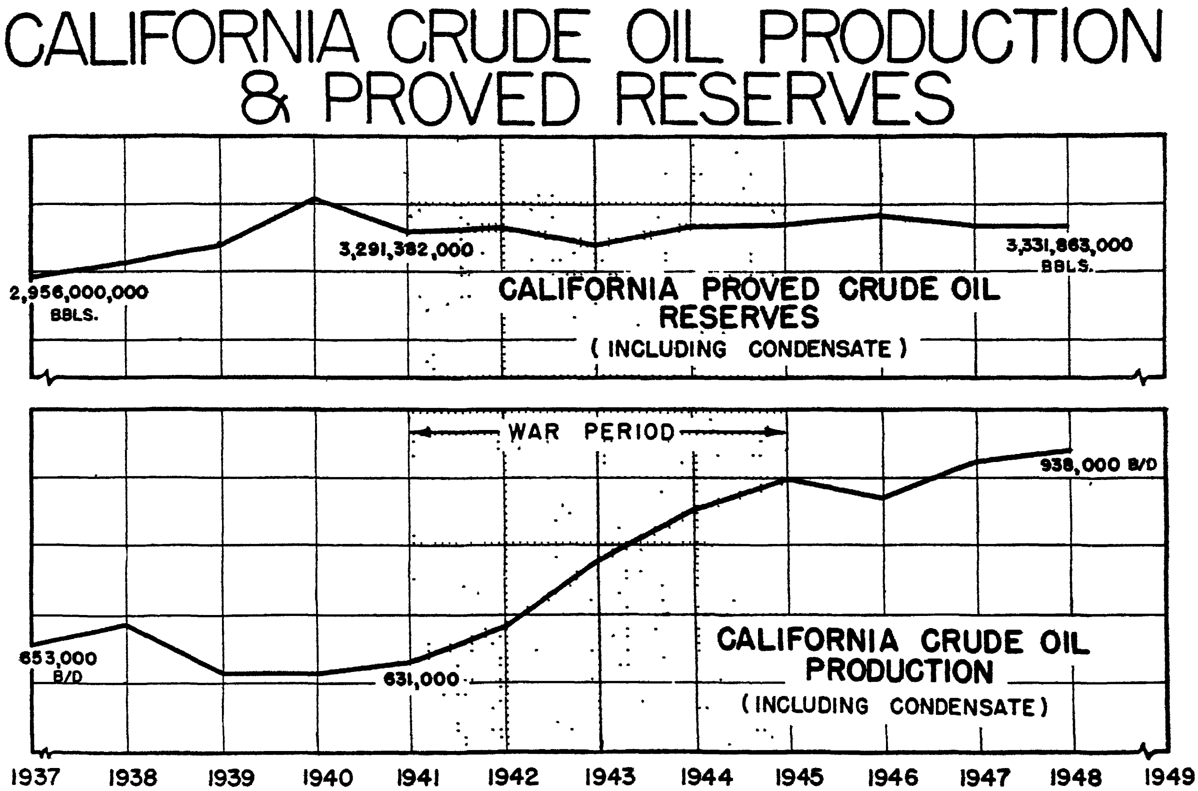

FIGURE 3.

remained substantially unchanged. Today, the state is producing about 950,000 barrels daily, and should be capable of doing so for several years to come (Fig. 3). This volume appears to be ample to take care of all demands over such a period, barring another national emergency. The present production level is the highest in the history of the state, despite the practical elimination of Elk Hills from the picture—a field that contributed much crude oil during the war. California's possibilities for developing new production are by no means exhausted. New petroliferous areas have been proved recently, the ocean off our shores is being surveyed by seismographic methods in a search for hidden geological structures, and some of our fields have secondary recovery possibilities.

Vast supplies of crude oil and natural gas, both proved and yet to be proved, are lying under the surfaces of west Texas and New Mexico, the Rocky Mountains, and Alberta, Canada, all of which are economically contiguous to our Western states. As a matter of fact, a recently completed pipe line from Rangely Field in Colorado to a new refinery in Salt Lake City will supply petroleum products to certain areas previously supplied from California. The fields of Venezuela, and even those of the Middle East, form a further backlog of protection; for example, much Arabian crude oil has already been shipped into the United States and Canada.

The present extensive requirements of the Pacific Coast economy have caused our natural gas supplies, obtained from both oil and dry gas fields, to become insufficient to meet all demands. This condition, however, is being corrected through the construction of pipe lines from Texas and New Mexico to California, and probably from Alberta to the Pacific Northwest. These supplemental supplies of natural gas, which will attain large volume by 1950–51 if announced plans are carried out, will curb demand for liquid fuels, particularly heating and heavy fuel oils, and thereby further reduce the future burden upon California's oil fields.

The growth in population and the general economy of the West have been based upon a solid foundation of rich natural resources and other favorable factors. Further growth may be expected, and petroleum will continue to play its vital role in the destiny of the region.

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RECENT PROGRESS IN TROPICAL MEDICINE

THE Fourth International Congress of Tropical Medicine and Malaria, held in Washington, D. C., in May 1948, was unique among medical meetings in the degree of democracy and fellowship encountered. A large number of the delegates (who came from all parts of the world) were research men who had spent years in the tropics, often in isolated areas, undergoing hardships that would cause the professional explorer to beat a hasty retreat. We have met these men along the steaming Amazon, in the rarefied atmosphere of the Altiplano, and in the isolation of the African veld. They are often difficult to tempt into giving accounts of their personal experiences, but when differences of opinion arose there was no hesitancy in expressing a healthy and well-substantiated conviction on both sides.

For example, the perennial question as to whether syphilis and yaws are manifestations of the same etiology arose following the report of Dr. Leon-Blanco. Men of many nations were drawn into the debate that followed, and the picture is now further confused by the disease pinta being included. Those taking part in the discussion whose homes are in the tropics and who, therefore, have had years of intimate knowledge of these diseases in all their varying forms are of the opinion that we are dealing with separate entities. Dr. A. Carrion, of Puerto Rico, perhaps adequately summed up the matter when he stated:

> It is difficult for me to believe that anyone who has had extensive experience with these conditions can retain any doubt but that they are different. I agree with Dr. Turner that it is helpful to consider these as separate entities. Only when we can culture the organisms on artificial media shall we be able to answer the question of whether these are different species or one species with three varieties.

Dr. Robert E. Johnson and Dr. W. S. S. Landell reported on the adaptability of individuals to tropical climates. They pointed out that in order to avoid mental and physical deterioration during periods of excessive heat, it is necessary to sleep through the cool of the morning. This necessitates late retiring and equally late rising, which should be of interest to all of us in this country, where certain groups have forced Daylight Saving Time upon the working public.

Malaria. Malaria is the perpetual target of greatest interest in tropical medicine and has been the subject of more careful study and investigation than perhaps any other disease known to man. In spite of the vast amount of accumulated knowledge, however, it continues to head the list of causes of sickness and death throughout the world. The economic loss due to malaria is incalculable, and vast areas of fertile land remain idle because of it.

Malaria is a controllable disease, yet it is seldom controlled. The annual death rate in some sections exceeds two hundred per one hundred thousand population per year. Recent work on the Island of Cyprus demonstrates, however, that the disease can really be conquered when there is determination to do so. Here the approach has been to destroy the adult and the larval *Anopheles* mosquitoes by using the newer insecticides and larvicides —particularly DDT. The entire island has been divided into small areas of 3–8 square miles, and in each of these trained men hunt down the mosquitoes in their obscure and often almost inaccessible breeding places. Painstaking diligence has paid dividends. In 1945 in Cyprus, 40 percent of all school children suffered from malaria; in 1948 the rate was only 1.3 percent. The entire campaign is expected to be completed this year at a total cost of little more than a million dollars.

In Holland, too, it has been reported that with the support of the International Health Division of the Rockefeller Foundation malaria control has made great advances. The residual effect of sprayed DDT was found to last for about five months in Holland. Therefore, spraying is done just before the malaria-transmission season begins and is sufficient to control the *Anopheles* mosquitoes for that year. In treated villages and towns, malaria was practically eliminated in 1948.

Cyprus and Holland are only two of many examples in point. In other portions of the world, too, much has been done toward controlling malaria by means of sanitation. Elsewhere, of course, there are still large areas in which proper sanitation is not economically feasible or physically possible. The search must go on for antimalaria drugs with which to treat the inevitable millions of cases. Many cases will continue to occur each year even in the United States.

During World War II, hundreds of potential antimalaria compounds were tested by researchers in the United States, Great Britain, and elsewhere. From the testing there emerged new drugs with undeniably good therapeutic effect. These include such 4-aminoquinoline drugs as chloroquine, such 8-aminoquinoline drugs as pentaquine, and the British-developed benzene-ring compound paludrine.

Chloroquine has in many locations completely supplanted atabrine as the medicine of choice in treating the usual case of malaria. All species of the malaria parasite respond quickly to chloroquine, and only a relatively short course of treatment is necessary. This is in contrast to the six or seven days' treatment required with atabrine. The suppressive dosage of the two drugs is quite different, too; whereas a daily atabrine tablet was found necessary to suppress malaria in malarious areas of World War II, only one dose weekly is required with chloroquine.

Some drugs with a 4-aminoquinoline structure related to chloroquine may prove equal or even superior to it. One of these, called camoquin, has had clinical trials in India, the Philippines, Brazil, and Bolivia and appears to be quite effective in stopping the course of the usual case of malaria with only a single dose of three or four 200-mgm tablets. Further studies on this promising substance are proceeding in these locations and on the Isthmus of Panama, and it is likely that more will be heard about it shortly. Obviously, the availability of a relatively nontoxic drug that is effective as a single dose by mouth will be of untold advantage in the treatment of malaria in rural areas and/or, when necessary, in ambulatory patients. Camoquin is not yet available in the United States.

The Fourth International Congress brought together an enormous accumulation of recently acquired facts on malaria. Colonel H E. Shortt, of the University of London, School of Hygiene and Tropical Medicine, reported on the obscure development of the malaria parasite during the time which elapses between the bite of the infected mosquito and the entrance of the parasites into the blood. The doctor used the parasite which infects monkeys for his study, and his exhaustive work has cleared up some of the doubtful phases that remain in the life of the plasmodium. He discovered that this intermediate or exoerythrocytic development took place in the liver and he followed this development through the days of its successful evolution.

Hookworm infection: The intestinal roundworms are considerably more of a problem than is generally realized by nonmedical personnel—or even by most physicians. As a matter of fact, hookworm and schistosome infections rank next to malaria as the most dangerous diseases of mankind. (A brief concept of the magnitude of the hookworm problem even in the United States was conveyed in the March 1949 issue of THE SCIENTIFIC MONTHLY.) Laughlin and Spitz, in a recent article in the *Journal of the American Medical Association,* have estimated that one in every fifteen servicemen on Pacific duty returned to the United States with the "Old World Hookworm," *Ancylostoma duodenale.* There is a distinct danger that this hookworm, which is more resistant to treatment than the "New World Hookworm," *Necator americanus,* may become firmly established in this country if cases are not discovered and treated as soon as possible.

Dr. W. O. Cruz has ably defended the conception that hookworm anemia is a deficiency disease and can be combated effectively by correcting nutritional deficiencies.

Generally, there is some reluctance on the part of physicians and laboratory personnel to thoroughly examine the stool specimen under the microscope for worm eggs. As Laughlin and Spitz state, "The extensive contact with malodorous excrement is usually enough to stifle the enthusiasm of the most ardent diagnostician" These investigators have described a method of stool examination which concentrates the worm eggs in a stool specimen by making use of certain proportions of saline, aerosol, ether, and xylene. It seems a rather better method than other concentration methods which use brine, zinc sulphate, etc. With an ordinary fecal smear, at least 1,200 hookworm eggs per gram of feces are required before detection is regularly possible, but with the "AEX technique" very light infections may be picked up.

It is fascinating to note that, in the midst of warnings and admonitions about the seriousness of hookworm disease and the necessity of early diagnosis and treatment before onset of significant anemia, a therapeutic use for the disease should be found. But such is actually alleged to be the case A report appearing in the *Indian Medical Gazette* describes the use of artificial infections with *Ancylostoma duodenale* for the treatment of the disease polycythemia, an abnormal increase in the number of red blood cells in the human blood. The Indian physicians use 300–600 hookworm filariform larvae and apply them to the skin of the patient under wet blotting paper so that they will penetrate the skin. In due time, the hookworms reach the intestine where, as mature worms, they each account for almost a cubic centimeter of blood loss daily, which offsets the patient's polycythemia. Instead of causing anemia, as it otherwise would in a normal individual, in polycythemia patients the hookworm infection produces a rather normal hemoglobin value and red blood cell count. This type of treatment has been called "ancylostoma-

therapy," and has been used in about twenty-five patients with good results. It is a simple, long-lasting type of treatment because the duration of the untreated hookworm infection in the human body is five to six years.

Filariasis: Filariasis is the term applied to the infections in humans and animals caused by various small roundworms. Most generally, however, it is applied more specifically to the particular disease of humans caused by the worm *Wuchereria bancrofti*. Filariasis, when referring to the illness produced by *W. bancrofti*, is mosquito-transmitted. Ordinarily, it is a self-limiting febrile illness, and it is only when infection occurs repeatedly that serious symptoms occur. The mature larvae and the adult worm, passing through tissues, especially through lymphatic vessels, cause blockage of the lymph vessels and/or local reactions. Enlarged glands in the inguinal or groin region may occur, or there may be swelling of the testicles. Very severe cases in natives occasionally give rise to such a marked lymphostasis that a whole leg may become markedly enlarged and disfigured, a condition popularly known as "elephantiasis." During World War II, filariasis became a serious problem among servicemen in certain areas, mainly because widespread rumors arose that men so afflicted would become sexually sterile. Such false rumors caused considerable unnecessary consternation among the troops.

In spite of the fact that only repeated infection with filariasis will produce permanent swellings in the groin, scrotum, or leg, it seemed desirable to treat the cases that occurred among servicemen, although no satisfactory drug was known. The pentavalent antimony compound, neostibosan, and the trivalent antimony compound, fouadin, were used, but the results were not remarkable. Very recently, however, there has come to knowledge a substance called hetrazan (1-diethylcarbamyl-4-methylpiperazine hydrochloride) which appears to be quite promising. Treatment of patients in Puerto Rico resulted in either complete disappearance of all the worms or their reduction to very small numbers. In general, the worms completely disappeared from those patients who had received higher dosages. In none of the treated individuals was there any evidence of the disease in the lymphatic vessels, nor any other symptoms suggesting clinical filariasis. Hetrazan, therefore, seems about to take a conspicuous place in the treatment of one more of the previously well-nigh untreatable tropical maladies.

Typhoid fever, although not strictly a tropical disease, is of primary importance in the tropics. Early experimental work, proving that chloromycetin is a highly effective remedy against this disease, has been amply substantiated. This adds another disease to the credit of this new antibiotic, in addition to typhus and scrub typhus, for which it is so highly specific.

The steady advance in the therapy of Hansen's disease continues as new drugs prove their effectiveness. Before the end of the present year we shall hear of new compounds which produce results in a shorter time and are free from unwanted side reactions. Everyone engaged in work with Hansen's disease is grateful for the enlightenment of the public toward this malady. The superstitions and unfounded fears that have been associated with it are gradually disappearing, and the public is beginning to understand that Hansen's disease is nothing mysterious, but a chronic disease difficult to contract and of less danger to the public at large than the common cold.

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BOOK REVIEWS

SINOLOGY

History of Chinese Society: Liao (907–1125). Karl A. Wittfogel and Feng Chia-sheng. xv + 752 pp. Illus. $12.50. Macmillan. New York.

THE appearance of this huge book by Wittfogel and Feng ushers in a new era of research in Chinese history. Although the present work covers a very short period of Chinese history (a little over two hundred years) and though, even in this period, it covers only the recorded events of the alien dynasty which ruled a portion of China, the contributions of this volume, first of several to come, are extremely significant.

For the first time, the authors have made available to Western scholars who cannot read Chinese, and to Chinese scholars who do not have the necessary background or time, a vast body of material, classified, annotated, and interpreted in accordance with a particular theoretical framework arising from Dr. Wittfogel's many years of thought and study. The most important theoretical conclusion of the present volume has been explained very clearly by Wittfogel in his General Introduction. For many decades it has been commonly held by Sinologists, including even Professor Pelliot of France, that conquerors of China in historical times have always been completely absorbed by the Chinese. The materials presented in the present volume show how ill-founded such a view is. The authors have shown that rulers among the Chitans, founders of the Liao dynasty, not only resisted Sinicization, but they also attempted extension and expansion of their own cultural norms and ideals. In the end, when they no longer could reign in China, they preferred to return to tribal life in order to maintain their identity, instead of taking on a mode of life characteristic of the Chinese.

Some readers may raise two possible lines of objection. The first is an old one, and has been current among a number of Chinese scholars since Dr. Wittfogel visited China in the early thirties. These scholars maintained that the Chinese classics are too complex for any schematic treatment and that Dr. Wittfogel has merely been forcing the data into preconceived categories. This view is untenable for two reasons. First, those who have criticized Wittfogel in this vein have nothing better to offer. Second, science progresses by a race between data and theories, with the one sometimes ahead of the other. All scientific research involves some categories or hypotheses which must be tested by the data subsequently collected. To collect data one must have some theories; but to elaborate and maintain the theories one must have adequate data.

The second line of objection can be more vital. Some readers will think that Wittfogel and Feng have greatly exaggerated their case. These scholars may observe, for example, that most of the activities of the Chitans recorded were those of the ruling group, that many of the facts have merely been assumed from legislation or imperial orders, that no two peoples in contact with each other as were the Chitans and the Chinese have been known to retain completely their original identity, and that, therefore, although the "complete Sinicization" theories are questionable, the "complete resistance" theory is equally unscientific.

There is probably no effective defense against this criticism. What we can say, perhaps, is that Wittfogel and Feng's work represents a reaction against the theories so long current in Sinological thought and that in time a better-balanced theory will emerge, with the two extremes serving as checks on each other.

The present volume will be welcomed by all students who are connected with work on China in general and that on Chinese history in particular. The work will stand for many years to come, not only as an important source of inspiration but also as a standard against which future works will be measured. The reviewer, among others, will be anxiously waiting for the appearance of other volumes of the Chinese History Project, under the directorship of Wittfogel and with the able collaboration of such scholars as Messrs. Chu Tung-tsu and Wang Yu-chuan, and Mr. and Mrs. Fang Chao-ying.

FRANCIS L. K. HSU

Department of Anthropology
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THE EVOLUTION OF PHYSICS

From Euclid to Eddington. Sir Edmund Whittaker. ix + 212 pp. $4.00. Cambridge University Press. New York.

THIS book, as the author says in his preface, is not intended to be a summary of present-day knowledge in physics, but rather a history of the evolution of concepts and principles, especially such as have provoked long controversies, in some cases still unsettled.

In the course of this evolution there have been several developments of the first magnitude in importance, such as non-Euclidean geometry, relativity, and quantum mechanics. The author describes and discusses these developments in considerable detail, with some interesting comments on certain points. For instance, in discussing determinism, which asserts that all events in a physical system take place *causally,* he says that whether this assertion be true or false is

a question which can be decided only by a theory based upon, and tested by, observation. He goes on to say: "We may ignore all the metaphysical nonsense that has accumulated in this connexion." As an example of such nonsense he cites in another place a statement by Whitehead that from the philosophical point of view a predicate need not have a subject; in other words, that motion may take place without anything being moved; and that therefore there is no necessity for inventing an ether to transmit light waves through a vacuum.

The concepts of quantum mechanics are discussed by the author in considerable detail; and in this part of the book he finds it impossible to get along without mathematical equations, which he has rather successfully avoided in earlier sections.

In the concluding part of the book the author discusses Eddington's account of the structure of the universe, and closes by saying that the work of unification effected by Eddington may be compared with the great reform achieved by Maxwell seventy years earlier when he correlated electrical and optical phenomena on the basis of a single ether. The author states that Eddington's work has brought into relation the domains of general relativity and quantum mechanics, and has thus laid the foundations of a unified doctrine of nature.

PAUL R. HEYL

Washington, D. C.

SCIENCE AND MOTION PICTURES

Painting with Light. John Alton. 191 pp. Illus. $6.00. Macmillan. New York.

LIKE most things out of Hollywood, John Alton's *Painting with Light* is a slick job of telling how he and his contemporaries achieve their triumphs.

This 191-page book is crammed with nearly 300 illustrations. The author is a member of the charmed group of the American Society of Cinematographers. Mr. Alton covers every angle and problem with which a camera man or lighting expert would be confronted during the course of filming a picture. Every addict of 35-mm, 16-mm, or 8-mm cameras would do well to have this book in his library.

Actually, Mr. Alton not only presents problems but solutions to them, as in the chapter on Mystery Lighting. This portion should attract the attention of still-camera men, illustrators, and pictorialists. It is broken down into such subjects as shipwrecks, flashes of guns in absolute darkness, street lights, fireplace scenes, foggy nights, and the all-important low key shot of doubles, where the Actor's Guild requires the employment of acrobatic doubles.

The book continues with an endless number of short, concise hints, either to do or not to do, always tempered with moderation. I quote, "Snow scenes can be beautiful but for good results shoot in the early morning, even before sunrise or late in the evening. In the time between the light is flat." He goes on to say, "Nobody in his right mind would think of taking a picture in similar light inside, so why make the mistake outside?" He adds, "The wind, haze, mist, fog and storm are all elements that make snow scenes beautiful. Those are your colors. The sun is your brush, so go ahead and paint."

Tribute is paid to the late, great Alfred Stieglitz for his outstanding still portraits, noting that it took the film industry a long time to invent the motion-picture close-up.

The Hollywood Close-Up chapter is most thorough. It deals with camera angles, illumination, composition of subject and background, choice of lenses, make-up, and important tips on how to feature or suppress good or bad features of a subject.

This reviewer hopes that, someday, someone will turn out an equally fine book concerning the problems of the still photographer.

A. AUBREY BODINE

Camera Magazine
Baltimore, Maryland

SCIENCE AND ART

Art as the Evolution of Visual Knowledge. Charles Biederman. xi + 696 pp. Illus. $15.00. Charles Biederman. Red Wing, Minn.

MR. BIEDERMAN is a creating artist. Both his interest in science and his attitude as a writer appear to be much influenced by his efforts and enthusiasms as inventor and advocate of a new plastic style. He "rejects" both the academic and modern viewpoints as evasive of present responsibilities and finds his particular type of "nonobjective" art the only legitimate direction for the "non-camera" artist today.

The format of the book is impressive. Quarto, with a light-blue cloth cover and stamped white title, it is heavy and a trifle awkward. It is profusely illustrated with excellent half-tone reproductions of subjects from leading museums or printed by special permission of publishers of authoritative books on art, history, and science. These illustrations include human creations from Mousterian cupstones to photographs of X-ray diffraction patterns of urea oxalate. Of many works of individual artists shown, two are by Michelangelo, four by Leonardo, six by Monet, sixteen by Picasso, and sixteen by Biederman.

The artist-author's inventions in painted wood, metal, and plastic, when pictured beside X-ray diffraction patterns, crystal and molecular models, and mathematical string models suggest his fascination with science and his inspiration from certain of its visual aspects. One composition, called "White Construction with Colored Lights" (54½″ × 52″ × 12½″), made from painted glass and wood with hidden fluorescent tubes, is installed in the Interstate Clinic, Red Wing, Minnesota. The visual pleasures afforded by the fine reproductions of Mr. Biederman's "constructions" are apt to appeal to many readers as

more convincing than any satisfaction of be derived from his justification of "Constructionism." His book shares the compositional ingenuity of his visual arrangements, but his words often lack their precision and clarity. He constructs his novel reorganization of art history in two very unequal time divisions, to which about equal amounts of space are devoted. Art before 1840 is separated from that of the present by a smaller third division of eighty pages on the fifty-year period between the invention of the camera and the innovation of cubism. In the author's opinion, this crucial half century effected the transition from the age-long struggle of the artist to record the appearance of nature literally (with innumerable regressions such as the period of Christian art) to the present in which the artist strives to create art, not "imitate" the art of nature

A bibliography of 374 titles, many of distinction, is listed, from which quotations are liberally appropriated and freely applied. Sometimes these are interspersed between sections, sometimes included in the text. The former, coming in short series, produces a montage of impressions of the meaning to follow. The latter is often a clarification of it, as in the following example:

> Man must conform to the structure and material function of the organism and environment, the factors which constitute "reality," if he is to be able to manipulate and exploit that reality for his own purposes; in fact, the extent of his ability to do so depends precisely upon his ability to conform to the structural function of the world process. This Francis Bacon recognized in the 16th century when he wrote: "In order to master Nature we must first obey her."

Had the ardor of the artist been tempered with scholarly frugality, a much-compressed work might have said more. Mr. Biederman is his own publisher.

THOMAS M. BEGGS
National Collection of Fine Arts
Smithsonian Institution

INTRODUCTION TO ANTHROPOLOGY

Man in the Primitive World. E. Adamson Hoebel. xii + 543 pp. Illus. $5.00. McGraw-Hill. New York.

ONE index of the rapid growth anthropology has undergone in recent years is the number of general textbooks published. Four appeared in 1948, and two during the current year, a total far greater than in any previous comparable period. Of these, Hoebel's introductory text is among the very best.

In the author's own prefatory remarks,

> Anthropology must bring its conclusions to bear upon the problems of modern society; it must place its methods at the disposal of all the other sciences. Nevertheless, its great contribution to knowledge has been derived from its special quality as a comparative science. . . . It is still best to keep anthropology firmly rooted in the data of primitive society. . . . In keeping with this conviction, this book is primarily a study of man in the primitive world.

Accordingly, Hoebel has provided the student with a balanced series of chapters treating of human paleontology and prehistoric archeology, physical anthropology, the various aspects of primitive (*i.e.*, nonliterate) society, and the relationships of society and culture as expressed in culture areas, personality, invention, diffusion, and cultural evolution. Pertinent examples are given throughout, mainly from North American Indian tribes. On questions of theory and interpretation the author has maintained a strong measure of eclecticism. Understandably, no one person can be a master of all the many branches of so vast a discipline as anthropology; Hoebel's particular forte has long been primitive property, law, and government, and the chapters devoted to these topics are a real contribution to other professionals as well as to the student reader.

One omission in this text, of consequence particularly if it were to be used by underclassmen, is that Hoebel nowhere provides an extended discussion of the learning processes, as formulated in psychology, and on which rests the acquisition of cultural behavior among humans. The professional readers can discern, from numerous passages, that here, too, the author is eclectic. But it is one of the problems of teaching introductory anthropology to provide the student with a coherent psychological framework in which he can view behavior, and its development in individuals, in societies whose cultural patterns are often radically different from his own.

A glossary of technical terms, names, and concepts forms a valuable adjunct to this work; two things lacking that would have further enchanced the book's usefulness to students are a table of figures and the inclusion of the glossary items in the general index, for, although the various terms all appear in the body of the text, the glossary does not give page references. Several unfortunate typographical errors somehow crept in: it is *Bogaras,* not "Bogarus;" *Herskovits,* not "Herskowitz;" *Montagu,* not "Montague;" *R.M.,* not "R. I.," McIver; *S.D.,* not "S.O.," Porteus; and in some mysterious fashion Ralph Linton has acquired the middle initial M. which appears in none of his writings, Library of Congress cards, or biographies.

Style of binding, format, and slick paper combine to give the whole book the not unpleasant air of "solidness" characteristic of McGraw-Hill texts.

D. B. STOUT
State University of Iowa

THE TIMES WE LIVE IN

Reflections on Our Age. 347 pp. $4.50. Columbia University Press. New York.

THIS volume consists of twenty-two lectures delivered at the opening session of UNESCO in Paris in the latter part of 1946. It was hoped that some of the speakers would develop ideas that would guide the conference in its cultural activities, and that others, because of their eminence, would stimulate

public interest in the work of the meeting. As is customary in such enterprises, there is a considerable variation in the quality of the papers.

There appears to be such a strong determination on the part of UNESCO to avoid the evils of this world that some of its aims may occupy its energies for many years. Because of exchange controls and other impediments, the scholars of many countries find it impossible to acquire the books and journals they need; there are embargoes on the acquisition of works of art; scholars, particularly archeologists and anthropologists, find that they cannot visit parts of the world that they must see in order to carry on their work. One of the purposes of UNESCO, and a seemingly practicable one, is to moderate this heritage of World War II. It has set itself the further aim, however, once the scholars of the world are set free from the burdens that now oppress them, of assisting in the production of studies that will conform to ideals of accuracy and truth. To assist in carrying out this purpose UNESCO proposes to establish a World Library Center in Paris, the facilities of which will be available to scholars everywhere.

Thus, as Mr. David Hardman points out in his introduction to the present volume, we may look for an improvement in textbooks in history, geography, and civics. Instead of six versions of a military campaign we will have "one authentic, documented account." This idea was further developed at the Mexico City meeting of UNESCO in 1947, where it was pointed out that many of the troubles of the world were due to the promulgation of bad philosophy. It was therefore proposed by an American delegate that the philosophers should resolve that henceforth only true philosophy should be written. This idea has a respectable ancestry, beginning with Plato's observations on the dangers inherent in permitting poets to propagate myths, and including the progressive Wisconsin statute passed in 1923 which forbade the use in that state's schools of any textbook which falsified the facts with respect to the War of Independence or the War of 1812, or defamed the nation's founders, or misrepresented the ideals and causes for which they struggled, or which contained propaganda favorable to any foreign government.

Many of the speakers represented in the present volume were not members of UNESCO, and were thus free to expound their ideas without reference to UNESCO's program. M. Malraux, a de Gaullist, put forward a theory of art which aroused the utmost scorn of M. Aragon, who has identified himself with the Marxist point of view. Mr. Ayer, a leading nominalist, explained why all past and present realist and idealist philosophy is in error, and Mr. Herbert Read, also a nominalist, showed why all past aesthetic interpretation must be discarded. For the most part the scientists contented themselves with brief, excellent accounts of progress in our knowledge of such fields as the submarine underworld, the physiology of the nervous system, cave paintings, and genetics. The volume also contains a moving plea by the Greek representative on behalf of the claims of ancient Greek rationalism to a place in the deliberations of UNESCO.

HUNTINGTON CAIRNS
National Gallery of Art
Washington, D. C.

BOTANICAL EXPLORATIONS IN THE FIJIS

Naturalist's South Pacific Expedition: Fiji. Otto Degener. [8] + 303 pp. Illus. $5.00. Otto Degener. Waialua, Oahu, Hawaii.

OTTO DEGENER presents a curious mélange of his experiences in collecting plants in the Fiji Islands in 1940–41, with accounts of the past and present customs of the natives of these islands and their present condition under colonial British rule. His botanical collecting in the Fijis was under the auspices of the New York Botanical Garden and of the Arnold Arboretum and appears to have been eminently successful. (One is intrigued by the somewhat obscure arrangements whereby it seems that he is to inherit the junk-yacht *Cheng Ho* from its former owner, Mrs. Anne Archbold, the sponsor of the Cheng Ho Expeditions.)

The account of botanical collecting gives a thoroughly interesting picture of the work of an exploring and collecting botanist in the tropics, which is sometimes a little overtechnical or lacking in explanations for the nonbotanical reader. There are glimpses of shell collecting and of other zoological interests, but these are unfortunately brief. There were two collecting stations on Vanua Levu, and several on Viti Leu, including the slopes of Mount Evans. The results included the discovery of a remarkable new family of plants, the Degeneraceae, about which Mr. Degener is, perhaps pardonably, somewhat naïvely vain.

The anthropological information about the Fijians of the last century is a well-written review of the source material. Various accounts of present-day Fijian customs, like the drinking of *yangona,* the women's "sitting dance," and the methods of house construction are at firsthand. The information about medicinal and food plants is valuable.

The accounts of race prejudice, "white supremacy," and other failures of the British colonial rule seem to be painfully firsthand. Nevertheless, the status of the Fijians in the all-Fijian communities seems to be not without dignity and independence, and the amount of racial intermixture remarkably small. One may wonder what may be the impact of the Fijians' distinguished war service on the three-way race problem presented in the relations of whites, Hindus, and native Fijians in their island isolation.

KARL P. SCHMIDT
Chicago Natural History Museum

ATOMIC ENERGY IN PEACETIME

Radioactive Tracer Techniques. Geo. K. Schweitzer and Ira B. Whitney. vi + 241 pp. Illus. $3.25. Van Nostrand. New York.

THIS is the first laboratory manual on radioactive tracer methods the reviewer has had an opportunity to examine. In general, it seems to be a satisfactory text, but it does leave a great deal of material to be derived from the lecture course. The first four chapters are devoted to general consideration of health hazards, laboratory construction and conduct, shielding, and such matters, which are of considerable importance in orienting a beginner, but the authors have failed to include quantitative data which would add much to its value. For example, the magnitude of natural radiation exposure and data on stopping power of various shielding materials would be of interest. The novice would be helped by the inclusion of a few tables of pertinent data.

The hazard of experiment contamination merits more attention than is given in this text—the exposition of the concept of contamination potential is left to the instructor. A problem or two of this nature would increase the worker's respect for rules and regulations.

The discussion of the experiments is clear, although perhaps others might have been selected. Putting a beginner to work measuring zinc, for example, is giving him a tough assignment.

Surgical gloves and their proper use are of manifest importance, but other gloves, heavier ones in particular, have their place and are too generally overlooked.

Constructive Uses of Atomic Energy. S. C. Rothman, Ed., ix + 258 pp. Illus. $3.00. Harper. New York.

THIS volume is intended, say the publishers and editor, for the intelligent layman; its purpose is to assure him that really constructive uses of atomic energy are being actively developed. The papers chosen range from aircraft engines and atomic power for industry through the fields of biology, ceramics, and medicine to soil research, with an impressive array of authors.

The first chapter is based on Dr. A. H. Compton's Franklin Medal Lecture, which merits a greater audience than is reached by the *Proceedings* of the American Philosophical Society. Dr. Compton discusses the problem of establishing international order and, in general terms, the developments to be expected and problems to be met in the peaceful utilization of this new source of power. Although Dr. Compton might be less optimistic today (the lecture was given in November 1945) of the effectiveness of the release of atomic energy in "compelling man to become more human," his presentation of the case deserves thoughtful reading.

Dr. L. W. Chubb, in the second chapter, gives an excellent account of the development of concepts of atomic and nuclear structure from Democritus, 400 B. C., to the present.

A brief chapter by S. K. Allison serves to introduce the eleven chapters on specific peacetime applications of nuclear energy and radioactive by-products.

Chapters IV and VI, dealing with the industrial applications of, and chemical process control with, radioactive materials, will interest many readers familiar with instrumentation and control problems of industry. The fifth chapter, entitled Atomic Power for Industry, which gives a very readable account of the present status of power reactor development, as of July 1948, and Chapter VIII, on Atomic Engines for Aircraft, will clarify many a layman's idea of just what atomic power means in practice.

Most readers will probably find the papers on Radioactive Materials in Soil-Fertilizer Research, The Use of Tracers in Biology, The Medical Uses of Atomic Energy, and Radioactive Tracer Techniques in Pharmaceutical Research the most interesting and understandable, these chapters being written, for the most part, with considerable skill in nontechnical definition of the problem and the mode of attack.

The chapters on Radioactive Tracers in Metallurgical Research and Ceramics and Nucleonics, although interesting, are rather heavy going for the uninitiated.

The text closes with a brief (4-page) chapter on Nuclear Physics and Medical Research.

The book has two appendices that should be of particular interest to the nonscientific reader. The first is Chronological List of Highlights, the second A Glossary of Terms Used in Atomic Energy.

The printing and format are good, and illustrations, photographs, and sketches interesting and well captioned.

M. E. JEFFERSON
Southern Regional Research Laboratory
New Orleans

CANADIAN SCIENCE

A History of Chemistry in Canada. Compiled by C. J. S. Warrington and R. V. V. Nichols. x + 502 pp. Illus. $4.50. Pitman. Toronto.

IN A little less than half a century, Canada has changed over from a predominantly agricultural country to one of the major industrial countries of the world. With the wealth of her natural resources, the impact of chemistry on the growth of Canadian industry has been outstanding, and the book under review is a faithful record of what has taken place during and before this period. Compiled from the data supplied by specialists in industry, the universities, and the public service, the text is more than a rearrangement of the material submitted by various sources. The sequence of the chapters is such that there is a remarkable continuity, leading from the chemistry of metals, mineral products, coal, and petro-

leum to that of the organic substances derived from wood, agriculture, and fisheries, and, finally, the synthetic products derived from coal, air, and water. The last chapters are devoted to the public services, research institutions, chemical organizations, journals, and the teaching of chemistry. This arrangement provides an entertaining story of the development of chemistry in Canada that can be read by the layman as well as by the chemist. Descriptions of processes are particularly well written, and due credit is given everywhere to the individuals, teams, and companies who devised such processes. The book is well illustrated with half tones, line drawings, and maps.

Chemical research, mostly applied, is noted throughout the book, but there is no special chapter devoted solely to research in pure science. Only brief mention is made of the main lines of research in which notable advances have been made. This would lead one to believe that, had the authors tried to describe such work, the size of the book would have been considerably greater. Let us hope that, someday, a comprehensive story of this work will be written with the same objectivity and the same degree of accuracy found in this fine contribution to the history of chemistry in North America. The authors, the Chemical Institute of Canada, and the donor company (Canadian Industries Limited) should be congratulated on this successful effort.

Léon Lortie

Université de Montréal

ENTOMOLOGY

Traité de Zoologie. Anatomie, Systématique, Biologie. Tome IX: *Insectes. Paléontologie, Géonémie, Insectes Inférieurs, Coléoptères.* Pierre-P. Grassé, Ed. 1,117 pp. Illus. 4,500 fr. Libraires de L'Académie de Médecine. Paris.

THIS is the first to appear, the second in contemplated sequence, of three volumes on insects of the *Traité de Zoologie,* which will eventually comprise seventeen volumes and cover the entire animal kingdom. The work calls to mind the ambitious efforts of Cuvier and Buffon.

The present volume discusses the geographical distribution of insects, the classification of fossil insects, the minor orders of insects, and one major order, the Coleoptera. The authors of this part, Chopard, Denis, Despax, Jeannel, Paulian, and Grassé, (the editor for the entire section on entomology), are all names well known in entomological literature.

Jeannel's opening section on the classification and phylogeny of fossil insects is a concise summary of the present knowledge on the subject. It is well documented and copiously illustrated. This is followed by the same author's discussion of the geographical distribution and evolution of insects, which is a condensation of the views set forth in his separate work, *La Genèse des Faunes Terrestre,* and is in turn based upon the theories of Wegener.

The treatment of each order or group of related orders is by a French specialist in the group. The plan followed under each consists of a general diagnosis of the group, a comprehensive description of the forms included, an extensive section on the morphology, and a section on the systematics, which covers all the families of the order, as recognized by the worker writing the part. Each part is illustrated with an abundance of figures of the particular structures and of the gross habitus of numerous species of insects. The excellent illustrations, many published for the first time, are one of the really outstanding features of the work.

Some of the associations and divisions, as well as the names used, will not be familiar to all workers, but the essence of the system used may be found by reference to the *Table des Matieres* on pages 1110–17. The reasons for the arrangement and use of names will generally be found in the discussions and by checking the papers cited in the bibliographies.

Each section is followed by a rather extended bibliography, which contains many of the titles of the most recent papers that have appeared. These bibliographies will prove as useful as the many illustrations.

Grassé's discussion of the termites is quite extended and illuminating, covering approximately 140 pages. When one stops to consider the importance of the group from an economic standpoint, as well as from the standpoint of their interesting social habits, this is not an overemphasis.

The treatment of the Coleoptera by Jeannel and Paulian occupies about one third of the volume. The number of recognized families is greater than in any American work, but higher categories in taxonomy are a matter of personal preference. Their division of the order into suborders and superfamilies invites consideration by American workers.

The other large complex treated in this work is the Orthopteroids. The inclusion of the Plecoptera and Embidina in this superorder seems strange in view of the rather evident relationships both have with the Blattaria.

The book will have definite value as a reference work for the teaching entomologist, and it will be highly useful to the general systematic worker as a resumé of the current ideas of the French specialists involved in the undertaking.

John G. Franclemont

Arlington, Virginia

BRIEFLY REVIEWED

Our Sun. Donald H. Menzel. vii +326 pp. Illus. $4.50. Blakiston. Philadelphia.

THIS is the eighth and latest in the series called "The Harvard Books on Astronomy," written for the amateur astronomer and others interested in the

latest theories of astronomy. Moving pictures of the sun taken through special filters have been studied by Dr. Menzel and his associates, resulting in revised solar theories through an increasing knowledge of the sun. The book makes use of a new term: "spectroheliokinematography." Not only is the word new since 1938, but the use of the instrument, which is a moving-picture technique applied to the older spectroheliograph, illustrates the difference between methods used for a study of the sun's disk only eleven years ago and at the present time.

The book will be more interesting if the reader has seen the 16-mm film *Explosions on the Sun,* made at the Climax, Colorado, station of the Harvard Observatory, or similar films made at the University of Michigan. He will then understand much better the discussion of solar prominences in Chapter 8. The reader will also be intrigued by a review of the latest theories of sunspots as given in Chapter 6. In fact, he may disagree so completely that he may want to formulate his own theory.

The book is fascinating reading, with excellent illustrations and rather thorough descriptions of solar phenomena, but it is not always too easy to understand. No attempt is made to discuss completely the mathematical and physical theories that the expert will demand.

The book is a *must* for all amateur astronomers. It is to be hoped that it will inspire the amateur with a telescope to make systematic sunspot observations and to try to correlate his observations with observations of aurora displays and radio fadeouts.

C. M. Huffer

Washburn Observatory
Madison, Wisconsin

The Ways of a Mud Dauber. George D. Shafer. xii + 78 pp. Illus. $2.50. Stanford University Press.

THE author, a retired professor of Stanford University's Department of Physiology, became intrigued with some white pellets that were visible through the body wall of the larvae of a wasp. He wanted to know about these and set about with scientific acumen to find out what they were and why. They were really uric acid crystals, as he learned in a short time, but he became so engrossed with wasps that he devoted five years to an intensive study of their physiology, their metamorphoses, and their habits.

The result is an exhaustive, but not exhausting, life history of the same mud daubers. Combined with a flair for ascertaining facts, Professor Shafer developed a sympthetic and affectionate regard for some of the objects of his studies. One of them, Crumple-Wing, "a pot marred in the making," had a useless wing. The professor actually tried to replace this by cementing the wing of another wasp in place of the abortive one. This did not work, but Crumple-Wing continued her life as normally as she could with this handicap, and when she finally died the professor dedicated his book to her.

It is interesting reading, both for the layman and for the entomologist. Perhaps some other students will find out more about the life of these insects than Shafer did, but it will take them a long time and a great deal of careful observation. I enjoyed the book.

W. M. Mann

National Zoological Park
Washington, D. C.

The Epitome of Andreas Vesalius. L. R. Lind, Translator. xiv + 133. Illus. $7.50. Macmillan. New York.

FOUR centuries ago, Andreas Vesalius of Brussels fathered the science of modern human anatomy. From the work of this great pioneer observer stems much of modern objective biological science. Vesalius' major work, *De Humani Corporis Fabrica* (1543), is immense; his *Epitome* is a masterly condensation of a vast amount of material. Dr. Lind's translation is faithful to the original Latin, yet uses modern technical phraseology. In addition to the complete translation, beautiful plates give us the complete Latin text, and all the meticulous woodcuts of the original.

The volume is beautifully printed, the illustrations are excellent, and the clear, precise translation is a pleasure to read. For all those interested in the history of the development of science, medicine, and, particularly, human anatomy, this first complete translation of the work of the famous master is a true treasure.

Edward J. Stieglitz

Washington, D. C.

CORRESPONDENCE

SENSE AND NONSENSE

That the man who wrote with such charm and good humor on *The Lungfish, the Dodo, and the Unicorn* failed to respond to the charm and good humor of Matt Kahn's drawings in your May issue is in itself arresting. Beyond that, however, we are reminded of the continuing need for a zoology and biology of the scientific temperament.

Perhaps when the breeding, feeding, and organizational habits of scientists have become "textbook stuff" we will have a differential physiology for those who think *The Conquest of Space* nonsense and those who think "glamour girls in four colors" nonsense. Only then would it be possible for Willy Ley to write, as only he could write, a study of mythical, rare, and extinct types of scientists.

With apologies to him a sample item might read . . .

> To this day there are occasional reports scattered over America of a rare type of scientist who takes an interest in art. Typical is the example cited by an observer in Manhattan:
>
> "Shortly after my publication of *The Application of Boolian Functions to the Sex Life of Economists* (Brown and Bigelow, $4.00), I chanced to be in an art museum in upper Manhattan. I encountered one of the strangest things I have seen. A man and woman entered the room where I sat ruminating. I recognized the male at once as a Ph.D. in organic chemistry I had often seen on the campus of a nearby university. As near as I could judge, this specimen was of the somatotype 1–3–6, quite tall and aggressive in stance.
>
> "The two stopped before a Picasso oil—'The cube of a nude,' to be exact. I distinctly heard him say, with the bleating tones these specimens often use in the presence of the female, 'I have always loved this painting.'
>
> "My astonishment was so great that I had no thought of capturing the specimen. Later when I sought him out on the university campus I found that he had migrated elsewhere.
>
> "This incident left no doubt in my mind that there exists a very rare type of organic chemist with the somatotype 1–3–6 who likes Picasso."

As a subscriber I enjoyed Matt Kahn's drawings as much as Willy Ley's book.

WYMAN GUIN

Lakeside Laboratories, Inc.
Milwaukee, Wisconsin

ERNIE AND PHYSICS

Whenever Ernie leans against a wall
He thinks of how he holds it up as much
As it holds him, and, suddenly grown tall,
Confronts the world without a mental crutch.

FRANCIS BARRY

Philadelphia

PRAISE AND BLAME

The September number of THE SCIENTIFIC MONTHLY has so far failed to reach me, although I notice it is out. I would appreciate it if you could trace this copy or mail a duplicate to me. I should hate to miss it, as I find it of extraordinary interest, combining as it does so skillfully, presentation in terms accessible to those outside the special fields covered, of articles reporting the latest scientific developments. I enjoy, too, perhaps for mildly "escapist" reasons, such accounts as "The Hummingbirds' Brook," in the December 1946 issue, and "A Botanist's Dominican Diary" in March and April 1944.

H. P. DUTTON

Evanston, Illinois

"New Permanent Antifreeze" (*Sci. Mon.*, 1949, 69, (4), iv). I object to tripe like the above in a serious scientific publication—unworthy of a Hearst newspaper. And it is not the first time this column has made poor selections.

ARTHUR BARRY

Greenville, Delaware

BACK NUMBERS

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DECEMBER 1949

ISOTOPES—THEIR DISTRIBUTION AND USE

PAUL C. AEBERSOLD

From 1938 to 1942 Dr. Aebersold (Ph.D., California, 1938), under the direction of Dr. Ernest O. Lawrence, inventor of the cyclotron, was associated with physics and biophysics research at the Radiation Laboratory, University of California. During the war he worked on various phases of the atomic energy project in Berkeley, California, Oak Ridge, Tennessee, and Los Alamos, New Mexico. In 1946 he became chief of the Isotopes Branch of the Manhattan District and is now chief of the Isotopes Division of the Atomic Energy Commission, which supplies isotopes to hundreds of research institutions, universities, and hospitals all over the world.

IN THE first three years of the United States program of isotope distribution, from August 2, 1946, to June 30, 1949, more than 7,000 shipments of radioactive isotopes and 731 shipments of concentrated stable isotopes were made to government, private, and industrial laboratories for use in peacetime research. All the shipments were made from facilities now controlled by the U. S. Atomic Energy Commission. The isotopes were distributed for use in all the major fields of scientific investigation, as well as for applications in medicine, agriculture, and industry. The radioactive isotopes went to 549 departments in 305 institutions located in 39 states, the District of Columbia, and Hawaii, and to 150 institutions in 21 foreign countries. The concentrated stable isotopes went to 209 departments in 144 institutions within the continental United States.

Radioisotopes were distributed for use as tracer atoms and as sources of ionizing radiations. Tracer atoms are used in all fields of basic and applied study in which knowledge is desired on the movement, transformation, and chemical behavior of atoms and molecules. As sources of ionizing radiation, radioisotopes are used in medicine for detecting and treating certain diseases, and in industry for radiographic testing and process-control applications. The stable isotopes were distributed for use as tracer atoms and as objects of nuclear study. As tracer atoms the stable isotopes were also widely used in all major fields, and as objects of investigation per se they were mainly used by nuclear physicists and chemists.

What does a program, already grown to such magnitude and still growing rapidly, mean to the over-all advancement of modern science? Is government control of isotope distribution a handicap to their maximum usefulness? What precautionary steps have been taken with respect to the health-hazard aspects of radioisotope distribution? What are the security risks, if any, in making distribution to laboratories in foreign countries of these materials, many of which are direct by-products of atomic energy? Is the use of isotopes a passing "fad"? Presentation of this summary on the status of isotope distribution and utilization is intended to help provide an answer to these general questions, as well as a better over-all understanding of the program.

Development of distribution program. The use of naturally occurring radioisotopes, such as radiolead and radium, as radioactive indicators, or "tracer" atoms, dates back to the late 1910s. It was not until the middle 1930s, however, that the artificial induction of radioactivity permitted radioisotopes of the more common elements to come into usage as tracer atoms. Through the

medium of the cyclotron it became possible to produce radioactive species, or radioisotopes, of all the elements. With a few exceptions, one or more radioisotopes, having radiation characteristics suitable for tracer or other applications, could be produced of each element. There was, however, one great drawback to the widespread use of cyclotron-produced isotopes—the expense and small-scale capacity of production. Although radioisotopes were used appreciably in laboratory-scale research investigations and in certain applications for medical treatment, this work was chiefly limited to a dozen or so institutions possessing cyclotrons.

Before World War II those scientists who had used radioisotopes had formed a keen appreciation of their usefulness. They had found, for instance, that radioisotopes could be used to label uniquely a specific batch of atoms, which could then be traced through a labyrinth of chemical or physical reactions. The labeled atoms could be traced independently even in the presence of other atoms or molecules of the same substance, and in spite of multiple reactions of numerous other kinds of atoms and molecules. It was also found that radioactivity detection instruments could reveal the presence of extremely minute quantities of radioelements—quantities millions and sometimes even billions of times smaller than those detectable by chemical means.

Thus, even before the war, science had in radioisotopes a research tool with the twofold power of extremely high sensitivity and unique specificity. It was a tool greatly in demand but small in availability.

During the war many of the scientists who were familiar with cyclotrons and the handling of radioactivity became associated with the development of the atomic energy project. They soon realized that the uranium chain reactor would be an excellent unit for large-scale production of a wide variety of useful radioisotopes. At the conclusion of the war these scientists, realizing the potential value of radioisotopes to peacetime research, proposed that reactor-produced radioisotopes be made generally available for scientific investigation. Working jointly with the Manhattan District, Corps of Engineers, U. S. Army, the original operators of the atomic energy project, they formulated a program based on this proposal. The present isotope distribution program is a direct outgrowth of that early planning. Although the spadework connected with establishing the program was mainly completed at that time, most of the responsibility for its administration has come under the jurisdiction of the Atomic Energy Commission.

Increased availablity of isotopes. To appreciate the capacity of a nuclear reactor for radioisotope production, one has only to compare it with a cyclotron. Production of the most widely useful isotopes in both units is based primarily on inducing radioactivity into stable target elements. In the reactor the target elements are bombarded only with the neutrons of the chain reaction, whereas in a cyclotron much more versatility is possible in the type and energy of the bombarding particles. In the nuclear reactor there is also a vast source of radioisotopes in the radioactive fragments resulting from fissioning, or splitting, of the uranium employed in the chain reaction. These are the so-called fission products.

Because of the extremely high density of neutrons (approximately 5×10^{11} neutrons/cm^2/sec.) available in the reactor for bombarding target elements, and because of the over-all size of the reactor, it is possible to produce thousands to many millions of times as much radioactivity as in a cyclotron. Also, because of the reactor size, it is possible simultaneously to induce radioactivity in hundreds of different target materials. Although there are a number of very useful radioisotopes that cannot be produced by neutron bombardment, thus requiring cyclotron production, most of the widely useful isotopes can be produced in a reactor.

With reactor production facilities, distribution would no longer have to be limited to a relatively small number of investigators, and research workers would not have to limit their investigations to those experiments requiring minimum quantities of tracer materials. Production would be sufficient for investigators to trace not only elements and simple inorganic compounds but also complex organic compounds and biological materials, even though the synthesis of such labeled materials would be inefficient in the use of active material.

The development of stable isotope separation and utilization has followed a similar pattern. Prior to the war, stable isotope separation was accomplished by electrolysis, exchange reactions, and fractional distillation on a very small scale. Deuterium, or hydrogen 2, carbon 13, and nitrogen 15 were the only stable isotopes, which were concentrated in quantities sufficient for significant tracer use. Stable isotopes of many elements can now be concentrated by large-scale electromagnetic separators similar to those used during the war for the mass-scale separation of fissionable uranium 235 from

nonfissionable uranium 238. Approximately 130 different concentrated stable isotopes are now available to investigators in this country.

Stable isotopes are generally not as applicable as radioactive isotopes for tracer investigations because they can only be identified on the basis of differences in weight. This determination requires costly instrumentation and is much less sensitive than the methods used in radioactivity measurements. Stable isotopes have, however, proved very valuable as tracers in investigations where it is not permissible to introduce radioactivity, or where great sensitivity is not required. They are, of course, also invaluable for fundamental nuclear studies requiring isolated nuclear species.

Criteria of production and distribution. Before the isotopes distribution program could be initiated, certain criteria for both production and distribution had to be established. This was especially true for radioisotopes because of the variety of health-safety problems involved. In distributing stable isotopes, on the other hand, the principal problems were concerned with limited availability and cost. Answers had to be found for such questions as: What radioisotopes should be produced and in what quantity? In what chemical form should the isotopes be made available? To whom should isotopic materials be made available? What portion of the production costs should be borne by the prospective user? What, if any, limitations should be placed on the applicant?

A number of such questions were automatically answered by the Atomic Energy Act of 1946. Answers to others have been tentatively determined by the Atomic Energy Commission and its advisory groups in the best interest of the distributor and the isotope user. In some instances, such as in price scheduling and in procurement procedures, it has been necessary to make adjustments from time to time to meet changes in availability and demand.

To assist in the formulation of policies under which isotopes would be distributed, the Commission appointed a Committee on Isotope Distribution. This Committee, composed of twelve members, scientists and physicians who have had considerable experience in the handling and use of radioactivity, is subdivided into a Subcommittee on General Applications and a Subcommittee on Human Applications. Besides making recommendations to the Commission on basic policies governing allocation and distribution, the Subcommittee on Human Applications reviews all requests proposing to use radioisotopes in human beings, and the Subcommittee on General Applications reviews requests proposing to use large quantities of activity in experimentation outside the laboratory.

Because radioactive materials are potentially hazardous to health, it is necessary that the prospective user have adequate facilities, proper safety equipment, and specialized scientific background to insure their safe handling and use. It is also necessary to know what use will be made of the radioisotopes. These requirements are set forth in the Atomic Energy Act. The Atomic Energy Commission and its advisers have given these directives as liberal interpretations as was deemed feasible. The Commission has, for instance, adopted the philosophy that it is better to expend effort and money on educating isotope users in proper techniques than on attempting to police the health-safety aspects of isotope work. Although the policies may lean toward the conservative side, they have paid dividends in health safety. In more than three years of isotope distribution no case has been noted of injury to an individual using or handling radioisotopes.

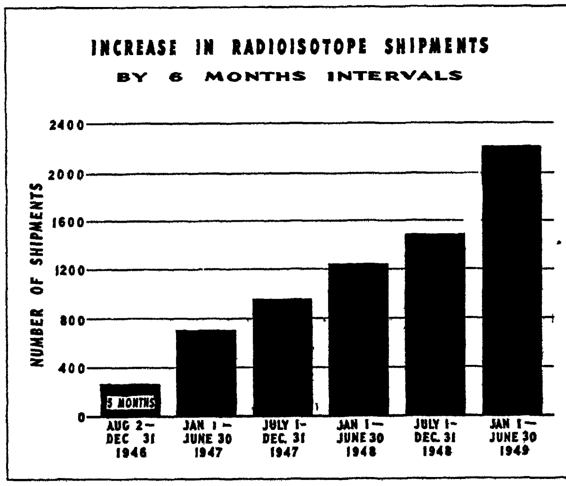

FIGURE 1.

The isotope user is allowed to obtain safety equipment of his own choice and to design laboratory facilities that best suit his own needs. The requirement that the applicant indicate his proposed use of the material is primarily a safety measure. He is only required to show that proper use of the material will be made in a manner that is both feasible and safe. This assures maximum safety to both the investigator and all others likely to be exposed to the radiation. Disclosures of usage which the applicant desires to keep confidential are closely confined within the Commission. It is required, however, in the interests of accelerating

the peacetime applications of atomic energy, that the user agree to publish the results of his use of isotopes within a reasonable time. The user is nevertheless at liberty to obtain patent rights in the usual manner to inventions resulting from his use of isotopes.

Production improvements. When the program first started, requests for isotopic materials warranted the immediate production of a wide variety of radioisotopes. The task of preparing approximately one hundred different varieties of radioisotopes made it necessary to limit their availability to the simplest and most generally adaptable chemical forms. But by placing the emphasis here, it was possible to make these "basic products" available cheaply and on a large scale. At first, production was limited because of the many new problems to be overcome. During the war the reactor laboratories were chiefly concerned with problems related to plutonium production. Little work had been done on developing production processes for those radioisotopes that would be most in demand for research, especially for use in biology and medicine. Initially, therefore, distribution of certain items had to be governed by a priority system outlined in the Atomic Energy Act. This provides that first priority be given to uses in fundamental research and medical therapy. In the past two years, however, the chief center of radioisotope production, the Oak Ridge National Laboratory, which is operated for the Commission by the Carbide and Carbon Chemicals Corporation, has markedly increased total production. Priority distribution is no longer necessary.

Improvements in quality production have also taken place as the program has grown. Considerable improvement has been made in the chemical processing procedures for separating radioisotopes which have been prepared by transmutation from the original target material. In this particular type of nuclear reaction the stable isotope of one element is converted into a radioactive form of another element, and the radioactive atoms may then be chemically separated from the stable atoms. Investigators receiving shipments of such separated radioisotopes can now be reasonably assured that the material will meet specifications of greater than 99 percent radiochemical purity. Although the production operators do not guarantee chemical and radiochemical purity, every effort is made to produce as high-quality material as possible. A continued effort has also been made to improve chemical specifications and the reliability of chemical analyses.

In at least one instance, quality improvement has been effected by entirely changing the method of production. Only within the past year has radioactive iodine 131 been extracted routinely as one of the products of uranium fission. Prior to that time this isotope was prepared by subjecting tellurium to neutron bombardment. The new production method not only gives better yields and permits the preparation of much greater quantities, but also guarantees higher quality. The chemical impurities in the stable tellurium are no longer a source of chemical contamination of the end product. More recently, the method of producing radioactive carbon 14 has been slightly altered. Although preparation is still based on the transmutation of stable nitrogen, the target material has been changed from calcium nitrate to beryllium nitride. This particular change has been made primarily in an effort to increase efficiency of production; the higher concentration of nitrogen in the nitride permits a better yield and a higher-activity product.

Closely associated with quality improvements are the advances that have been made in increasing the amount of radioactivity in the final product. Improvements in health-safety features of the production facilities now make it possible to prepare, handle, and ship much larger quantities of activity in a single radioisotope shipment. This is an important feature to investigators requiring larger quantities of activity either for large-scale tracer experiments or for high-intensity radiation sources.

Even more important than increased total activity is the recently announced availability of materials with increased specific activities, that is, with greater activity per unit weight of the element. Any radioisotope having a half life greater than sixty days can be appreciably increased in specific activity through extended time of irradiation in the nuclear reactor. Twenty radioisotopes with increased specific activities are now available. These include radioactive tantalum, whose specific activity has been increased from 105 millicuries to 1,000–3,000 millicuries per gram of tantalum; radioactive calcium 45, increased from 0.3 millicurie to 5–10 millicuries per gram; and radioactive cobalt 60, increased from 30 millicuries to 2,000–3,000 millicuries per gram. For radiocobalt, which has been suggested as a substitute for radium in certain applications in medical therapy and industrial radiography, the new material has a gamma ray output per unit weight approximately three times that of radium.

Continued improvements in the availability of the basic isotopic materials can be expected as a

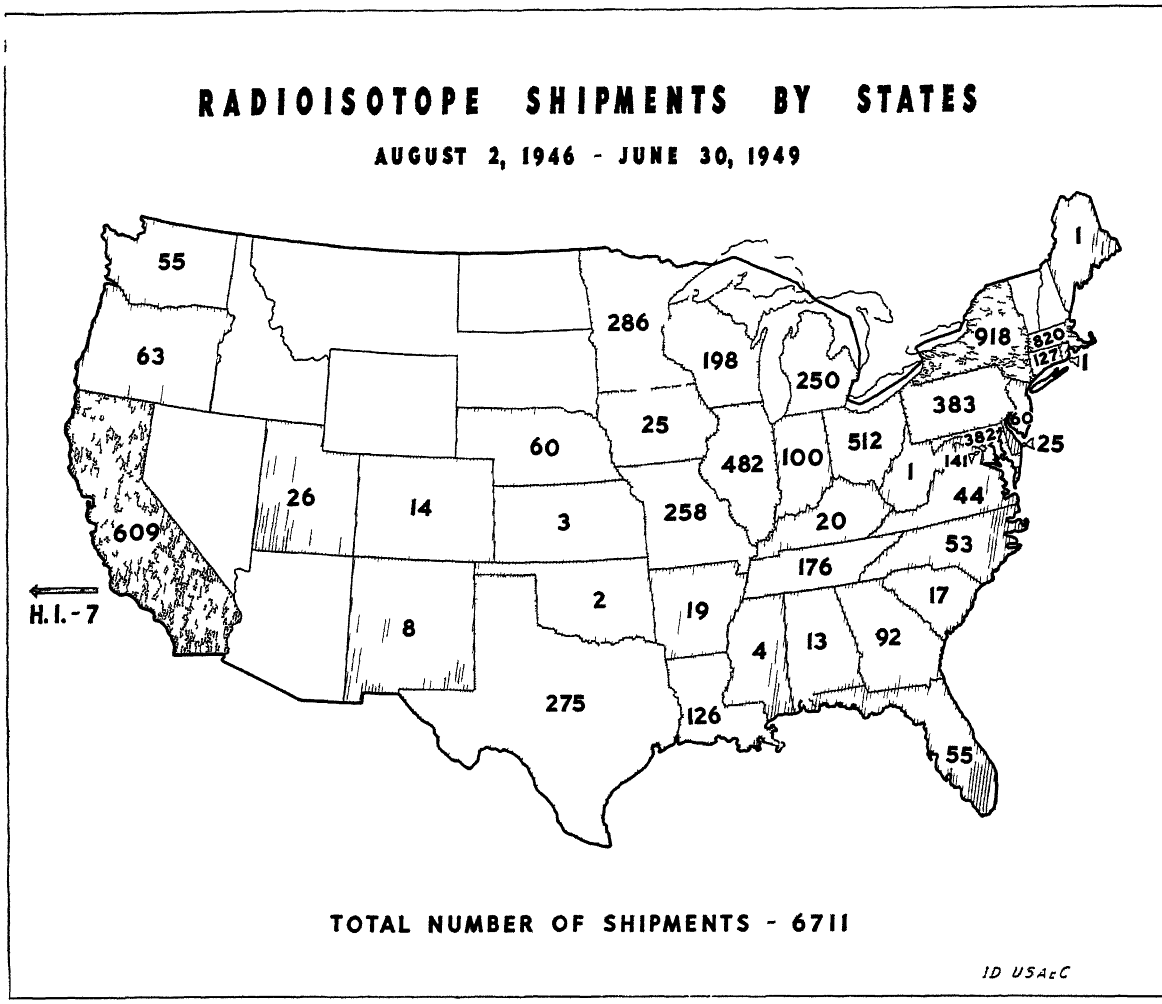

FIGURE 2

result of recently improved facilities. Until quite recently almost all the radioisotopes made available through the distribution program had to be prepared and processed in temporary facilities originally built as a pilot-plant for plutonium production units at Hanford, Washington. The Commission has just completed, at the Oak Ridge National Laboratory, a new building for chemical processing of radioisotopes at a cost of almost $2,000,000. With these new facilities it is expected that much improvement will be made in production, processing, and shipping methods.

Improvements have likewise been made in the availability of concentrated stable isotopes as the distribution program has expanded. During the past year helium 3, boron 10 in elemental form, boron 11 in compound form, and 21 electromagnetically concentrated isotopes of sulfur, barium, cerium, tungsten, and mercury have been added to the list of more than 110 stable isotopes formerly available. Workers associated with the electromagnetic separation program will continue to build up a reserve, or "bank," of a wide variety of concentrated stable isotopes from which samples may be withdrawn and loaned as required for research purposes.

Cyclotron-produced isotopes. To extend further the availability of useful isotopes the Commission announced in June of this year that it would also make available certain long-lived radiomaterials which cannot be produced in a nuclear reactor but must be made in a cyclotron. To assist in this phase of the program, arrangements have been made with cyclotron laboratories of Massachusetts Institute of Technology, the University of Pittsburgh, the University of California, and Washington University. Target materials will be bombarded at these locations and then forwarded to

the Oak Ridge National Laboratory for chemical extraction and distribution of the desired isotope to the users.

The cyclotron is the only source of at least 10 very useful radioisotopes. These include such important species as beryllium 7, sodium 22, iron 59 free of iron 55, iron 55 free of iron 59, and iodine 125. It is also the only source of very high specific activity radioisotopes of over 15 elements, including beryllium, fluorine, manganese, iron, nickel, and zinc. It is believed that making these materials available to isotope users at a reduced cost through a central distributing agent will result in extending or supplementing many important studies now under way with reactor-produced isotopes. For example, studies with short-lived sodium 24 (14.8-hour half life) on body fluid volumes and ion transport across blood vessel walls can be greatly extended with long-lived sodium 22 (3-year half life). Conversely, short-lived iron 59 (46.3 days) free of long-lived iron 55 (4 years) can be used more safely in clinical studies.

Isotope-labeled compounds. At the start of the distribution program, as pointed out earlier, it was decided to make available as wide a variety as possible of radioisotopes in simple inorganic chemical forms. To a large extent this procedure has been continued as the program has grown. For certain types of utilization these simple basic forms are immediately applicable with little or no further chemical processing. For example, radioiodine for studying and treating various thyroid gland disorders is usually administered as an aqueous solution of sodium iodide, and radiophosphorus for treating polycythemia vera is administered as an aqueous solution of phosphoric acid. In both instances, these are the chemical forms in which the isotopes are shipped from Oak Ridge. The processing necessary for administration is usually limited to sterilizing, aliquoting, and buffering the radioactive solution.

But for most research uses of radioisotopes, the basic forms cannot be directly used. A modified form, or even an especially synthesized compound containing the isotope, is required. To use a tracer isotope such as radiosulfur, for instance, in studying the metabolic fate of a sulfur-containing amino acid, it is first necessary to incorporate the radioisotope in a specified way into the compound to be followed. The isotope-labeled compounds may be prepared either by chemical or biological synthesis. Not only are such compounds essential for many research problems, but they have proved useful in certain medical applications, such as the use of radioiodine-labeled diiodofluorescein for locating brain tumors.

Preparation of isotope-labeled compounds is an activity which has been largely left to the resources of individual investigators and private enterprise. At present three private laboratories—Tracerlab, Inc., Boston, Abbott Laboratories, Chicago, and Texas Research Foundation, Renner, Texas—are preparing such compounds for distribution. Over 275 shipments of approximately 60 different compounds have been made from these laboratories to date, and the rate of such secondary distribution is rapidly approaching 50 shipments per month. These are in addition to the limited number of compounds prepared in Commission laboratories and obtainable directly from the Commission.

The need for labeled compounds, however, still outweighs their availability—not so much in quantity as in variety of compounds. Synthesis procedures are often difficult, time-consuming, and expensive. This factor limits availability and makes the cost excessively high. It is a handicap to both the distributor and the potential user and, also, often prohibits the preparation of a potentially important labeled compound.

Synthesis of labeled compounds. To make a greater variety of isotope-labeled compounds available, the Commission has arranged to assist financially the development work necessary for synthesis of selected isotope-labeled compounds. Contracts have recently been made with six private laboratories equipped with the necessary special facilities and personnel to participate in this program. Compounds scheduled for synthesis include amino acids, such as methionine and glutamic acid; organic acids and derivatives, such as fumaric acid and phthalic anhydride; hormones, such as testosterone and progesterone; important dyes, such as Nile blue and fluorescein; and miscellaneous compounds, such as vitamins, nitrogen mustard, and penicillin derivatives.

In recent months the preparation of isotope-labeled compounds by biological synthesis has aroused much interest among isotope users. More than 50 different varieties of such compounds have been extracted from plant and animal products after their synthesis from simple chemical forms by metabolic processes. These compounds, however, have been mostly prepared in small quantities as by-products of individual research investigations and are therefore not available for distribution and wide-scale use. With a view to producing such biologically synthesized compounds in quantities sufficient for more extensive use, Oak Ridge and Argonne National Laboratories have initiated isotope "farm" programs. A "farm" in this case is a laboratory especially equipped to handle the administration of radioisotopes to ani-

mals and plants and to extract the labeled materials. Compounds scheduled for synthesis under this program include such materials as glucose, fructose, ascorbic acid, nicotine, opium, and morphine.

Results of use. Indication of the wide usefulness of isotopes is evidenced by the rapid expansion in isotope distribution. Figure 1, for example, shows that the number of isotope shipments made during the first six months of this year was nearly double that for the same period last year and more than triple the 1947 figure. An illustration of overall distribution within the United States is given in Figure 2. Further evidence and a more objective criterion of the value of isotopes are to be found in the nearly 1,900 publications that have already resulted from Commission-produced isotopes. These publications are listed in the report *Isotopes—A Three Year Summary of United States Distribution,* recently released by the Atomic Energy Commission.* Further illustrations of the many ways in which isotopes have been used are found in the *Fourth* and *Sixth Semiannual Reports of the Atomic Energy Commission* to Congress.* The *Fourth Report,* issued in July 1948, was devoted almost entirely to isotope utilization in peacetime research programs outside Commission laboratories. One of its appendices consists of progress reports from individual research men, in which they outline typical experimental investigations and findings. The *Sixth Report,* issued in July 1949, contains many examples of isotope utilization in biology and medicine.

In 1948 a report on the utilization of isotopes distributed by the Commission could be made only on the basis of individual progress reports. Now that there has been time for many of the investigations to be completed, it is possible to cover the field with a bibliography of published papers.

It would not be possible here to discuss in detail the hundreds of uses of isotopes. A summary of isotope utilization amounts to a representative cross section of all the research and investigation in the wide variety of fields in which isotopes are used. In view of the diverse interests of the readers of this journal, it seems desirable to present at least a brief summary of some of the major uses of isotopes.

Use in biology and medicine. In biology isotopes have been used to label and study a large number of body constituents and related substances. Materials which have been labeled and traced through complex body processes include phospholipids, sugars, proteins, nucleic acids, vitamins, hormones, antibodies, amino acids, drugs, dyes, organic acids, blood cells, and others. Isotopes have been used to develop an entirely new technique for studying metabolism and the synthesis, transport, utilization, and breakdown of various body compounds.

To date only a limited number of applications have been found for radioisotopes in medical diagnosis and therapy. Unfortunately, publicity in regard to such applications has usually surpassed actual achievements, especially in the diagnosis and treatment of cancer. Certain diagnostic applications have, however, proved valuable; for example, the use of radiosodium for the differentiation of normal and restricted blood flow and for radiocardiography; radiophosphorus for determining the exact extent of a tumor mass during brain surgery; and radioiodine for preoperative location of certain brain tumors, for detecting disorders of thyroid gland function, and for locating thyroid cancer metastases. Diagnostic uses of radioisotopes are expected to increase, however, for two reasons: only small nonhazardous quantities of radioactivity are usually required, and an increasingly larger number of isotope-labeled compounds will become available. The manner in which some of these compounds are selectively absorbed by certain tissues, or are differentially metabolized, will no doubt prove to be of diagnostic value.

In medical therapy, radioisotopes have proved useful in relieving, though not curing, a limited number of disorders. Radiophosphorus has been used for treating polycythemia vera and chronic leukemia; radioiodine for treating hyperthyroid-

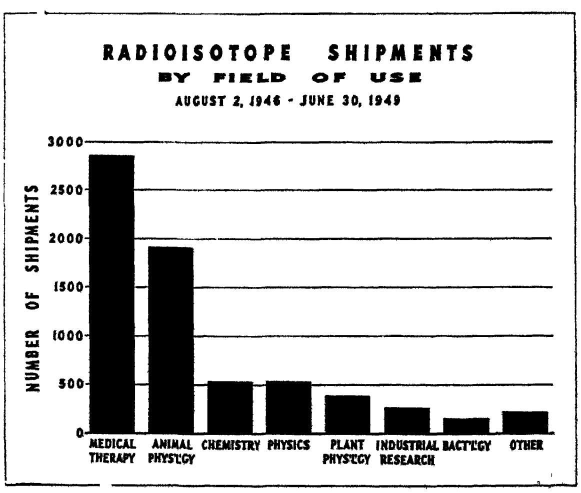

FIGURE 3

* These publications may be obtained from the Superintendent of Documents, Government Printing Office, Washington 25, D. C.

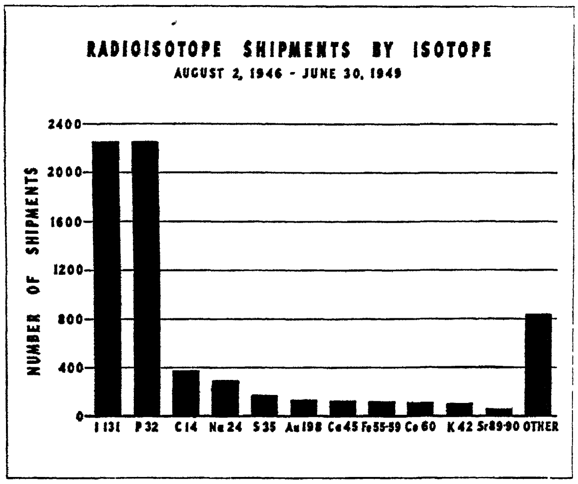

FIGURE 4.

ism and thyroid cancer metastases; radiogold for treating subsurface tumors of the lymphoid system and chronic leukemia; and radiocobalt as an interstitial source for treating accessible tumors. Also, very intense radiocobalt sources now available, equivalent in gamma ray output to over a pound of radium, should be valuable as teletherapy sources for treating deep-seated tumors.

It is possible also that isotope-labeled compounds may be used to a considerable extent to deliver radioisotopes and their accompanying ionizing radiation to specific tissues or organs. The use of such compounds will be limited to those into which the desired radioisotope can be incorporated and which will be selectively absorbed by the specific tissue or organ. Because of the much larger quantities of radioactivity needed in treatment, and the necessity for keeping total body irradiation at a minimum value, high selective absorption of the active material will be required.

As shown in Figure 3, a larger number of radioisotope shipments have been made for use in medical therapy (including diagnosis) than in any other field. The number of different projects utilizing radioisotopes in this field, however, is not as large as in some of the other fields. This is explained by the fact that the two isotopes most frequently used in medicine, radiophosphorus and radioiodine, have comparatively short half lives, and therefore several shipments may be made to complete a single investigation or case. A comparison of the number of radiophosphorus and radioiodine shipments with shipments of other radioisotopes is shown in Figure 4.

The potentialities of radioisotopes, together with their greatly increased availability, have stimulated an increasing interest in developing uses of these materials for the study, detection, and treatment of cancer. Because of this widespread interest, the Commission in April 1948 initiated a program for making available radiosodium, radiophosphorus, and radioiodine free of production costs to investigators proposing to use the materials in the study, diagnosis, or treatment of cancer and allied diseases. On February 25, 1949, this program was supplemented by making available free of production costs for use in cancer research all radioisotopes normally distributed from commission facilities. This phase of the distribution program has accounted for 2,059 shipments of isotopes of approximately 10 elements. Over 90 percent of these shipments have been of either radiophosphorus or radioiodine.

Use in plant physiology and agriculture. Isotope applications made thus far in the field of plant physiology fall largely into one of two categories, studies of photosynthesis and studies of phosphate fertilizer utilization. The first is a fundamental investigation into the mechanism of the process by which green plants, with the aid of sunlight, are able to convert water and carbon dioxide into complex sugars and other compounds. With radioactive carbon as a tracer, investigators have tried to determine the rate at which the process takes place and to identify the intermediate compounds formed. Although investigations are still incomplete, the tracer technique has enabled workers to identify some of the intermediates. Five compounds have been identified as having been formed within five seconds, and it also has been found that within ninety seconds at least 15 compounds are produced, including simple sugars containing 6 carbon atoms.

The studies on phosphate fertilizer uptake by various crops under different conditions of soil and climate are mostly being conducted by the United States Department of Agriculture in collaboration with state agricultural experiment stations. During the past growing season extensive field tests, using several different phosphate fertilizers labeled with radioactive phosphorus, have been carried out in more than 12 states on such crops as corn, potatoes, tobacco, sugar beets, alfalfa, oats, clover, rye grass, cotton, and peanuts.

Nutritional requirements and metabolism in plants of such elements as calcium, zinc, rubidium, and iron have also been studied with radiotracers. Metabolic disorders such as chlorosis and those caused by the parasitic action of fungi are readily studied by the tracer technique. Radioisotopes have also been used to study the action of plant growth regulators and the effects of radiation on plants.

Also of agricultural interest are radioisotope tracer studies in the field of animal husbandry. The tracer technique has proved to be particularly valuable in metabolism studies concerned with mineral requirements and nutritional deficiencies. Tracers have also been used in applied problems of entomology and pest control.

Use in physics and chemistry. Isotope applications in chemistry, like those in physics, are primarily of a fundamental nature. Tracer techniques have been used to study the mechanism and kinetics of exchange reactions, diffusion coefficients, oxidation, crystallization, and solubility. Chemists have also used tracers to determine the mechanism of many organic and biochemical reactions.

Physicists have used radioisotopes for studying nuclear characteristics such as disintegration schemes, the absolute energies of both beta and gamma radiation, beta ray spectra, nuclear spins, magnetic moments, and radiation absorption coefficients. They have also used radiomaterials to study and improve methods of radiation detection and dosimetry measurement.

Isotope studies in both chemistry and physics will undoubtedly have considerable influence on the scope and ultimate usefulness of isotopes in other fields of research. In physics, for instance, new knowledge of atomic nuclei is not only essential to the advance of atomic science,.but also basic to the utilization of isotopes in all fields of research.

Use in industry. Industry's use of isotopes has to date been limited mainly because of a lack of the necessary facilities and specially trained personnel in industrial laboratories. Nearly fifty industrial research laboratories, however, have initiated radioisotope research.

Tracer isotopes have, for instance, been used in studying the polymerization and vulcanization of rubber, the dehydrogenation and aromatization of petroleum, and the mechanism and catalysis of reactions such as those involved in the production of synthetic gasoline. Metallurgists have made extensive use of radioisotopes in studying steel-making reactions such as determining the mechanism and kinetics of slag-metal reactions, oxidation and crystallization, the molecular constitution of slag and metal baths, the diffusion coefficients of metals, the metallurgical activity of one element as influenced by the presence of other elements, and the concentration gradients in solid solutions. One of the most straightforward industrial investigations with radioisotopes has been the study of lubrication and wear phenomena associated with friction.

Several applications of the "gadget" or control type, such as radioactive thickness and height gauges, have been developed where the radiomaterial is used only as a source of ionizing radiation. Radioactive thickness gauges have been devised for the automatic gauging of paper, rubber, plastic, glass, and steel sheets and are already available on the commercial market from two different sources.

International distribution. The international distribution of radioisotopes by the United States is in keeping with the traditions of science. It also stems from the realization that, to assist other nations to help themselves attain better standards of health and living, they must be assisted in developing a vigorous activity in basic and applied science. Further, it must be recalled that in all fields American scientists are greatly indebted to the scientific achievements of scientists in other countries. In the basic discoveries of atomic energy this indebtedness is especially great. It is even more pronounced in the fields of radioactivity, nuclear transmutation, and isotope tracer techniques, wherein most of the "firsts" took place abroad.

Since radioisotopes are direct by-products of atomic energy, some concern might arise over the security aspects of their international distribution. Before distribution was undertaken, however, all conceivable security problems were given thorough and lengthy consideration by the Atomic Energy Commission and its advisory groups. The fact that radioisotopes were released for distribution in the United States without security restrictions, actually with the requirement to publish results, indicates that security is not a significant factor in their utilization.

Radioactive materials available for export are limited to 20 radioisotopes, selected because of their primary value for medical and biological applications. The radioisotopes are furnished only for fundamental scientific investigations and for medical research and therapy. All isotopes distributed —in fact, all radioisotopes—can be made with a cyclotron. Inasmuch as the major countries of Europe and some minor countries possess cyclotrons, radioisotopes are already available abroad. In fact, most of the requests received from abroad have been from groups who have previously used cyclotron-produced isotopes. The supplying of reactor-produced isotopes permits use by the many countries not possessing cyclotrons and makes possible use of the much larger quantities required for many medical and biological purposes.

Each country receiving isotopes agrees to re-

port semiannually to the U. S. Atomic Energy Commission on the progress of research done with the materials. In addition, each country agrees to permit qualified scientists, irrespective of nationality, to visit those laboratories where isotopes are being used. To date the following countries have made formal arrangements through diplomatic channels to the Department of State and have received radioisotope shipments: Argentina, Australia, Belgium, Brazil, Canada, Colombia, Denmark, Finland, France, Iceland, Italy, Netherlands, New Zealand, Norway, Peru, Spain, Sweden, Switzerland, Turkey, Union of South Africa, and the United Kingdom (Fig. 5). In addition, the following eight countries have completed the necessary arrangements, but have not placed orders: Chile, Cuba, Guatemala, India, Ireland, Lebanon, Mexico, and Uruguay. In the 22 recipient countries, the materials have been used in 150 institutions, many of which are world-renowned in the fields of research, education, and medicine. The impressive list of recipient institutions is published in the report *Isotopes—A Three Year Summary of United States Distribution,* referred to earlier.

It is expected that results obtained from foreign studies will provide valuable supplementary information to findings in the United States. For example, U. S. scientists investigating the possibility of using radioactive cobalt 60 as a substitute for radium in medical therapy have used the materials as interstitial needles and as teletherapy units. In Switzerland an investigator is making an entirely different approach. He first introduces a small rubber balloon by means of a catheter into the cavitary organ to be irradiated. The balloon is then filled *in situ* with the radioactive cobalt in solution. Upon completion of this work the advantages or disadvantages of the technique employed may be weighed against techniques being used in this country.

In Nigeria, British West Africa, radiostrontium has been used to tag the mosquitoes that are

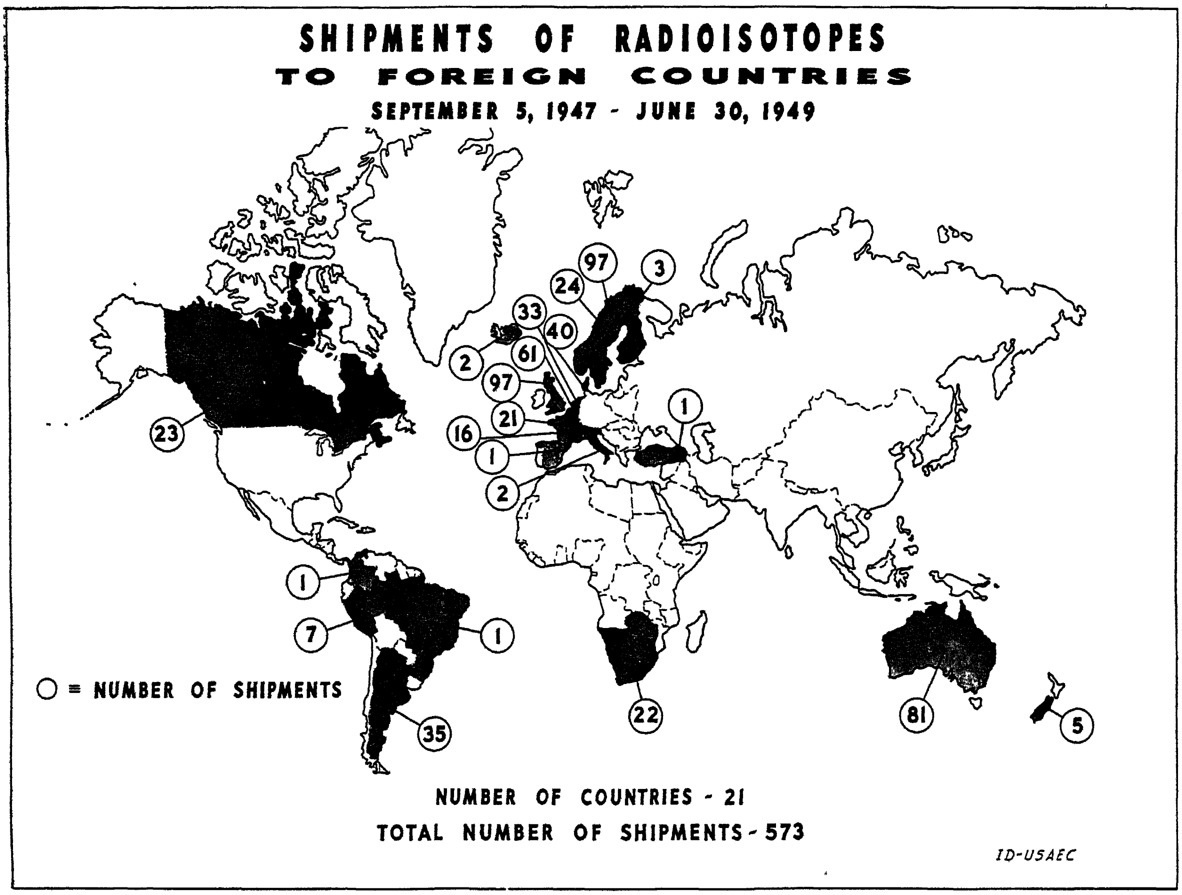

FIGURE 5.

carriers of yellow fever. The tagged mosquitoes are then used in studies designed to permit an estimation of the spread of yellow fever from a source of infection. In Australia, radiocobalt, radioiron, and radiozinc are being used to study the parasitic action of mistletoe on eucalyptus trees. Although the spread of yellow fever and the growth of eucalyptus trees are not serious problems in this country, the techniques used and the results obtained will certainly give valuable information to U. S. investigators using radiotracers in allied problems.

Compliance with the terms for obtaining isotopes has been excellent. Progress reports are submitted regularly, and the laboratories receive foreign visitors, including many from the United States. Good will and understanding have been evidenced by all foreign representatives dealt with in carrying out the program, and great appreciation has been shown by scientists and medical men who receive the isotopes. Although it is too early to expect extensive publication arising from the isotopes distributed, the results already reported are of high caliber and indicate that much value is certain to come from the researches conducted abroad.

Conclusions. The rapid growth in the isotopes distribution program may be attributed to a number of factors, including: (1) wider appreciation of the number and variety of potential isotope applications; (2) increased availability of isotopes and isotope-labeled compounds; (3) increased numbers of persons trained to handle and use radioactive materials; and (4) the AEC-sponsored program for supplying radioisotopes free of production costs for cancer research.

On the same bases, isotope utilization can be expected to increase in the future. A steady increase is expected as a result of both new types of research applications and new groups undertaking research with isotopes. A marked acceleration may take place if new wide-scale applications are developed for use in medical practice or industry. Future isotope utilization will also depend on the participation by private industry in various phases of the distribution program, for it is expected that the areas in which private enterprise can make valuable contributions will further expand.

The Commission will continue to encourage wider use of isotopes through its policies of extending the availability and usefulness of isotopes and isotopic materials, supporting educational programs in isotope techniques, and making available information on the feasibility and health-safety aspects of isotope utilization.

As tools of science and technology, isotopes will take their place with other long-established scientific instruments, and there should be no end to their growth in usefulness.

THE SCIENCE REPORTER

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THE SURGICAL TREATMENT OF CONGENITAL CYANOTIC HEART DISEASE

ALFRED BLALOCK, C. ROLLINS HANLON, and H. WILLIAM SCOTT, JR.

Dr. Blalock (M.D., Johns Hopkins, 1922), who, with Helen B. Taussig, developed the so-called blue baby operation, is professor of surgery and director, Department of Surgery, The Johns Hopkins University, and surgeon-in-chief of The Johns Hopkins Hospital. Drs. Hanlon (Johns Hopkins, 1938) and Scott (Harvard, 1941) are assistant professors of surgery at the Hopkins and surgeons in the Hospital.

An understanding of the recent developments in surgical treatment of congenitally cyanotic children ("blue babies") requires a knowledge of the normal and abnormal structure and function of the human circulatory system. The normal structure of the heart and blood vessels was known to Galen in A.D. 179, but it was not until 1628 that William Harvey described the circulation of the blood from the heart through the arteries of the body and back to the heart by way of the veins. In the nineteenth century physiologists showed that the dark venous blood returns to the right side of the heart and is pumped out through the pulmonary artery to the lungs, where it is oxygenated. This bright-red blood returns through the pulmonary veins to the left side of the heart and is pumped out through the aorta and its arterial branches to the tissues of the body.

The circulation through the normal heart is shown in Figure 1. The blood in the right auricle and right ventricle, having given up its oxygen in the tissues, is dark in color, whereas the left auricle and ventricle are filled with bright-red oxygenated blood returning from the lungs. The septum between right and left sides of the heart prevents mixing of the venous and arterial blood. Blood is propelled from the thin-walled auricles into the heavily muscled ventricles through one-way valves, which prevent regurgitation into the auricles when the ventricles contract. As a result, contraction of the ventricles drives the blood into the aorta and the pulmonary artery, where other one-way valves maintain the forward progress of the circulation.

For a better understanding of congenital circulatory derangements one must consider the fetal circulation (Fig. 2). Before birth the lungs are collapsed, and the fetus derives its oxygen from the mother. Oxygenated blood returns from the maternal tissues through the vein in the umbilical cord to the right auricle of the fetus. Some of this blood passes directly into the left auricle through an opening in the septum between the auricles, the "foramen ovale." The remainder passes into the right ventricle and is ejected into the pulmonary artery. Not all the blood in the pulmonary artery reaches the lungs, however; a large proportion of it is shunted through the "ductus arteriosus," which is a tubular connection between the pulmonary artery and the aorta. By reason of the flow through the foramen ovale and the ductus arteriosus, blood from both sides of the heart is pumped into the systemic circulation.

After expansion of the lungs at birth, there is an adjustment in the pressure relationships of the two sides of the heart. The foramen ovale closes, as does the ductus arteriosus. As a result all the blood in the right auricle passes into the right ventricle and thence to the lungs (Fig. 2). Thus the normal postnatal circulatory plan is established.

In some instances the ductus arteriosus fails to close. This leads to serious derangement of the

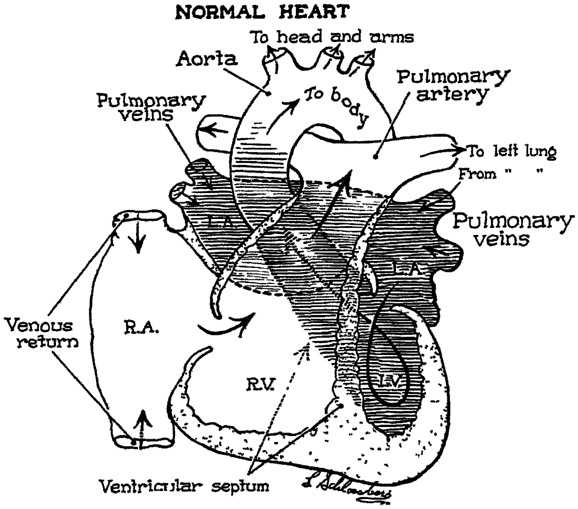


FIG. 1. Circulation through normal heart.

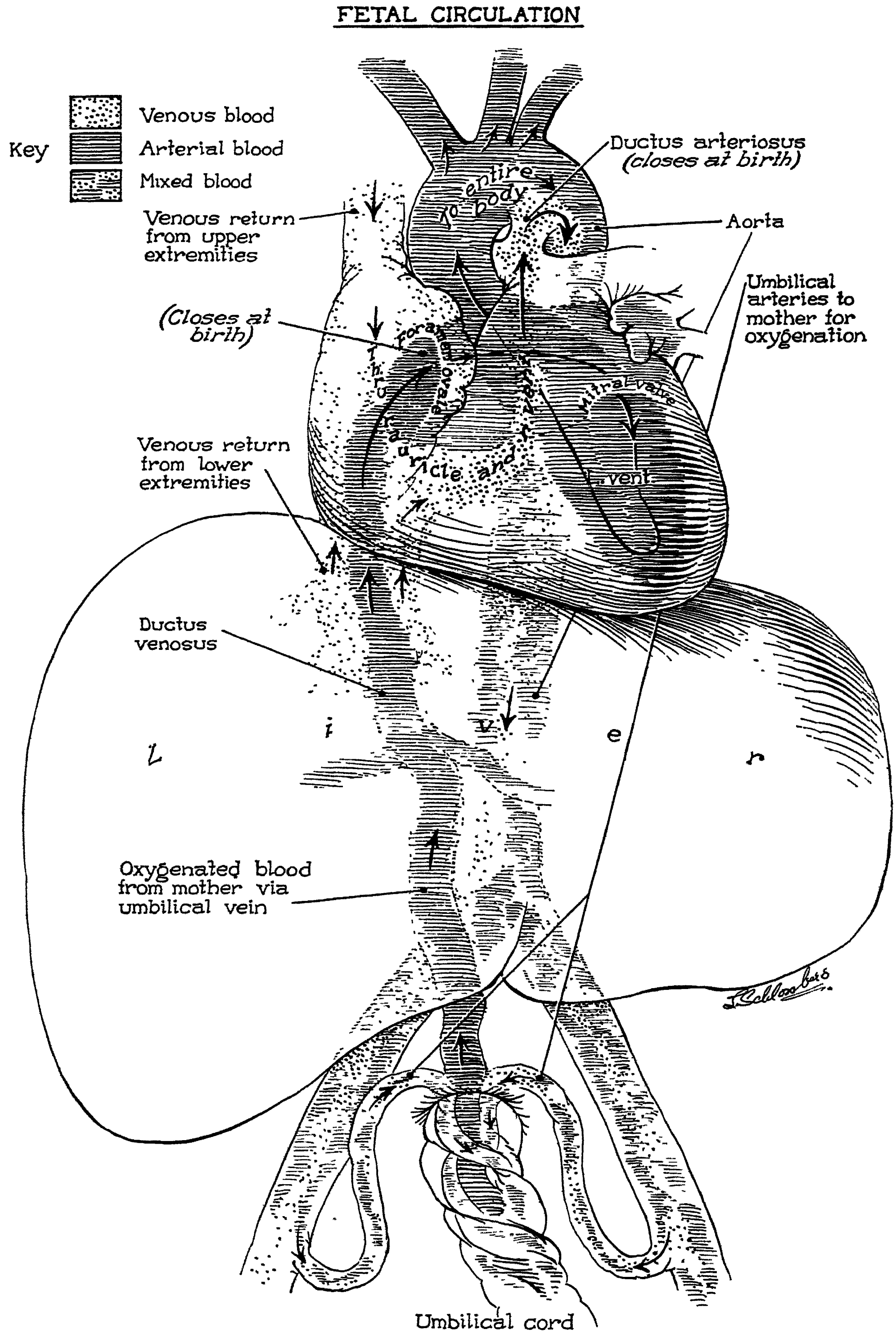


FIG. 2. Course of circulation in fetus. With closure of the foramen ovale and ductus arteriosus the normal adult circulation is established.

circulatory plan. The persistent ductus arteriosus represents a form of congenital heart disease in which the developmental error is late in occurrence; in contrast to this, most congenital abnormalties of the heart result from aberrations that occur early in embryonic life. Some of these are so severe as to be incompatible with extrauterine life, and others permit existence with handicaps of varying degree. The cause of these embryonic errors is not always clear, although it has recently been shown that certain virus infections in the mother during the early stages of pregnancy give rise to cardiac defects or to other abnormalities.

Congenital malformations of the heart may be divided into two groups, based on the presence or absence of cyanosis. Cyanosis (*kuavos*, "dark blue") is a blue or dusky color of the lips and skin due to the presence of an abnormally large amount of unoxygenated hemoglobin in the blood of the superficial vessels of the integument. There are normally about 15 grams of hemoglobin in 100 cubic centimeters of blood It has been shown that at least 5 grams of hemoglobin per 100 cubic centimeters of blood must be unoxygenated before cyanosis is apparent. Thus it is clear that cyanosis is a sign of severe oxygen deficiency.

As a result of the low oxygen saturation of the blood, cyanotic patients are frequently retarded in growth and development and are markedly limited in their physical activities. Many of them are unable to walk more than a few steps without great fatigue and shortness of breath. The body attempts to compensate for the chronic oxygen deficiency by producing an excessive number of red blood cells, a condition known as polycythemia. Whereas the normal person has 4.5–5.0 million red corpuscles per cubic millimeter of blood, cyanotic individuals may have 8 or even 10 million red blood cells per cubic millimeter. This makes the blood more viscid than normal. Another peculiar feature in these patients is the phenomenon of "clubbing" of the fingers and toes (Fig. 3). Although this bulbous thickening of the digits is characteristic of congenital cyanotic heart disease, it is not invariably present. Moreover, it may be seen in various other disturbances of pulmonary function that cause lowered oxygen saturation of the blood.

The structural defects in the heart that produce cyanosis are numerous. In most cases the great vessels arise from the ventricles in an abnormal manner. Frequently in these malformations only a small portion of the venous blood reaches the lungs, and a large volume of venous blood is pumped directly into the systemic circulation.

The most common malformation of the heart giving rise to cyanosis is called the tetralogy of Fallot. Fallot, a French pathologist, described the four cardinal features of the anomaly in 1888 (Fig. 4). On examining such a heart one finds narrowing, or stenosis, of the pulmonic valve or of that part of the right ventricle leading into the pulmonary artery. The base of the aorta lies farther toward the right side of the heart than normal, so that it overrides the septum between the right and left ventricular chambers. Furthermore, there is an abnormal opening high in this septum between the ventricles near the origin of the aorta. And, finally, there is thickening of the right ventricular muscle as a result of the strain imposed by the resistance of the narrow valve to the pumping action of the ventricle.

It may readily be seen from the diagram (Fig. 4) why patients with Fallot's tetralogy have cyanosis. Because of the narrow pulmonic valve, only a small proportion of the blood in the right ventricle can pass into the pulmonary artery to be oxygenated in the lungs. The remainder is pumped into the aorta, either directly from the right ventricle or by way of the interventricular septal defect into the left ventricle and thence into the aorta. Thus there are at least two factors operating to cause cyanosis in these patients—one being inability to propel a normal volume of blood into the lungs for oxygenation, and the other the passage of a large volume of dark venous blood from the right ventricle into the left ventricle and aorta without its being oxygenated in the lungs.

As has been mentioned, children with cyanosis are greatly restricted in their activity; moreover,

FIG. 3. Hands of patient with congenital cyanotic heart disease (tetralogy of Fallot). The "clubbing" of the fingers may disappear after operation has relieved the cyanosis.

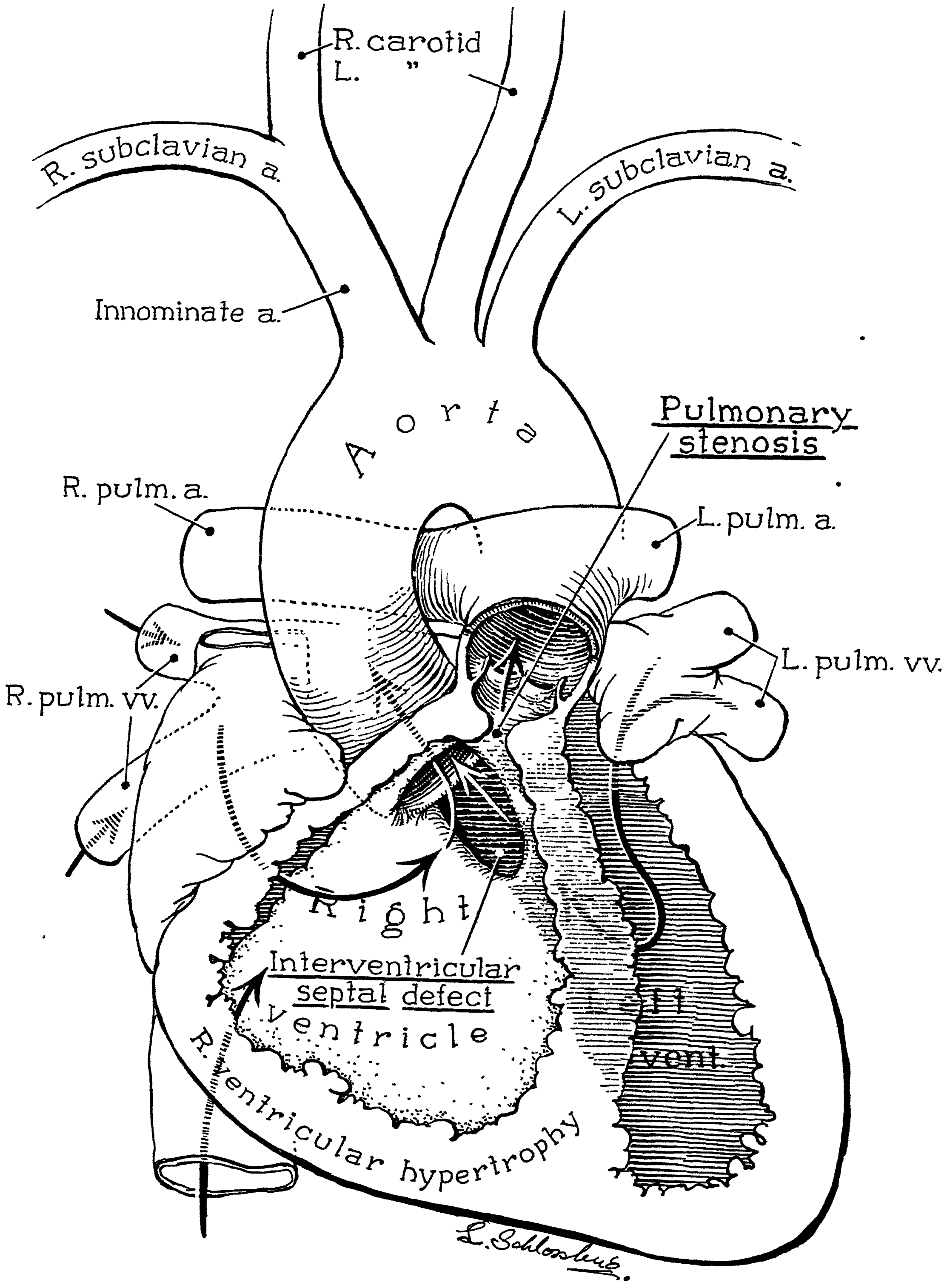


FIG. 4. Course of circulation in tetralogy of Fallot. The four features of this cardiac malformation are: (1) pulmonary stenosis; (2) interventricular septal defect; (3) shift of aorta to right; and (4) hypertrophy of wall of right ventricle.

they are much more prone to infections than are normal children. In addition, the increased viscosity of the blood may lead to clotting in the vessels of the brain, resulting in paralysis similar to that occurring in "strokes" in the aged. The defective parts of the heart may be the site of localized bacterial growth, which leads to generalized infection of the blood stream. All these factors combine to lower sharply the life expectancy of these individuals. Most of them die before reaching adult life, many during infancy or early childhood. In rare instances a person with such a defect may live to a relatively advanced age, the most famous being Henry Gilbert, the distinguished American music composer, who suffered from the tetralogy of Fallot but lived until his sixtieth year.

It is difficult to establish an exact diagnosis of the specific defect of the heart present in a "blue baby." Physical examination shows cyanosis, general underdevelopment, and clubbing of the fingers and toes. Auscultation with the stethoscope will frequently detect a murmur which blurs the clear quality of the heart sounds, but differentiation of the various malformations requires careful study of the heart shadow under the fluoroscope. A typical case of the tetralogy of Fallot may be diagnosed merely by physical examination and fluoroscopy. The more complicated malformations, however, frequently require elaborate studies. These include the electrocardiogram, catheterization of the cardiac chambers, and angiocardiography. The electrocardiogram records a tracing of the changes in electrical potential that take place with the contraction and relaxation of the heart. Catheterization studies are made by threading a fine sterile tube into an arm vein and passing it along the vein directly into the heart, where its position may be determined by means of the fluoroscope. Samples of blood from the heart chambers are taken for oxygen determination, and pressures may be measured in various parts of the heart by the use of special manometers. These determinations provide clues to the specific malformations that exist. Fluoroscopy in conjunction with manipulation of the intracardiac catheter often permits direct demonstration of abnormal openings between the right and left sides of the heart. Angiocardiography is a method of visualization of the heart and great blood vessels using the rapid intravenous injection of a radiopaque substance such as iodopyracet, followed by roentgenograms in rapid sequence.

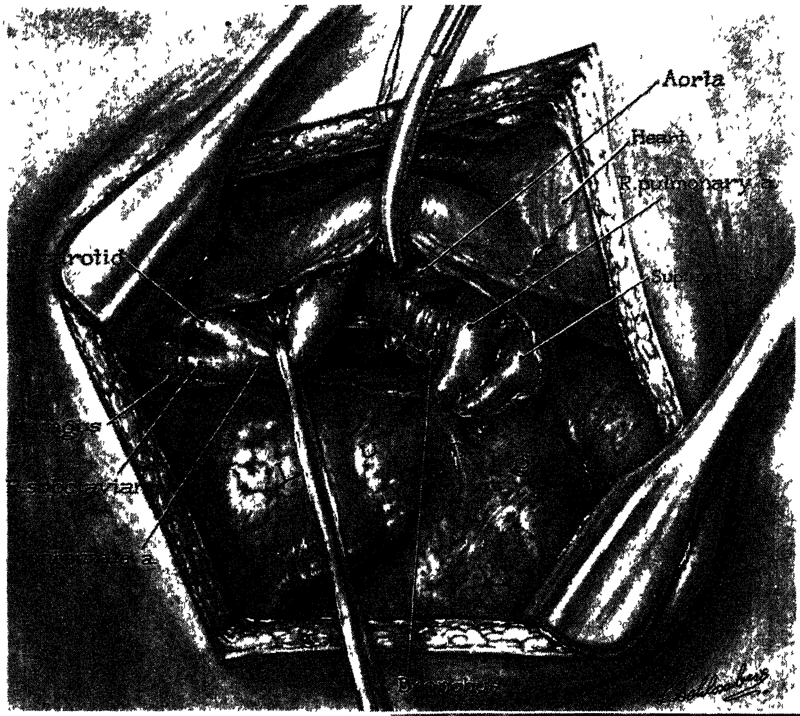
By these technics the malformation in a patient with cyanosis may be diagnosed with considerable accuracy. Once diagnosed, the problem of treatment has until recently been largely unsolved. Even with the best medical management these patients were doomed to a life of invalidism. The circulation to the lungs is not only reduced but it cannot be increased to meet the extra demands imposed by exercise. The relatively greater oxygen lack that occurs with exercise leads to extreme cyanosis and often to loss of consciousness. Patients not infrequently die during such an attack. The greatly reduced life expectancy of patients with congenital cyanotic heart disease emphasizes the need for a method of treatment.

The surgical attack on this problem has been based largely on efforts to increase the amount of blood flowing through the lungs, the need for which was shown by Blalock and Taussig. The beneficial results of increased pulmonary flow in patients with pulmonic stenosis are well demonstrated by the small number of individuals with the tetralogy of Fallot who have in addition a persistent ductus arteriosus. Some of these relatively fortunate patients have had no cyanosis and little or no limitation of activity.

If the ductus arteriosus remains open in a child with congenital pulmonic stenosis, it provides a channel whereby a large amount of blood from the aorta may enter the pulmonary artery beyond the point of narrowing. This blood passes to the lungs for oxygenation, thereby increasing tremendously the amount of oxygenated blood returned to the heart for distribution to the tissues.

Operations on patients were preceded by experiments on animals in which cyanosis was produced and attempts made to reduce or to abolish it. Cyanosis was produced by removal of lobes of the lungs in anesthetized dogs, followed by an anastomosis of the proximal ends of the arteries to the veins of the lobes. Thus some of the improperly oxygenated blood from the right ventricle would return to the left side of the heart without having picked up any oxygen in the lungs. Cyanosis was the result. After considerable trial and error for nearly two years, it was found that an anastomosis between a branch of the aorta and a pulmonary artery (artificial ductus arteriosus) would cause a lessening of the cyanosis. These experiments supplied the basic concept of treatment and afforded a means for perfecting the technic for this type of vascular surgery.

Even after completion of the basic experiments and the attainment of proficiency in the operative technic, there was hesitancy about operating on the cyanotic patient. There was fear that the cyanotic patient would not withstand anesthetization for the period of time necessary for the per-



A: Dissection of vessels in right chest has been completed. Note right pulmonary artery and right subclavian artery.

B: The subclavian artery has been divided and is ready to be sewed into the side of the pulmonary artery.

FIG 5 Operation for relief of pulmonic stenosis

formance of the operation. There was especial concern that the period during which one of the two pulmonary arteries would have to be occluded could not be tolerated. Subsequent experience has revealed that most of the cyanotic patients withstand anesthetization remarkably well. It is true that they usually become more cyanotic when one of the pulmonary arteries is occluded, but rarely does such a maneuver result fatally. In addition to the above worries, it was feared that the patient's arm might become gangrenous as a result of the sacrifice of the subclavian artery in the performance of the anastomosis between the systemic and pulmonary circulations. There has been no serious impairment of circulation to the arm in any of our patients, however.

Before describing the operation and giving the results on patients, it must be emphasized that patients with the tetralogy of Fallot are cyanotic not only because the artery to the lungs is constricted at its point of origin from the heart but also because the main artery to the body (the

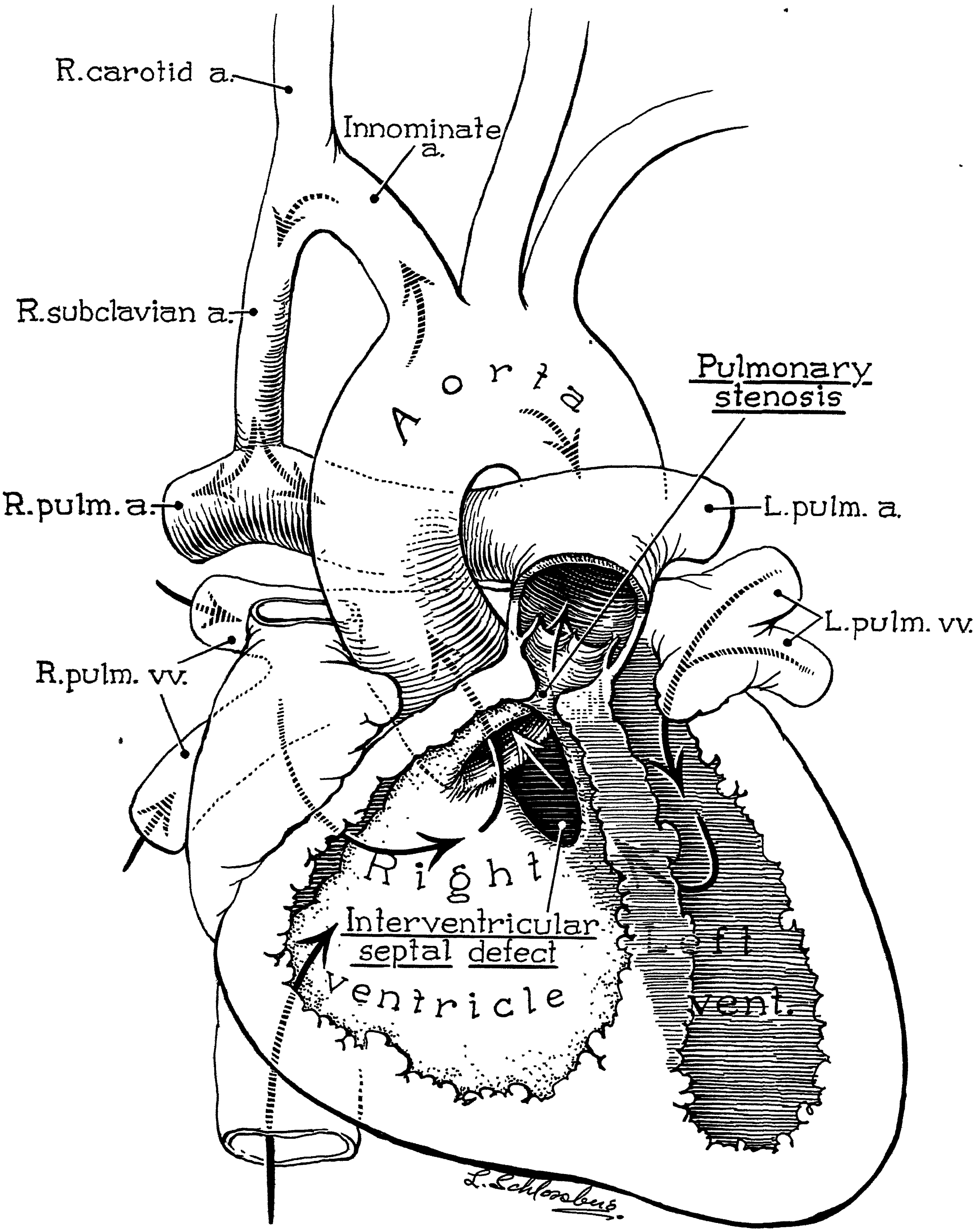


FIG. 6. Course of circulation in tetralogy of Fallot after anastomosis of right subclavian artery to side of pulmonary artery. This increases blood flow to both lungs.

aorta) contains blood that is inadequately oxygenated. The purpose of the operation is to cause some of the blood in the aorta to flow through the lungs, where it may take up oxygen. Fortunately, the blood pressure in the aorta is quite high, and the pressure in the pulmonary artery is low, so that a large quantity of blood will flow to the lungs through a relatively small communication between aorta and pulmonary artery. An opening of large size may result in heart failure.

The operation on patients is carried out under intratracheal cyclopropane or ether anesthesia, which provides maximum oxygenation of the patient's blood while allowing maintenance of quiet, even respirations during the procedure. A transverse incision is made on the front of the chest from the midline out into the armpit, and the chest cavity is entered between two of the upper ribs. The lung on the side of the incision is temporarily deflated, and the artery leading to it is carefully dissected free (Fig. 5*A*). The subclavian branch of the aorta is exposed, and an occluding clamp is placed across its base. This artery is ligated several centimeters from its origin and divided. The cut end of the subclavian artery is then turned down to the side of the pulmonary artery. Blood flow through the pulmonary artery is checked temporarily by clamps, and a small incision is placed in its wall (Fig. 5*B*). The open end of the subclavian artery is then sewed with fine silk into the side of the pulmonary artery. The constricting clamps on the two vessels are released slowly after the anastomosis is completed. If an accurate union has been made, there is a little or no leakage of blood, since fibrin quickly seals the suture line. A palpable vibration, or "thrill," can be felt in the pulmonary artery after the clamp on the subclavian artery is released. This indicates the greatly increased flow of blood through the pulmonary artery because of the anastomosis (Fig. 6). The lung is then reexpanded, and the incision in the chest wall is closed. There are several variations of the operative technic. One of these is a side-to-side anastomosis between the aorta and the pulmonary artery, as described by Potts and his associates.

The majority of patients withstand the operative procedure remarkably well. Oxygen and penicillin are administered postoperatively. Within a week most patients are able to be out of bed and are generally allowed to leave the hospital about two weeks after the operation.

With the circulation to the lungs increased by a suitable anastomosis, the color of the patient improves immediately in most cases. The improvement does not reach its maximum, however, until the elevated red blood cell count approaches normal; this usually requires two to three weeks or longer. The clubbing of fingers and toes is more persistent but disappears entirely in many patients. The most important alteration is the ability of the patient to take part in various activities. The degree of improvement in this respect varies, but in most cases it is very gratifying. Some of the patients who could walk only a few steps before operation are now able to run or to walk long distances without undue fatigue. Many patients cannot be distinguished by their activities from normal children.

During the past four and one half years, the group at The Johns Hopkins Hospital has operated on 840 patients who were suspected of having the tetralogy of Fallot or a slight variation of this combination of defects. Hundreds of similar patients have been operated upon elsewhere. The age of the patients in our series has varied from six weeks to forty-four years, the great majority being children. Including the atypical cases, approximately one patient in six has died during or since the operation. During some periods, as many as forty-four consecutive patients have been operated on without a death. The improvement in the survivors varies, but in the majority it is very great The time which has elapsed since the first of these operations is too short to permit an evaluation of the ultimate results. One may state with certainty, however, that many of the patients are leading much happier lives.

Our discussion has been limited largely to a consideration of treatment of the tetralogy of Fallot (pulmonic stenosis) because this is the most common type of congenital heart disease accompanied by cyanosis, or blueness, and because considerable progress has been made in its therapy. Satisfactory surgical methods for correction of several other types of congenital heart disease are well established. Notable among these are the operations for patent ductus arteriosus and for constriction ("coarctation") of the aorta. These corrective procedures, in common with the operation for the tetralogy of Fallot, are performed outside the heart itself. Various investigators are now working on methods for treating other forms of heart disease, both congenital and acquired, which require actual intracardiac surgery. Some progress is being made in this regard.

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OF DIGITAL COMPUTERS CALLED BRAINS

WARREN S McCULLOCH and JOHN PFEIFFER

Dr. McCulloch, professor of psychiatry at the University of Illinois College of Medicine, situated in the Illinois Psychiatric Institute, Chicago, has long been a student of the operation of the nervous system, including the brain This article is based on a speech made by Dr. McCulloch before the American Institute of Electrical Engineers last January Mr. Pfeiffer is a well-known science writer who is a member of the Board of Editors of Scientific American.

IT HAS been said that since the days of Helmholtz no man can understand all science. If by this is meant knowledge of all descriptive details, it is increasingly true. But there are many signs that at the level of theory the most remote of disciplines have developed central ideas that are pulling them together. The wildest speculative analogies of a generation ago turn out to be similar problems in dissimilar materials. What is learned in one field today furnishes a profitable attack in the remote tomorrow.

One of the most important notions that has proved its value in many sciences may be traced back to the atoms of Democritus. But the concept of ultimate units had its first bearing on modern science when the discovery that gases combined chemically by volume in the ratio of small whole numbers marked the transition from alchemy to chemistry. Unless you compare the writings before Dalton proposed the atomic hypothesis and before Avogadro computed the number of molecules per gram molecular weight with all that comes afterwards, it is difficult to appreciate how great a revolution this idea can produce in any science.

The same idea found its way next into biology when, to explain Mendelian inheritance, we invented genes as atomistic carriers of prescriptions for the adult form. As a result, this part of biology is today perhaps its cleanest discipline. The cellular hypothesis—especially the neuronal hypothesis—quantized living things in a way germane to our argument. Then physics felt its impact, and the mathematics grew up to handle it, so that we live not in an atomic world in the sense of the chemist, but in the particularized world of the nucleonic physicist—a world ultimately composed of a very large number of a very few kinds of particles, with even their energies and possible positions atomized. In this world it seems best to handle even apparent continuities as some numbers of some little steps.

Recently, the notion of ultimate units has been applied to the communications field and has opened the way to important advances in the design of calculating machines and to a better understanding of the working of our brains. The art of communication is very old, but the science is very new. When we reduced our speech to syllables and built an alphabet, we took the first great steps in the right direction, for our letters are comparable to the atoms of chemistry. All communication is a matter of information conveyed by signals, and the next great step was atomic signals. The bulk of our signals were and are analogical. They are signals in which a value of some continuously varying quantity stands for the message, like the current in your telephone or the distance on your slide rule. The difficulty with such signals is that their precision is limited to about six significant figures at best, and no combination of devices can increase that precision; hence, the information that analogical signals can convey is inadequate for many of the requirements of modern science.

There is another kind of signal in which some variable is restricted to a small number of possible values at distinct times or separate places. The message is the order or number of these possible quantities, as in the case of the dots and dashes of telegraphy and the coin of the realm. In the simplest case, this reduces to the presence or absence—call it one or zero—of something at an appointed place or time. This is the famous binary digit, now nicknamed "bit." Any number can be written in bits as well as in the decimal system; in the binary system, 1, 2, 3, 4, and 5 become simply 1, 10, 11, 100, 101. Since a device does not have to be very sensitive to distinguish between a "one" signal and a "zero" signal, precise workmanship is unnecessary. Every time you send a binary message through a relay it comes out as

good as new, so that you can combine devices that use bits to obtain an extra bit in any array—and a corresponding increase in precision—at no extra price per bit. It is interesting that any letter on this printed page, being one of some 60 items, can be selected from the printer's tray by six all-or-none binary decisions—and that such a six-bit item is a molecular signal, whose six decisions are its ultimate particles. In this quantizing of signals the science of communication finds its appropriate atoms.

The use of the binary digits 0 and 1 happily conforms to what is perhaps the simplest part of logic. We reduce the logic of relations to the logic of classes when we consider only one relation, membership in some class. We reduce the logic of classes to the logic of propositions when we consider membership in only one class, namely, the class of truths. We then assign the value 1 to anything if it is true—if not, 0. In fact, it was Boole's discovery of this property that led him to the attempts to construct a logical machine. His mathematical theory of logic has just been reprinted, but, *meanwhile, largely through the work of Whitehead* and Russell, much of mathematics has been reduced to logic. Thus, today one speaks of any device for communication which performs its computations by these discrete signals indifferently as either a digital or a logical machine. And all large modern computing machines are principally digital.

In a certain sense, the scope of any communication device, including a computing machine, is ultimately determined by the number of distinct states that it enjoys. A device of n binary component organs (nerve cells, electromagnetic relays, vacuum tubes), each of which can be one of two possible positions at any given time, has 2^n possible states. Now, since the world is composed of particles which can only be in discrete states, we could treat all machines as if they were digital devices if we could make full use of all these particles in their states. But we cannot weigh to one electron more or less, or measure the position of a pointer to the least possible jump of nuclear dimensions. We can only deal with these statistically and, when we do so, we ignore the theoretically distinguishable states of the system in the average. The same goes for our macroscopically digital devices, when we combine their components intentionally to secure averages or suffer stray fields from the operation of one component to influence another. The average may look more precise, but its accuracy ends with the last significant bit. All analogical devices are ultimately digital devices, averaged whether we will or no.

I

The human brain consists of, say, 10^{27} or 10^{28} of the ultimate particles of the physicist, and one might describe how it works by stating how those particles behave—and we may ultimately be able to do so. But for our purposes it would be about as silly as to describe a chair by the motions of its component particles. The whole development of the anatomy and physiology of the nervous system suggests that we can simplify the problem. As a device for handling signals, the brain can be conceived as an ensemble of nerve cells or neurons, which act quasi-independently. There are some 10^{10} of them, and they constitute about a twentieth of the total volume of the central nervous system. So, on the average, each is made up of some 10^{16} or 10^{17} ultimate particles. To quantize the neurons cuts the complexity of the problem about in half. We shall therefore consider these cells as components and their all-or-none nervous impulses at least binary signals. Thus, regarding the anatomy of the nervous system as if it were a wiring diagram and the physiology of the neuron as if it were a component relay of a computing machine, we shall describe the brain in terms thoroughly familiar to the electrical engineer whose business is communication. For our object is to set up working hypotheses as to the circuit action of the central nervous system as a guide to further investigation of the function of its parts, and as a scheme for understanding how we know, think and behave.

Let us replace each neuron by a relay having its own battery, charged by a series of chemical reactions converting sugar and oxygen to carbon dioxide and water. When the supply of these materials fails, or the reactions are obstructed, the voltage of the relay ceases to be in the proper range, and the circuit action goes wrong in any one of many ways. Doctors generally call the resulting disorders of thought and behavior "functional" psychoses, because they disappear when the chemical conditions are again such as to produce proper voltages at the proper places. Some of these conditions have been treated successfully by producing an epileptic fit, either with drugs or by the passage of alternating currents through the brain. The fit discharges the batteries so frequently that they are for a time exhausted. During this period and the early part of the restoration of the voltage, the brain uses 8–80 times as much energy as it usually needs.

Even so, it takes many minutes for all voltages to be near enough to normal to restore a semblance

of the brain's regular circuit action. We know it is the voltage that matters, not the particular reactions that produce it, for a nerve deprived of oxygen and so at the wrong voltage fails to transmit signals. But as soon as its voltage is restored from an external battery—say, a dry cell—it again transmits its signals properly. Except during fits on the one hand and comas on the other, the brain requires a nearly constant supply of energy which, once it is spent, must be removed as so much heat In fact, the brain heats the pint of blood that flows through it per minute a little less than 1 degree Fahrenheit. In electrical units this is a little under 24 watts. So long as this power is supplied and the heat removed, neither the neurophysiologist nor the communication engineer need think in terms of energy.

Instead, it is best to think in units of information conveyed by electrical impulses, the all-or-none signals of the neurons they traverse. These cells vary downward in size from giants that have a body, say, two thousandths of an inch in diameter, fronds, or dendrites, about a tenth of an inch long, and a tap root, or axone, perhaps a thousandth of an inch in diameter and up to six feet long. The metabolic battery of every cell keeps its outside about a tenth of a volt positive to its inside until it is stimulated. It can be stimulated anywhere by driving its outside negative, thereby starting a current which flows from near-by positive parts of its surface into the excited region, partly recharging that region, but also draining the region whence it came, until it becomes a sink for current from the surface beyond it. Thus, the pulse of current travels along the neuron, turning itself inside out like a smoke ring as it goes.

For electrical purposes the cell membrane may be regarded as a leaky capacitor kept locally charged by the metabolic battery. The pulse therefore travels at a rate determined by the distributed resistance, distributed capacity, and distributed source of voltage, so that we may think of the entire neuron as if it were simply a distributed repeater. From the circuit constants of the neuron we would expect a maximum impulse speed of 150 yards a second in fat fibres and a minimum of, say, a foot a second in thin fibres—and so it is. Since the fast fibres usually connect remote places and the slow near places, the effect of distance on temporal relations is generally minimized. The nervous impulse rises to its peak in about a tenth of a millisecond and falls away in several tenths, after which the neuron may conduct an impulse like the first.

But if the impulses are repeated in quick succession, their voltage declines and it becomes harder to produce them. When impulses arrive upon a neuron, there is a delay of about half a millisecond before its own impulse starts; physiologists call this time the synaptic delay. To trip a neuron, several pulses must arrive upon it over separate axones within one or two tenths of a millisecond, and, finally, it may be prevented from responding at all by impulses arriving in its vicinity some half a millisecond before those that would otherwise trip it. From electrical records of brain and nerve we know that some neurons do conduct trains of 200 pulses a second, but 20 a second is more nearly the usual upper limit, so that they are called on to respond somewhere between a tenth and a hundredth as often as they could.

Judging by speed of response, neurons fall into the middle range of man-made relays. They are about a thousandth as fast as vacuum tubes, about as fast as thyratrons, but faster than electromagnetic and mechanical devices. Because they are much smaller than electronic valves, though the voltage gradients are about the same, they take lower total voltages and less energy. A computer with as many vacuum tubes as a man has neurons in his head would require the Pentagon to house it, Niagara's power to run it, and Niagara's water to cool it. The largest existing calculating machine has more relays than an ant but fewer than a flatworm. Neurons, cheap and plentiful, are also multigridded, the largest having upon them thousands of terminations, so that they resemble transistors in which the configuration of electrodes determines the gating of signals.

Moreover, since neurons are mechanically rather stable and repair themselves, they have a long effective life. We get no new ones from the day we are born until we die. Every neuron in our head is as old as we are, and most of them are still serviceable. Of course, they may be killed by injury, accident, germs, want of sugar or of oxygen, or failure of their blood supply. When this happens to enough of them in the proper places, we suffer neurological diseases and what we call "organic" psychoses, meaning that the damage can be found in the dead brain.

So far as we can discover, Shannon was the first to apply Boolean algebra to nets of relays. But he was principally interested in whether circuits were open or closed, rather than in the time of opening and closing. In complete ignorance of his work, Pitts and McCulloch constructed a calculus for the impulses of nervous circuits, which has subsequently been used in teaching the gen-

eral theory of digital computing machines. The calculus is very simple. It is made from the calculus of atomic propositions of Whitehead and Russell, by treating the impulse of a neuron as a proposition embodied in the signal of a particular neuron at a particular time—and that time is measured in synaptic delays. For example, N_7 $(t–3)$ means that neuron number seven transmitted an impulse three synaptic delays before an arbitrary origin of time. If two neurons are connected so that N_1 has an impulse at time t only if it received an impluse from N_2 at the time $t–1$, they write $N_1(t) \supset N_2(t–1)$, in which the horseshoe $\supset$ can be read "implies" or "only if" for this, the sequential relation of nervous signals, is what in logic is called material implication.

Three contemporaneous relations of signals are clearly demonstrable within the central nervous system. For neurons that can be tripped by either of two other neurons, Pitts and McCulloch write $N_1(t). \supset .N_2(t–1)\ v\ N_3(t–1)$. In this expression the v is an abbreviation of the Latin *vel*, the "and/or" of legal documents. It is usually called the disjunction of its arguments. For neurons that in physiological parlance are said to require summation—that is, neurons that will transmit a signal only if signals from two or more sources arrive on them practically simultaneously—they write $N_1(t). \supset .N_2(t–1)\ .N_3(t–1)$. The dot between $N_2(t–1)$ and $N_3(t–1)$ means that both must have happened; it can be translated as "and." It is usually called the conjunction of its arguments. Finally, for neurons which can be tripped by an impulse from one neuron unless prevented by an impulse from another, they write $N_1(t). \supset .N_2(t–1)\ .{\sim}N_3(t–1)$, in which $.{\sim}$ may be read "and not," stands for conjunction with negation of its arguments.

These three expressions would be insufficient to generate the whole of the calculus of propositions unless there is a steady source of impulses or a neuron that regularly fires spontaneously, like the local oscillator in a heterodyne circuit. Then they are more than sufficient. Using this calculus, Pitts and McCulloch proved that even nets devoid of closed paths could, like Thüring's machine, compute any computable number or—what amounts to the same thing—could have a single signal in a specified neuron which implied any arbitrarily selected figure in the input of an appropriate net. In such a net the logical relation of implication extends backward in time and backward toward our sense organs, so that what happens in our heads implies the world that did impinge upon our sense organs. In the world of physics, things either happen or they do not, and that is all there is to it. But in communication the simplest things are signals which are either true or else false. If you press on your eye, you will see a light when there is no light, just as when lightning strikes the wires your telephone may ring although no one is calling. In such a case, what was apparently implied had not happened, and this is the asymmetrical nature of the relation of implication, that the true only implies the true, but the false may imply either.

Notice also that the domain of this implication of signals extends only into the past. What is going on in our heads at a given relay time does not imply what is yet to happen in our arms and legs. The impulses descending from our brains play upon complicated servomechanisms that keep us adjusted to the world about us in motion and at rest. These have their own feedbacks over afferent, peripheral neurons, and the impulses coming by these shorter paths may so determine their behavior that something else may happen instead of what we intend. This compels us to distinguish between what we will and what we shall do and, in forcing this distinction, has created the notion of the will. When we build computing machines that can compare what they intend with the record they make for us to read, they will be confronted with a similar difficulty, and may then be said to have wills of their own. This may lead them to prefer magnetic tapes or wires, whose signs may be erased, to an irrevocable punch in a card. At the moment this may sound far-fetched, but we are beginning to require similar discretion of them in the use of their own long-storage memories, whence they may recall items and reject all but the one wanted.

Modern computing machines have several kinds of memory. Theoretically, the simplest is one composed of relays like the rest of the circuit, but arranged in a closed path of sufficient length so that the beginning and the end of a train of signals running around the loop do not overlap. In such a path a train of impluses patterned after some input may continue to circuit as long as we please; as long as it lasts, it continues to reiterate in the form of its input the thing sensed. Kubie first proposed, and Lorente de No first demonstrated, the existence of such paths within the central nervous system. The moment we introduce them into our scheme of things, we bring into our calculus two new things, for we now know that there was some event which was of that figure, but we have thrown away its exact position in time. Pitts and McCul-

loch, following Whitehead's and Russell's example, write $(\exists x)\phi(x)$, which is read, "There is some x such that x is of the figure ϕ." ϕ, the function of x, is called a universal, or idea. In our example, it is the figure of the input, and x can take the meaning "any event which was of that figure, regardless of when it happened." $\exists(x)$ simply asserts that there was some x. By combining this assertion with negation, we get—for example—$(\exists x)(\sim\phi x)$, which means the same as $(x)(\phi x)$. Both expressions say, "For all x's, x is of the figure ϕ." It is for this reason that we may regard ϕ as a universal.

Man-made relays are too expensive, large, fast, and inefficient to be used like neurons for reverberating-chain memories. The memory devices of calculating machines are acoustic tanks, cathode-ray tubes with screens of various persistences, and many other instruments which clear themselves completely when they are shut off. But none of these devices does anything that cannot be done by reverberating chains, and this holds true for every kind of memory—whether it is born in us or made by us—even for the marks on this paper. The reason is that to be effective there must be a way from the computer into the storage, and from the storage back to the computer, which effectively completes a path around which the information goes.

We know that there are probably two other kinds of memory in us, one of which is principally responsible for our skilled acts. The learning of these things resembles the soldering-in of a multiplication table. The other kind of memory preserves for us a record of things sensed but once in passing. In several ways it resembles a stack of photographs or snapshots stored in the order in which they were taken, but found with difficulty—if at all—in the reverse direction. They seem somehow to be earmarked so that, like the punch cards in a machine, they can be selected or recalled on the basis of common properties. Judging by the number of such similarities at our disposal, our snapshots must be earmarked in many hundreds of places. From the work of John Stroud, of the U. S. Navy, it seems that we would have a chance to "snap" about 10^{10} such pictures in a normal life, and, if each had any reasonable content and a thousand ways of recalling it, human memory must have storage space for some 10^{13}–10^{15} bits.

The physicist Heinz von Foerster, from a study of the curve of forgetting of nonsense syllables and from very general considerations based on biology and quantum mechanics, came to about the same number. He points out that if we know the mean half life of a trace, or stored bit (which in us is half a day), the half life of our signals for access to and egress from the storage of the traces, and the number of parallel channels in and out of it—then we know how large that memory must be to exist in statistical equilibrium with the access and the egress. It is simply the number of parallel channels in and out (10^7) multiplied by the ratio of the half life of the trace to the access time, both in seconds (10^5 and 10^{-3}, respectively), or 10^{15} places for stored bits of information. Reasonable considerations for the stability of each such bit would allow it about 10^5 or 10^6 possible loci in its cells, making a total of something like 10^{21} total loci in the brain. A half life of half a day gives an energy per step of somewhere in the first or second octaves in the infrared and a total power to maintain traces of the order of a hundredth of a watt. All these specifications could easily be met by what we know of brains whose enzymes are resonant at the proper wave lengths and the number of whose protein molecules is several times 10^{21}.

But Foerster presents a second reason which makes us suspect protein molecules. Whereas all ordinary physical processes have curves of decline which approach zero asymptotically, the curve of forgetting approaches asymptotically a residual value of several percent. In other words, some things are never forgotten. This can only be accounted for by some regenerative process capable of multiplying traces before they fade, and only protein molecules—nucleoproteins, to be specific—seem to possess the ability to generate others templated after themselves. (An interesting parallel to this regeneration of traces is to be found in the modern proposal to use in computing machines a gridded tube of short persistence, writing in and taking out information in one round and, in the next, refreshing the trace by reading, erasing, and rewriting it.) Moreover, clinical evidence indicates that protein denaturants destroy memory, most clearly so when alcohol, lead, or other poisons attack an old central structure of small neurons, whose destruction produces the famous Korsakow's psychosis and makes it impossible for animals to learn.

II

So much for the regenerative closed paths of memory. All other paths are normally inverse—that is, they work on the general principle of the governor of a steam engine. When the engine is running faster or slower than a certain level, the governor senses the discrepancy and more steam is fed into the boiler or released from it. Similarly, each inverse path of the brain establishes some state or level for its system, by bringing the system back toward that state whenever there is a displacement

in any direction. The established state is the end in and of the operation of its system. This is the fundamental nature of a function.

The function may be mediated by a path lying entirely within the nervous system. For example, there is a circuit which keeps the average number of impulses reaching the bark of the brain, or cerebral cortex, nearly constant, by inhibiting the relaying of impulses thence whenever their number increases beyond a certain level. In this it resembles the automatic volume control built into any good radio. There are the reflexes whose path comes from sense organs, goes through the central nervous system, and returns directly or indirectly to those sense organs, so that whatever fluctuations they are forced to undergo are stopped or reversed by the returning impulses or their secondary consequences. It is these that make it possible for us to stand and walk even on a ship at sea. It is these that keep our bodies at one temperature and our blood pressures at constant level. Many of them are, in fact, servomechanisms, for we can by impulses impingent on neurons in the path of a given reflex determine the particular value of the variable sought by that reflex. Finally, there are our appetitive circuits which, in the engineer's phrase, are inverse over their targets. It is by means of these that we have and seek our ends in the external world. In short, it is the program of the branch of the science of signals called cybernetics to analyze all purposive behavior in terms of inverse feedback.

But our immediate interest in these circuits is somewhat different. Because each feedback circuit brings some aspect of an input back to a preassigned value, it rids that input of the gratuitous particularity of the value it had when we encountered it. In the case of vision, three such devices work one after the other. First, the slow adaptation of the retina to light; second, the quicker pupillary reflex; and, third, the automatic volume control of impulses relayed to the cortex—rid a form to be seen of its variable intensity at the input, so that we know that there was some brightness of illumination which was of the given figure. Because these devices generally pull toward one value predetermined for us in the scheme of our nervous activity, the final value seems to us God-given, and the process is said to reduce the input to a canonical brightness or what-not. The anatomy of one of the most interesting of these reflex circuits was described by Cajal, and its physiology was discovered by Julia Apter. It goes from the eye, whose field it maps on the back of the midbrain in the superior colliculi, structures which compute a vector from the center of the line of gaze to the center of gravity of retinal excitation, and relay this vector to the oculomotor system. This system then turns the eye so as to reduce the vector to zero, bringing the apparition to the canonical position which is the center of the visual field, represented at the fovea of the eye. In this way it rids the form to be seen of the gratuitous particularity of the place where it was first detected.

Now, short of going toward or from objects, there is no way to move our eyes that will reduce the image to a canonical size on the retina. But there is another way of attaining the same end. This depends, as we shall see, on a kind of averaging performed in a net whose output is not significantly altered by minor changes in the thresholds of neurons or their excitations, or even their connections so long as these be to cells in the right neighborhood. One can even poke holes through such a net and still have it work well for most purposes. The path from the eye through the lateral geniculate maps each half of the visual field on the surface of the opposite primary visual cortex onto a fine mosaic of neurons, or relays, in such a way that the distance measured on the surface is roughly proportional to the logarithm of the angle between the line of sight and the light ray coming from the point to be mapped. The incoming axones come up into the receptive layers of the primary visual cortex and branch as they ascend to terminate upon layer above layer of these mosaics of relays. In this way every input from the visual field produces a small, roughly circular area of stimulation in the primary visual cortex, the circle being greatest in the highest layers. But, owing to the logarithmic nature of the mapping, these circles on the cortex correspond to ovals in the visual field, having the small ends of their long axes pointing at the point of fixation. Thus it comes about that by picking the proper layer of the cortex one can find a figure of stimulation corresponding to any required magnification or dimunition of the figure of input at the eye.

If now we set the threshold of every neuron in the receptive layers of the primary visual cortex so that it will fire only if it receives an input from the eye and excitation alerting its whole layer—and then alert the layers one after another—we shall have made from a figure of one size in the input figures of all sizes, bigger and smaller, in the layers of the cortex. The signals from these neurons of the receptive layers ultimately converge downward upon neurons at the depth whose axones leave the primary visual cortex, to turn up again in the second visual cortex in a thoroughly scattered

fashion. As a result, to any figure of the output of the primary visual cortex there will by chance correspond some one locality of maximal excitation in the secondary visual cortex. An excitation at this point corresponds to the shape, regardless of size.

At the present moment, it seems possible that the so-called alpha rhythm of the brain is the sweep of the alerting pulse through the cortex, for its frequency (about ten per second) is that at which one can see shapes regardless of size or hear chords regardless of pitch. If this be the answer, then the alpha rhythm should disappear—as it does—when the cortex goes to work with attention to its input as in looking at anything, for the signals in its receptive layers and in its large output neurons have their principal voltage gradients also directed vertically through the cortex. Stimulation of the primary visual cortex of a man awake with his brain exposed at operation should produce—as it does—an apparent ovoid blob of light, whereas stimulation of the secondary visual cortex should give us—as it does—a form seen.

III

We may characterize all processes of this kind as follows. Given an input, we may lead it into a matrix of relays and there make all the transformations belonging to some group—in the visual example, magnifications and dimunitions. Then we compute a set of numbers, q, each of which is the average—for all transformations of the group $(T \varepsilon G)$ of the value assigned by some arbitrary functional, f, to each transformation (T) as a figure (ϕ) of excitation in time, t, and space, x, in our matrix. This gives us the following expression:

$$q = \sum_{T \varepsilon G} f\,(T(\phi\, x\, t)).$$

To define the form without any loss of information we would need a matrix of such averages, with as many degrees of freedom as our original matrix of neurons. In practice we are content with a far smaller number. This is so general a theory of coding devices that it applies also to reflexes. In them, the functional merely needs to assign the value zero to all transforms but the last.

For this very reason, the theory of computing invariants for groups of transformations is not of much help in the design of mechanistic hypotheses. To them we must be guided by our knowledge of anatomical structure and by physiological determination of the relations of the input and output of that structure's signals. When a brain has an idea, we know that there is some invariant in its activity. It may be a succession of signals in time over a single neuron, as in the case of reverberant memories, or the canonical value determined by a reflex, or merely a position in a mosaic, as in the last example. Moreover, a net of neurons can convert any figure of simultaneous signals over a given number of neurons into a sequential figure of impulses over one neuron, occupying as many synaptic delays as its prototype required neurons simultaneously, thus exchanging space for time. Conversely, it may exchange time for space. Hence, unless we know something of the net, it is clearly impossible to guess what sort of invariant implies a given idea. Finally, the same invariant may be calculated equally well in more than one place. For example, a vertical line can be seen equally well whether it lies to the right or the left of the point of fixation, and that means in the one case in the one hemisphere and in the other case in the other hemisphere of the brain. Rarely do we know enough of the details to guess what we ought to expect or where we ought to look, when we record directly the electrical pulses of the working brain.

But the story of ENIAC—the electronic numerical integrator and computer at the Ballistics Research Laboratory—is equally scandalous. It is so complicated that no one knows its entire blueprint, and certainly no one knows whether it is wired according to that blueprint. In the case of our own brains, it is certain from the chemistry of our chromosomes that our genes cannot specifically determine all the connections of our neurons. What they do is to specify a relatively simple machine, which goes on to build a more complicated machine, which elaborates a third and so on, until the last prescribes our most complex structures, like the cerebral cortex. Von Neumann has suggested that the plan is something like this: the earlier machines are never completely superseded or separated from the final machines, but serve to tend them. When they find any part preoccupied or out of order, they shift the problem to be solved to portions of the newer structures of the brain that are free and able to solve it. This trick is old among the builders of computing machines, and is already automatic in the differential analyzer at MIT. For a minimal computing machine of a few thousand relays, which does nothing in parallel channels, von Neumann estimates that one error in 10^{12} unitary operations is tolerable. The best modern electromagnetic relays fail about once in 10^9 operations, and neurons far more often. Neurons cannot be replaced,

but there are many of them. Consequently, we find that brains perform most operations in parallel, many times over, and then demand agreement before passing on their signals. For all these reasons, large numbers of neurons may be dead and gone and the answer come out correct.

There is another thing that makes it even harder to detect local and scattered failures Consider a collection of n neurons, say, in some place like the bipolar cells of the eye, no one of which can influence another. Such an ensemble of n neurons can be in any one of 2^n states in any synaptic delay. Following Wiener, we will define a unit of information as the decision as to whether in a given synaptic delay a given neuron does or does not transmit an impulse. Then n units of information are required to specify the state of an ensemble of n neurons. For each unit of information subtract one from that exponent. From this it follows that the amount of information in an ensemble of neurons is the logarithm to the base two of the reciprocal of the probability of the state in question. In thermodynamics we have a familiar quantity, namely, the logarithm of the probability of a state, which is called entropy. Thus, information is negative entropy. The second law of thermodynamics, which insures an increase of entropy, means that information can never increase as it passes through any computing machine.

Actually, we know that information is always corrupted in every device for communication. It is only the noise that increases. The amount of corruption can be expressed as the information in the input divided by the information in the output. Since each human eye has a hundred million photoreceptors, each capable of transmitting one unit of information per synaptic delay, a man receives at least 2×10^8 units of information per millisecond. It is possible to estimate the information he puts out per millisecond by noting that a new telephonic device, sampling speech once per millisecond and emitting one pip or none according to the instantaneous amplitude of the sound wave, transmits almost all the information conveyed by the human voice. So the amount of corruption in a single passage of information through a man is about 10^8. Something of the order of 10^3 we may attribute to the way in which our nervous system is coupled to our muscles. With the rest we purchase a kind of security.

Impulses from the hundred million photoreceptors of each eye converge through bipolar cells on, say, a million ganglion cells whose axones extend into the brain. The thresholds of these transmitters to the brain are sufficiently high so that signals from many sources must converge on them contemporaneously to trip them. The convergence of these connections is obviously of the order of 100 to 1. The requirement of coincidence insures that the impulses that do reach the brain have a high probability of corresponding to something in the world, for the chance of error may be reduced to one part in 2^{100}, or about one part in 10^{30}. It is this process of required coincidence, repeated all the way through the nervous system, which insures that what we do is adjusted to something real in the world about us. Obviously, in such a circuit an enormous number of the parallel paths at any level may be defective, and the loss of information will disappear into the general corruption. For this reason alone, many neurons might die before we had to take our brains to the doctor.

Let us compare this with the picture that develops when any other inverse circuits become regenerative. This is Kubie's theory of the reiterative core of every neurosis. When a circuit of this kind becomes regenerative, the regeneration tends to grow until the circuit has swept into its orbit all the cells that it can force to fire in phase. In short, its gain increases until it overloads itself maximally. It now ceases to transmit any information. Such was the howling of our old one-tube receivers, with a feedback from the plate to the grid when the gain was too great. When such a circuit action persists long enough, it causes some sort of change in us such as that underlying the acquisition of skilled acts. We may then turn off the circuit temporarily, and, when we turn it on again, it will get going in its old regeneration. In fact, any input that can contribute, however indirectly, to its excitation will start it howling as before.

Consequently, the neurotic process seems to wall off from us that information which the structures it invades would normally convey, and even other information that passes through channels that might contribute to its excitation. We know something of the whereabouts of these vicious regenerating circuits in two or three mental conditions. In the early states, we may stop the process by blocking the regenerative loop or, by sufficiently violent excitation over other channels, steal from it those neurons it had swept in phase and so reduce its gain to the point where it ceases to regenerate. But later it becomes necessary to section the paths. When this can be done in peripheral nerves or in the spinal cord, the patient suffers minor mayhem. But when we have to cut the loops uniting the forefront of the brain with deeper structures, we destroy some of the highest traits of

character—certain judgments with respect to things and men, and the ability to discover new underlying generalities among ideas.

There is no use looking for chemical reactions gone generally wrong or for dead or dying neurons in these brains. We must, while they are alive and active, detect electrically what portions are regenerating. We do not mean that there are no changes in such nervous systems. There must be alterations of excitability of neurons, perhaps of their connections, but as yet we cannot find them. Every time we build an inverse feedback into a machine we make it possible for it to have a comparable gremlin, and gremlins are diseases which defy dimensional analysis. Pure numbers and the logarithms of pure numbers are but the stuff of which they are disordered forms.

THE SOIL SPEAKS TO MAN

In all the world I am your provider—
I hold the sinewy roots of tall trees—
I clutch the grass and hold it aloft, waving.
The small animals are not strangers to me.
Nor the rivers,
Nor the oceans,
Nor the onslaught of rain as it runs through my hair

I have been servant to you—tall, strong men;
You have pushed me aside, molded me, but never changed
my elements.
I have, like many Atlases, held your world on my shoulders,
Buildings, bridges, brawling bulwarks standing against
your brother-men.
And I have done naught but yield to your wishes,
brutish, mauling, scarring my skin.

I am the moist, dark earth—
Stronghold of life—
Guardian angel to you and to your sons.
I am strong, though I yield;
I am rich, and you would possess my riches.
You, the transient wanderers, will cease roaming
only to find rest once again in my grasp.
You, whom I have nourished, will nourish me,
and life will be everlasting.

DAVID F. OLSON, JR.

THE SCIENCE OF HUMAN LEARNING, SOCIETY, CULTURE, AND PERSONALITY

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THE past decade has witnessed a revolutionary development in the psychological and social sciences. A number of disciplines that had previously pursued independent courses in the analysis of particular facets of man's individual and social behavior have been discovered to dovetail into one another so neatly that they are well on the road to being fused into a single integrated science. The first major steps in achieving this integration were made at the Institute of Human Relations at Yale University, but the movement has spread to other institutions and is being pressed forward with especial vigor by the new Department of Social Relations at Harvard University.

This development has been widely misunderstood as a mere pooling of separate scientific skills and techniques on cooperative research programs The significant fact, however, is that the integration has taken place at the level of theory. At least four previously distinct systems of theory have been found to interdigitate so that each supports the others and is in turn illuminated by them. These four are the theory of learning and behavior developed by behavioristic psychologists, the theory of social relationships and social structure developed by sociologists and social anthropologists, the theory of culture and cultural change developed by anthropologists with significant assistance from sociologists, and the theory of personality and its formation developed by psychoanalysts and psychiatrists.

There is as yet no general agreement as to an appropriate name for the emerging unified science. Such terms as "human relations" and "social relations" slight the psychological components and, to some, suggest application rather than theory. The "science of human behavior" carries too strong a connotation of behaviorism and too weak an implication of important social and cultural factors. The general term "social science" seems to exclude psychology. In default of an apter expression, we shall, with tongue in cheek, use "lesocupethy" —coined from LEarning, SOciety, CUlture, and PErsonality THeorY. Perhaps it will irritate some reader into proposing a more satisfactory name.

I

The position of "lesocupethy" in the hierarchy of the sciences poses no difficulties. It is rooted in biology as the latter is rooted in chemistry. But as biology is differentiated from chemistry by complications introduced by living matter, so is "lesocupethy" distinguished from biology by complications resulting from the interaction of learning and society.

In themselves, learning and society represent two of a considerable number of major types of adjustment which have been independently developed several times in the course of organic evolution. Other examples include parasitism, symbiosis (e.g., plants which depend for pollination upon bees, which in turn depend upon the nectar of the plants for food), and aerial locomotion (independently achieved by insects, pterodactyls, birds, and bats).

The most basic type of behavior mechanism with which nature has equipped its living species is instinct. The organism is provided by heredity with a structural organization whereby it automatically responds to stimuli by specific forms of behavior which through natural selection have become established as adaptive in the life conditions typically encountered. Being essentially rigid, however, instincts cannot help the organism if conditions diverge from the typical. To meet this situation and prevent wholesale extinction of species under fluctuating conditions, organic evolution has developed inherited mechanisms of learning in all but the simplest species. These supplement instincts by enabling the individual organism to modify its behavior adaptively within

a greater or lesser range of varying conditions. A notably flexible mechanism of learning is hereditary in all mammalian species. Experimental psychologists who have made intensive studies of animal learning agree that the basic mechanism in man differs in no significant respect from that in other higher mammals. The fundamental principles of acquired behavior are thus mammalian rather than specifically human, and can be illuminated by experiments with rats and dogs just as experiments with fruit flies have advanced the knowledge of human genetics.

Social life is another major type of adaptation which organic evolution has repeatedly produced. Gregarious species are exceedingly numerous, but the most startling superficial resemblances to man are found among the social insects—bees, wasps, ants, termites. In most instances natural selection has created social aggregations by equipping a species with hereditary mechanisms which have the effect of attracting individuals instinctively to one another. Sex is one example. Another is the sweet juices exuded by certain social insects for the delectation of their fellows. Brought into association in some such way, the members of a society enjoy advantages not available to isolated individuals—for example, mutual protection, insurance, enhanced power, and the benefits of a division of labor.

Although neither learning nor society is peculiar to man, in conjunction they have produced something unique in nature—a new level of complexity in natural phenomena which demands for its understanding a distinctive body of scientific theory. The inherited mechanism of learning and the hereditary bases of social life will ultimately yield their secrets to biological science, but the products of their interaction will require the special sciences that compose "lesocupethy," at least until the millennium when all biology has been reduced to chemistry and all chemistry to physics.

Except in man, what the individual of any species learns in his lifetime dies with him. Every individual in every generation starts from scratch. All or most of what he learns he acquires for himself. He derives little or no benefit from the experience of others, even if his species is one of those characterized by social life. In man, however, most of the behavior acquired by any individual, in whatever part of the world or period of history he may live, has previously been learned and found adaptive by other and older members of his own society, and he in turn transmits this behavior, together with any adaptive modifications acquired through his own life experience, to other and younger members of his social group. The interaction of learning and society thus produces in every human group a body of socially transmitted adaptive behavior which appears superindividual because it is shared, because it is perpetuated beyond the individual life span, and because in quantity and quality it so vastly exceeds the capacity of any single person to achieve by his own unaided effort. The term "culture" is applied to such systems of acquired and transmitted behavior. Since cultures change with the varying and cumulative experience of individuals in social groups, it becomes possible to say of man, as of no other species with the hereditary capacity to learn, that societies as well as individuals learn. Social learning is synonymous with cultural evolution.

In social species other than man, the forms of social organization are primarily determined by the biologically inherited mechanisms which produce association. They are therefore the same in all societies of the species, except for minor modifications dependent upon ecological or demographic factors. Among fur seals, for example, one does not find some groups with matriarchal and polyandrous families, nor among honeybees some hives with kings and male workers. In man, however, extreme differences in social organization are common, even in tribes of the same subrace, language, and geographical area. Among the Siouan tribes of the Western Plains, for example, the Omaha are patrilineal and the Mandan matrilineal, and among the Malayan tribes of Sumatra the Batak are patrilineal and the Minangkabau matrilineal. The conclusion is therefore inescapable that in man—alone among the social animals—society itself is largely learned, i.e., is the product of cultural rather than of biological evolution. To be sure, biologically conditioned social bonds are not wholly absent. Thus lactation helps to link mother and child, and sex to bind husband and wife, in the organization of the human family. No biological or innate basis is discernible, however, for the overwhelming proportion of the social ties which produce the complex organizational structures characteristic of human groups.

Since human society is never a spontaneous expression of biological potentialities but must always be learned as a part of culture, it follows that man must be molded to his society much as a colt is broken to harness. He must, in short, be "socialized." In all societies this is largely accomplished during infancy and childhood, when the culture of the group is implanted by inculcation, and the unsocial or antisocial impulses with which the child

is born are disciplined and redirected to fit him for the social roles he must fill as an adult. Conflicts are thus inevitably set up in the developing child between his biologically inherited impulses and the demands of his society as these are imposed upon him by his parents and later by others. The manner in which these conflicts are resolved by the individual, reflecting the concrete circumstances under which the social disciplines are impressed upon him, largely determines his "personality."

II

It should by now be abundantly clear that learning, society, culture, and personality are far indeed from being separable entities, even though until recently they have mainly been studied in isolation by psychology, sociology, anthropology, and psychoanalysis, respectively. Their interrelationships are so intimate that leaders in all four fields are coming more and more to recognize that they form the subject matter of a single integrated science. So significant are the interrelationships that they deserve somewhat fuller exposition.

Since society, culture, and personality are all learned, students of any of these phenomena must constantly bear in mind the fundamental principles of learning as these have been worked out by such behavioristic psychologists as Hull. Unless they do so, their conclusions will suffer in clarity if not in validity.

Knowledge of the structure of society is equally fundamental to the students of learning, culture, and personality. It forms an essential part of all normal situations in which human beings learn. Unless he reckons with it, the behavioristic psychologist can never explain adult human learning, however accurately he may account for the acquisition of habits by rats and other nonsocial animals. Culture only exists in, and is borne by, organized human groups, and the anthropologist who ignores the latter can tell us little of significance about the former. Since personality is largely the product of group pressures, the psychoanalyst should reckon with all important aspects of a society's structure. In our own society, for example, if he deals only with the family situation, and overlooks such significant structures as those of status and prestige, his interpretations will fall far short of completeness.

Culture is deeply relevant to the study of learning, society, and personality. Most of what any human individual learns is already part of the culture of his group, and the cultural habits that he already possesses in large measure predetermine his behavior in any new learning situation. The psychologist can ignore culture when he studies the behavior of rats or dogs, but if he does so when his subject is man, his explanations may be wide of the mark. Since social interaction always takes place within a framework of social structure, which is regularly a part of culture, sociological generalizations made without reference to culture are likely to be meaningless. Culture is crucial to the analysis of personality, not only because traits of the latter are often socially shared but also because the disciplines through the imposition of which personality is formed are largely prescribed by the culture.

Personality is no less significant than is culture for an understanding of human learning, since the reaction of any individual in a learning situation is likely to reflect significantly his resolution of the conflicts arising during his socialization and the unconscious anxieties and hostilities generated thereby. Society, too, reflects personality factors. Thus, as the sociologist Sumner showed long ago, human societies are characterized by "antagonistic cooperation" because of the conflict between individual impulse and social pressures, and they exhibit the phenomenon of "ethnocentrism" because the hostilities generated but suppressed by in-group disciplines are displaced toward other groups in such forms as race prejudice, religious intolerance, and national rivalries. Personality also affects culture. It appears, for example, to be a significant factor in the development of what is called "national character." Moreover, as Kardiner and Linton have shown, certain aspects of culture, such as religious beliefs, tend to be reflections or projections of attitudes commonly engendered during the socialization process.

During the early attempts at the Institute of Human Relations, in 1935–42, to assemble the theories of learning, society, culture, and personality into a single integrated discipline, two very important discoveries were made. The first was that the four theoretical systems, although developed in relative isolation, fitted together almost as well as the adjacent pieces of a jigsaw puzzle. The gaps and inconsistencies were unexpectedly few. Other systems of theory fitted much less well —some because of dubious validity, others because of their more limited scope or more pragmatic character. Economic theory, for example, appeared to be related primarily to the conditions prevailing in a restricted group of complex societies during a limited period of history, and thus to be culture-

bound rather than universal It had, for this reason, afforded little help to anthropologists in understanding the economic behavior of primitive peoples.

The second major discovery was that each of the four systems shed new light on the others and often converted ambiguity into clarity. Thus, learning theory demonstrated that personality is really learned, despite Freud's persistent invocation of "instinct," and it corroborated Malinowski in his insistence that culture is always functional and does not persist through sheer inertia. Personality theory showed that the concept of "drive" as well as of "stimulus" is necessary in order to comprehend the motivation of learning, and it illuminated the view of Sumner that the elements of culture are emotionally charged rather than neutral or devoid of affect. The theory of social structure revealed that Freudian psychology rests on sociocultural as well as on biological or physiological assumptions, being concerned with the products of learning under conditions presented by family organization and the imposition of social sanctions. Culture theory demonstrated that psychological principles are never competent to explain any social phenomenon unless account is taken, not only of behavior mechanisms, but also of the historically determined conditions under which these mechanisms operate, particularly the so-called cultural base and the prevailing structure of social relationships.

The manner in which the four originally independent systems of theory have been found to dovetail gives confidence in the essential validity of each, and the new insights that each has brought to the others confirm the impression of their essential unity. Recent work by anthropologists like Gillin and Hallowell, sociologists like Merton and Parsons, psychologists like Mowrer and Sears, and numerous others reveals that the integrated discipline which we have dubbed "lesocupethy" is approaching maturity. It is perhaps best exemplified to date in the volume *Social Learning and Imitation,* by Miller and Dollard. I have myself recently shown, in *Social Structure,* that there are some problems of social science—e.g., the incidence of incest taboos—that are capable of solution only when findings from all four of the constituent disciplines are applied conjointly.

The juxtaposition of the several behavioral sciences has also revealed areas in which intensive research is urgently needed. One such is related to the fact that much of human behavior is ideational, depending upon the use of linguistic and other symbols. What the Gestalt psychologists have called "insight," and others, "intelligence," is known to be of enormous importance in learning When exhibited by human beings this is believed to involve a transfer of trial-and-error behavior from the motor organs to an ideational process utilizing implicit linguistic symbols, the individual resorting to a motor response only after he has "thought out" a promising solution to the problem facing him. That this symbolic process can use other than verbal tools is demonstrated by the fact that the anthropoid apes as well as man exhibit insight or intelligence. At present it can only be assumed that the same principles of learning are involved in ideational as in motor behavior. This conclusion must, however, be established or revised by experimental methods, for until this is done the exact bearing of learning theory upon personality and culture, in both of which symbolic behavior plays a highly significant role, will remain uncertain. In any such research the participation of experts in linguistics and semantics will be as essential as that of psychologists.

III

The disciplines that compose "lesocupethy" have in common the fact that they all deal exclusively with acquired behavior. It is readily admitted that biological factors bear directly upon human behavior in diverse ways. Learning is obviously affected by the physiological condition of the individual. Society, as already noted, has biological underpinnings in such phenomena as sex and lactation. Culture is possibly influenced in some small measure by racial heredity, and, as Gillin has pointed out, definite limits are set to its variability by man's innate endowment. Constitutional factors certainly play a prominent role in psychotic aberrations and very likely also in normal personality. The interpretation of all these and other comparable influences, however, is a task of the special biological sciences. "Lesocupethy" is concerned only with what is left when these are factored out. That this is a major assignment there can no longer be any doubt.

Incidentally, there is not yet sufficient recognition of the fact that vastly more is scientifically known about the acquired than about the innate factors in man's social behavior. Precise knowledge of the latter must in many cases await the full development of human genetics, which in the very nature of the case cannot be achieved until several generations have elapsed. By that time the integrated science of acquired behavior should have developed a hundredfold.

In all fields of science there are segments of verified knowledge which have not yet been integrated with the basic theoretical system of the discipline. In the social and psychological sciences there are comparable segments—most notably perhaps in sociology, economics, linguistics, and social psychology—which "lesocupethy" has not as yet assimilated. Thus far, however, there is in this field little evidence of major alternative theories, verified but unreconciled, concerning the same body of phenomena, comparable to the wave and corpuscular theories of light. Can it be that man's social behavior is actually less complex, not more complex, than the subject matter of physical science? And is it not perhaps possible that we may have more of the essential answers to the basic scientific problems even earlier in the former field than in the latter?

The union of disciplines which we have called "lesocupethy" is a pure science. Its objective is the maximal theoretical understanding of the peculiar ways of men. It does not directly seek the solution of any practical problem. Naturally it has much in common with such applied sciences as psychotherapy, social work, education, industrial relations, and colonial administration, just as the physical and biological sciences are applied in war, engineering, industry, agriculture, and medicine. Admittedly the applied sciences continually add substantial increments of knowledge to the pure sciences upon which they depend. Nevertheless, history has shown, and both industry and government now recognize, that the support of research in pure science frequently yields richer dividends through the application of newly discovered basic principles than are obtainable by a direct attack upon specific practical objectives. It is therefore not inconceivable that the cultivation of "lesocupethy" as a pure science may lead more quickly to a solution of international conflict, economic insecurity, industrial strife, family disorganization, and individual mental disorders than research oriented directly toward the solution of these pressing problems.

But whether or not such high aims are realized, the emergent integration of the basic theoretical systems of the social and psychological sciences undoubtedly represents one of the great turning points in the history of science. In significance it may prove the equal of the contributions of Darwin and Mendel in biology. In kind, however, it perhaps resembles more closely the extraordinary integrating achievement of Einstein in the field of physical science.

SONNET—TO SCIENCE

Science! true daughter of Old Time thou art!
Who alterest all things with thy peering eyes.
Why preyest thou thus upon the poet's heart,
Vulture, whose wings are dull realities?
How should he love thee? or how deem thee wise,
Who wouldst not leave him in his wandering
To seek for treasure in the jewelled skies,
Albeit he soared with an undaunted wing?
Hast thou not dragged Diana from her car?
And driven the Hamadryad from the wood
To seek a shelter in some happier star?
Hast thou not torn the Naiad from her flood,
The Elfin from the green grass, and from me
The summer dream beneath the tamarind tree?

EDGAR ALLAN POE

[*Reprinted in commemoration of the death of Edgar Allan Poe in 1849—one hundred years ago—in Baltimore, Md.*]

COSMIC RAY MEASUREMENTS IN ROCKETS

GILBERT J. PERLOW

Dr. Perlow (Ph.D., Chicago, 1940) is in charge of cosmic ray work in rockets at the Naval Research Laboratory, where he has been engaged in research since 1942.

THE cosmic radiation is often studied most fruitfully from a vantage point high in the air. With the relatively simple apparatus available in the early days of the science, the very existence of a cosmic radiation was the subject of considerable controversy. Radioactivity from the ground was known to produce similar effects. The early balloon flights, which demonstrated an increasing radiation with altitude, rather than the reverse, clearly demonstrated the extraterrestrial nature of what was observed. At the present time it is known that the rays that impinge on the upper atmosphere are the remote ancestors of those which strike the ground. In the description of the intervening generations, it is convenient to imagine the atmosphere replaced by a sea of water of such depth that it contains the same mass as the air it is pictured as replacing. The water is then 10.3 meters deep. It appears that, on the average, the primary rays impinging on the top surface are reduced in number by half by the time they have traversed the first meter; the remainder are reduced by half in the next meter, and so on. The chance is then less than one in a thousand of a primary reaching the ground. In the process of attrition, the primary produces secondaries of several types. In their continued flight these in turn produce a third generation by either collision-induced reactions or by radioactive decay. Just what will be found at each depth depends on the complex genealogy of each species and its rates of production and absorption.

A rocket is not the most suitable laboratory for the study of the radiations in the actual atmosphere. It passes through in too short a time. In a typical case, at the end of one minute a V-2 is about 10 cms water-equivalent from the top of the atmosphere, and in thirteen additional seconds has covered the region from 10 cms to 1 cm. The chief virtue of the rocket is that it spends perhaps five minutes above this height, where the atmospheric effects are not believed to be very important from the standpoint of cosmic rays. The experimenter, if he is sufficiently clever, may profitably pursue two lines of research. First, by suitable apparatus, he may classify the incoming rays by type and by number of each type. It is believed that the primary radiation contains more than one component, although protons presumably form the major constituent. Second, he may equivalently replace a portion of the atmosphere by absorbing material carried as part of his experimental equipment and in this material study close at hand the intricate reactions which the primaries undergo in traversing matter. The first research is important to geophysics and astrophysics. It leads to knowledge of the magnetic fields of the earth, sun, and the galaxy; of the mechanism of production of high-energy radiation; and to many other things. The second is substantially a study of nuclear physics of high energies. The transmutation of the primary particles is due to interaction with the particles making up the nuclei of the atoms of the absorber.

Rocket research in cosmic rays and in other fields was advanced several years by the capture of a quantity of German V-2 rockets and parts at the close of the war in Europe. The Department of the Army undertook to make a number of these available for research on the upper atmosphere and to launch and track V-2s instrumented by several scientific groups. A great variety of experiments has been done, and more are scheduled. In addition to the V-2s, developments were started by the Naval Research Laboratory and by the Applied Physics Laboratory of The Johns Hopkins University for other types of rockets. These have culminated successfully in the Viking and in the Aerobee rockets. At the present time all three types are in use, but the supply of V-2s is approaching exhaustion.

The problems of instrumentation may best be understood after a description of the rockets themselves. The V-2 is a large device. It will carry a ton of scientific instruments to a height of 100 miles, more or less. This may be done provided, first, that the instruments will fit in the available space and, second, that they may be so disposed that certain rules are not violated concerning the position of the center of mass and thereby the stability of the rocket. A third requirement is that the instruments be mutually compatible. A powerful radio transmitter may not be placed close to a sensitive radio receiver, nor a cosmic ray experiment submerged by undesirable absorbing mate-

rial. The result of these conditions is that rocket research is successfully conducted on a much more modest scale per rocket than might be conceived.

The Aerobee is a considerably smaller rocket than the V-2. Its useful load is limited to less than 200 pounds, and its peak altitude is in the neighborhood of 70 miles. The same limitations of size, center of mass, and compatibility of equipment apply, but with greater stringency because of the smaller size. On the other hand, the Aerobee is easily handled and launched and is not expensive to build. The experimenter, therefore, feels more free to occupy its useful space with only one or two experiments. For present cosmic ray investigations, the altitude is adequate.

The Viking is again a large rocket—as long as the V-2 but of smaller diameter. It is designed to carry a load of about 1,000 pounds to the same height as the V-2. The limitation on position of the center of gravity no longer appears, and therefore greater height may be achieved with reduced load. For cosmic ray work, its size and permissible load allow considerable flexibility of apparatus design. Most important, however, it possesses a mechanism for maintaining a constant heading in space for a greater time than the other two. As this is being written (September 1949) the first cosmic ray experiments for Viking are being prepared.

The cosmic ray physicist has a number of choices of techniques for rocket research. Almost any type of detection instrument used on the ground may (with suitable modification) be used in the rocket. The simplest is a single Geiger counter. It responds to all electrically charged rays that may penetrate its walls. The counting rate is in general fairly high and is therefore scaled down electronically and transmitted to the ground by radio telemetering. The Applied Physics Laboratory of Johns Hopkins has made extensive use of the single-counter technique.

One may obtain less, but more detailed, information by arranging the counters in a "telescope." In this case a set of counters is disposed so that a single ray may actuate several of them. The occurrence of such an event is indicated by a coincidence in the time of discharge of the several counters traversed by the incoming particle. The direction of arrival of the ray is determined within the angular aperture of the telescope, in the same way that light may be collimated with a "telescope" of slits. Having isolated a cosmic particle, one may now proceed to interrogate it as to its properties. By placing a lead absorber below the telescope and observing whether any one of a tray of counters below the lead discharges coincidentally with the telescope counters, a very simple property, the penetrability, may be tested. Figure 1 is a schematic diagram of two such apparatuses. The one marked *"EXP II"* shows counter groups *a, b,* and *c* in coincidence, forming the beam-defining telescope. Two lead absorbers, each of 2-cm thickness, were disposed below in the incident beam. Counter trays *d* and *e* detected whether penetration occurred in each absorber. The counters x_1 and x_2 formed an anticoincidence "screen." They must not simultaneously discharge with *a, b,* and *c* if the event is to be considered real. If they did so discharge, counters *a, b,* and *c* may have been actuated by a shower of particles produced in the neighboring rocket structure. By means of this and similar experiments, it has been shown that the region above the atmosphere is traversed by numerous secondary particles produced in the atmosphere itself. Those which originated at an opposite magnetic latitude and equal longitude in the Southern magnetic hemisphere can enter the apparatus from above after a long spiral journey about a suitable line of magnetic force.

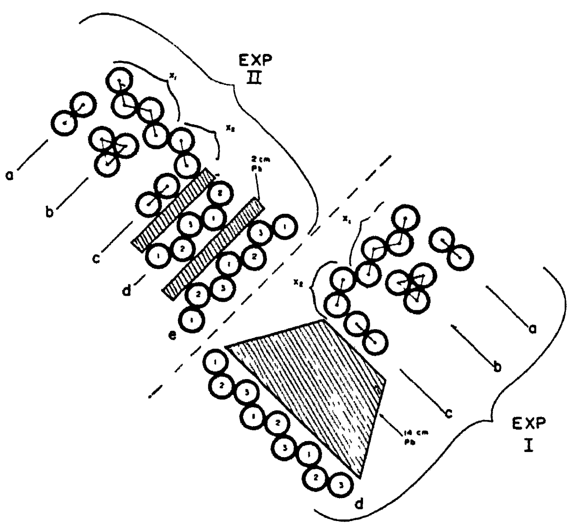


FIG. 1. Schematic representation of two counter telescope experiments flown in a rocket.

Another instrument which has been adapted for rocket use is the Wilson cloud chamber. A transparent container holding a gas and a saturated vapor is cooled abruptly by expanding its volume. The vapor condenses on the ions that a cosmic ray traversing the chamber produces along its path. The resulting track is illuminated by flash lamps and photographed stereoscopically. This done, the chamber is recompressed and allowed to attain equilibrium before the cycle is repeated. With the chamber we have used in the rocket, the time between expansions is about twenty-five sec-

onds, and about 14 exposures may be made. Eleven of these will be at heights above any important thickness of air. We may expect perhaps 3 useful tracks per expansion in this region, giving a total of 33 tracks per flight. In order to determine anything significant from the photographs, plates of material are placed in the chamber and the reactions of the incoming rays studied by examining what emerges from the plates. Figure 2, taken at an altitude of 145 kilometers, shows a dense track of a ray. From comparison of the density of the track with that of its neighbors, and from consideration of its penetration through the two lead plates, it may be deduced that it is most probably due to a nucleus of an atom heavier than hydrogen.

The rocket-borne cloud chamber possesses curious virtues over its ground-based prototype, and also certain equally curious disadvantages. These arise from the circumstance that the rockets in use at present burn all their fuel in about one minute. At the end of this time there is no thrust from the motor and also, due to the altitude, very little atmospheric drag. The rocket may then be thought of as being in "free fall," the only appreciable force on it being that of gravity. In the rocket's frame of reference the gravitational force of the earth vanishes and with it all effects due to this force. On the ground, the experimenter is frequently troubled with temperature gradients in his cloud chamber. These produce density gradients in the gas and the familiar gas motion called convection Wavy and otherwise distorted tracks result. In free fall the density gradients still exist, but not the convection, and if other sources of irregular gas motion are absent, the visible track represents the true path of the particle. Also, since the tracks do not appear to fall in the chamber, photographing them may be done somewhat more leisurely, and certain additional benefits ensue. The chief disadvantage arises from an incompletely understood property of the chamber. On each expansion a certain number of electrically neutral "condensation nuclei" are apparently formed Droplets condense on them and in a well-designed chamber are responsible for a slight fog. In the laboratory apparatus the droplets fall and remove the nuclei to the floor of the chamber. In the rocket chamber the droplets do not fall. They disappear when the chamber is recompressed, leaving the condensation nuclei unchanged in position. In several expansions the nuclei increase in number sufficiently to produce an unacceptable background of fog on each photograph. Were it not for the great density of the track of Figure 2 the photograph would be quite unusable.

FIG. 2. Cloud chamber stereo photograph at a height of 145 km. The dense straight track standing out above the background of fog and lighter tracks is a heavy primary.

FIG 3. An unsuccessful recovery.

We believe that it is possible to preserve the desirable effects of free fall and to minimize the unpleasant ones by causing the cloud chamber to spin for a short time after each expansion and thereby to centrifuge the droplets to the walls. This is shortly to be attempted.

Data from rockets may be obtained in two ways. First, radio telemetering will reproduce on the ground any measurement stated in the form of an electric voltage which does not vary too rapidly.

This is the primary means of data recovery. It obviously does not suffice if a photograph must be recovered. Fortunately this is not too difficult, either. If the rocket is rendered aerodynamically unstable by a suitable alteration of its shape in flight, the impact speed need be not much greater than 100 miles per hour. Suitably protected cameras may survive well enough to be used immediately afterwards. The alteration in shape is generally produced with explosives—for the V-2 blowing off the warhead by destroying its supports with TNT is quite effective. The recoverable apparatus is placed in some region other than the warhead. Figures 3 and 4 show a successful and an unsuccessful recovery.

A number of apparent difficulties envisioned at the start of the research program have turned out not to be serious. The recovery of film is an example. Another is the problem of vibration. Anyone who has attended a launching can testify to the terrifying racket heard even at considerable distances. How destructive the vibration must be to delicate instruments within the rocket! Actually, however, no great pains need be taken to design for it. We have used no elaborate antivibration mounting nor found any necessary. In electronic equipment, for example, ordinary good construction practice suffices. There have been relatively few failures in vacuum tubes attributable to vibration encountered in flight, even though as many as 300 have been used at one time. Of somewhat more importance than vibration, possibly, is the continued acceleration during the first part of the flight The Aerobee is the worst offender in this respect, since it is "boosted" from a launching tower by an auxiliary rocket. An acceleration of about fourteen times that of gravity takes place. Supporting members for the instruments must withstand a stress of fifteen times the static value. An apparatus weighing 130 pounds acts like a ton, and consequently a mechanical engineer becomes a highly desirable member of the research team.

The physicist who desires to do cosmic ray experimentation with rockets must count on relatively short times of flight and a number of engineering problems foreign to his previous experience On the other hand, the method holds promise of considerable reward. The technique should advance considerably in the next few years under the pressure of need for further knowledge about the interesting region above the earth's atmosphere.

FIG 4. A successful recovery.

GROWTH—NORMAL AND ABNORMAL

WILLIAM J. ROBBINS

Dr. Robbins (Ph.D., Cornell, 1915) was professor of botany, dean of the Graduate School, and acting president of the University of Missouri before he joined the staff of Columbia University as professor of botany in 1937. He has also been director of the New York Botanical Garden since that year. His article is based on an address presented before the Pacific Division of the AAAS in Vancouver on June 16, 1949.

GROWTH is an interesting subject because it is so personal. We have all grown, perhaps not as much as we'd like and perhaps in not quite the way we would prefer, but we all experience the process.

What is growth? Most people would probably answer this question by saying that when anything grows it gets bigger. But that is quite clearly not all we include in the concept of growth. A dog is not merely an enlarged puppy, a man is more than an overgrown infant, and a corn plant is more than a magnified corn grain. This aspect of growth which results in an organism assuming characteristic parts, shapes, and functions is frequently called differentiation, development, or morphogenesis, and in all but the simplest living creatures it is intimately connected with increase in size

We can clarify what growth is by considering some examples. An amoeba consists of a soft, minute bit of jelly from three hundredths to three tenths of a millimeter in diameter. Microscopic examination reveals that this jelly, called protoplasm, consists of two distinct parts—a granular, translucent outer part which forms the major portion of the amoeba and which is commonly called the cytoplasm, and a more or less spherical, less transparent portion called the nucleus. Together, these two parts make up a cell.

The single-celled amoeba lives in water and moves from place to place by a sort of flowing process. As the amoeba moves about in the water, it encounters bits of plant or animal material. It flows around these particles, engulfs them, digests them, and from the products of digestion constructs new cell substance. As a result, the amoeba enlarges, but not indefinitely. When it reaches a more or less definitely limited maximum size, it divides into two approximately equal parts, each with a nucleus and cytoplasm. The two daughter amoebae move away as independent individuals capable of growing and dividing again. If the amoebae stuck together as they grew, instead of separating and proceeding on their individualistic ways, a mass of living jelly large enough to be seen with the naked eye would develop in time.

Some of those interesting creatures, the slime molds, which may be found moving slowly over the surface and in the crevices of decaying wood or leaves, begin their lives much like the amoeba in appearance, size, and structure. As the slime mold grows, the protoplasm increases in amount and the nuclei divide. However, the new nuclei and their surrounding cytoplasms do not separate one from another; they remain together, and eventually a mass of protoplasm may develop, easily visible to the naked eye.

Growth for the amoeba and for many other simple plants and animals is primarily enlargement accompanied by relatively little increase in complexity of organization or of function. Most living things with which we are acquainted do not grow into mere masses of jelly, however—they have leaves, roots, stems, and flowers or legs, heads, and eyes

Any one of our common flowering plants, for example, normally originates in a cell in the pistil of the flower. This cell is the egg. It unites with a second, usually much smaller cell, the sperm cell, which comes from the pollen grain. The fused product of the egg and the sperm cell—the fertilized egg, let us say—is microscopic in size and structurally much like the amoeba, though not motile. The fertilized egg, located in the ovary at the base of the flower, increases in size at the expense of food drawn from the balance of the plant. It divides into a chain of cells. One of these differs from the rest in continuing its growth and forming the many-celled embryo plant. It divides in several planes, the number of cells and the total mass increasing thereby. Some of the cells produced by the division of the fertilized egg are organized to form seed leaves, some a young stem, and some the beginnings of the root; the whole comprises an embryo plant which lies within the seed.

If we plant the seed, it absorbs water. The

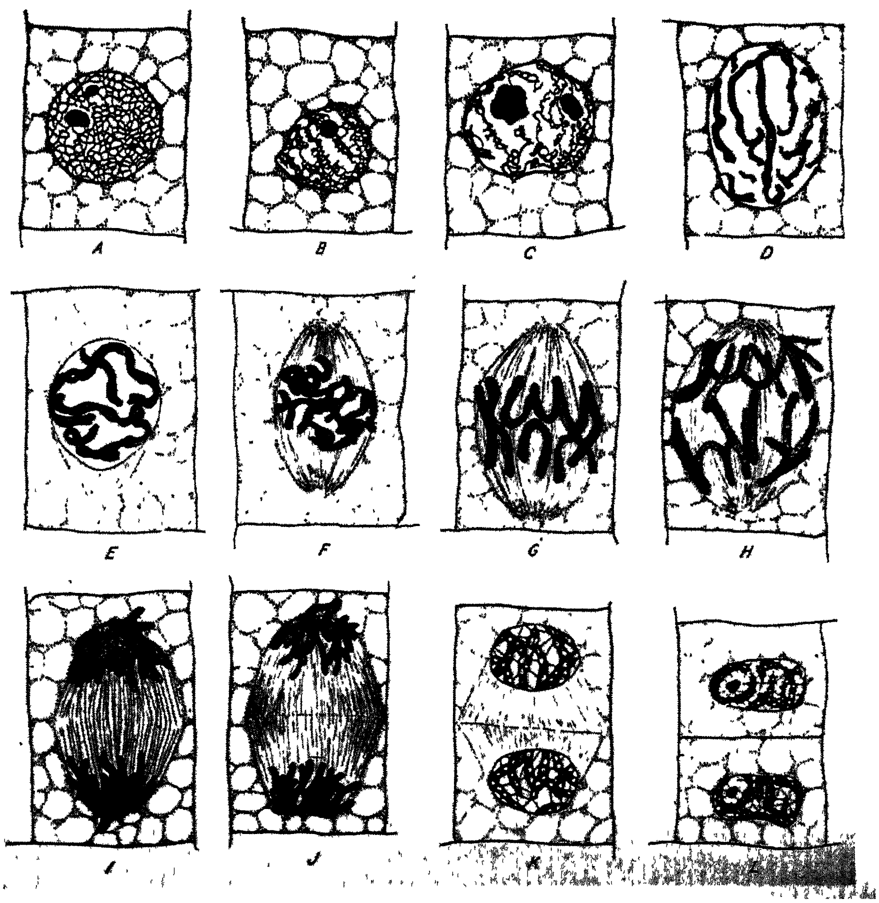
stored food is digested and used by the embryo plant, the cells of which increase in number and size. The seedling bursts its way out of the seed, establishes its root system in the soil and its stem and leaves in the air. Growth, limited to particular parts of the plant body, continues, and the result is an increase in size and the formation of new roots, branches, leaves, flowers, fruits, seeds, and other organs of the particular plant concerned.

It is by such a series of events, briefly and inadequately sketched here, that a bit of living jelly less than half the size of a pinhead originated some 3,000 years ago and grew into a giant sequoia with a trunk 30 feet in diameter and a height of more than 300 feet. During these 3,000 years, the original microscopic fertilized egg has divided into an astronomical number of cells—a cubic inch of sequoia wood contains more than one billion. During this period, development or differentiation resulted in the formation of roots, trunk, branches, needles, cones, and other parts, which by their shapes, arrangements, and specialized functions enable us to recognize this tree as a sequoia and enable the tree to continue its life and its growth and to reproduce itself.

The essential features of the growth of a seed plant are duplicated in the development of the more complex animals, including ourselves. The vertebrate animal, in common with most other living things, originates as a single cell, the fertilized egg. This divides into many cells, at first much alike in general shape and contents. However, some of these cells, which can be recognized in the early stages of development by their position relative to other cells and by their contents, form germ tissue, some form nerve tissue, some muscle and other parts of the adult body. In the growth of a living organism, we may distinguish three important phases. One of these is an increase in cell substance, another is an increase in the number of cells, and the third is the differentiation or organization of the cells into the specialized parts of the adult body. All these may occur more or less at the same time in the development of a living thing.

Let us examine each of these in more detail. How is new cell substance added to the amount already present?

A crystal of sugar grows in a supersaturated sugar solution by the addition of sugar molecules from the solution to those already present in the



Nuclear and cell division of a plant cell. Note complexity of process. (From Robbins and Rickett. *Botany*. New York: Van Nostrand.)

crystal. The added molecules of sugar are laid down in a particular pattern or crystal lattice, but it is sugar from the solution added to sugar in the crystal. In the growth of a living organism, however, the increase in cell substance involves the synthesis of new and complex chemical compounds from simpler substances. Protoplasm, cell walls, the material of bones, of shells, carapaces, hair, fingernails, and so on, are made from the food which the organism uses. The beef and bread and butter we eat today become living protoplasm or bone or hair tomorrow.

The growth of a living thing differs decidedly from that of a crystal; in fact, a living organism is an elaborate chemical factory which synthesizes or makes many kinds of chemical compounds, the most important of which are probably the nucleo-proteins. They are chemical compounds with large and complex molecules. They appear to be composed of proteins combined with nucleic acid, since they yield those substances on hydrolysis. The proteins are compounds of high molecular weights. The simplest formula of oxyhemoglobin, a protein of red blood corpuscles, is $C_{658}H_{1181}N_{207}S_2FeO_{210}$, equivalent to a molecular weight of 15,455. Compare this with the formula $C_{12}H_{22}O_{11}$ and a molecular weight of 342 for cane sugar. Nucleic acid also is a complex chemical compound. It consists of sugar, phosphoric acid, and certain nitrogenous bases. The formula for yeast nucleic acid is $C_{38}H_{49}O_{29}N_{15}P_4$, equivalent to a molecular weight of 1,303.

It is generally believed that the complex chemical compounds which make up the cell substance of a living organism are built up by steps from the simple ones which make up the organism's food; a series of intermediate chemical compounds is formed between the original simple foods and the final product, cell substance. This stepwise progression from simple to complex is made possible by a series of organic catalytic agents, the enzymes, also made by the organism, which operate on each stage as that stage is completed. Enzymes facilitate chemical reactions at the relatively low temperatures and slight acidities and alkalinities found within the bodies of living organisms.

Furthermore, although synthesis is likely to be emphasized in considering growth, there are other processes involved which are catabolic in character. Some foods must be digested; some chemical compounds must be broken down in the body so that energy may be transferred. The catabolic processes of digestion and respiration also occur in steps and are made possible by a series of enzyme systems.

Any substance that plays a necessary part, directly or indirectly, in the chain of reactions which end in the synthesis of new cell substance is called an "essential metabolite." Unless each essential metabolite, each chemical substance in the stepwise construction of cell substance, each enzyme which facilitates the chemical reactions concerned, is made within the organism or supplied from without, the series is interrupted, new cell substance is not made, and growth does not occur.

Organisms differ in their self-sufficiency as far as essential metabolites are concerned. The most completely self-sufficient would be one capable of constructing its cell substance from the elements—carbon, nitrogen, hydrogen, oxygen—and various metals like iron, manganese, cobalt, and so on. I know of no organism so self-sufficient as this. Any one of them requires at least some of the elements to be in a combined form.

A green plant has relatively simple requirements. It can make all its essential metabolites from water, oxygen, carbon dioxide, and mineral salts, including nitrates.

Aspergillus niger, a common black mold which does not have the green pigment chlorophyll, will grow in a solution of sugar and minerals. From these simple materials it can construct all the essential metabolites concerned on the way to the final product, *A. niger.*

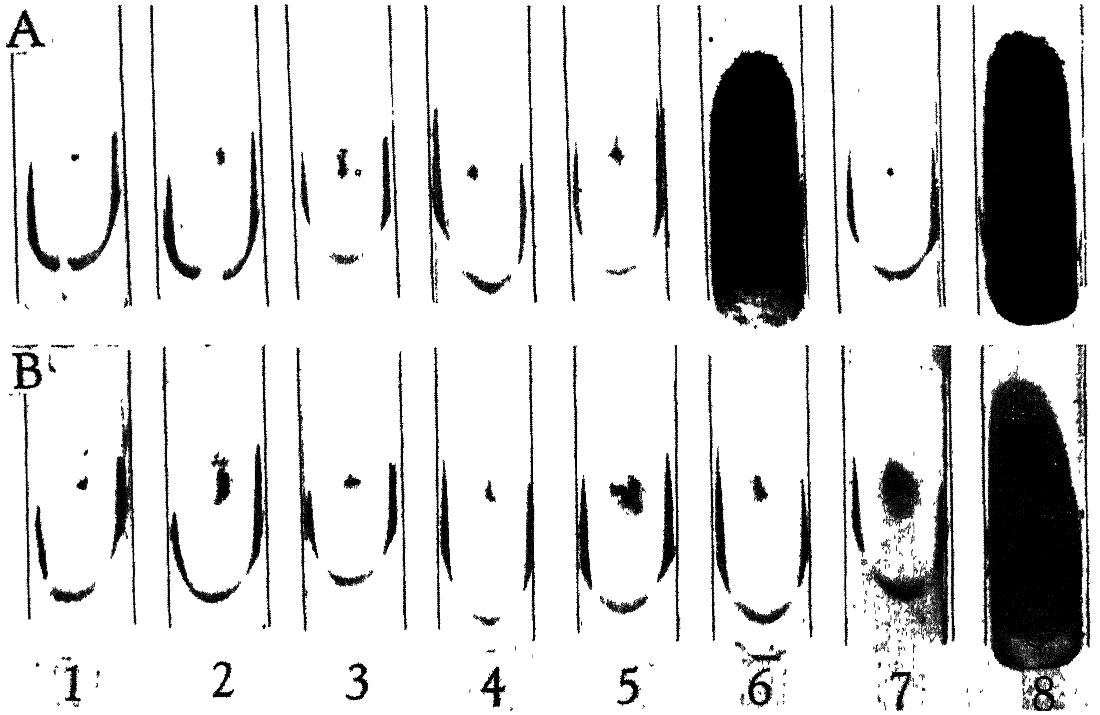
Phycomyces blakesleeanus, the black bread mold, is able to make from a solution of sugar, asparagine, and mineral salts all the essential metabolites it needs except one. Its metabolic machinery is complete except for one part. That one is the vitamin thiamine; unless minute amounts of thiamine are supplied to this fungus, it will not grow.

There are a good many living things with parts missing in their metabolic machinery. They have to be supplied with one or more vitamins, with organic bases such as adenine, with particular amino acids, or with other substances. Organisms with such deficiencies do not grow unless the food with which they are furnished contains the essential metabolites they require but are unable to make Our own metabolic machinery has a good many missing parts. We are unable to construct in our own bodies several of the amino acids, a number of vitamins, and some other substances. Not only does normal growth fail to occur, but various deficiency diseases may develop if these missing essential metabolites are not in our food. We depend for the missing essential metabolites upon other living things that are able to make them.

The concept of growth as a series of catalyzed

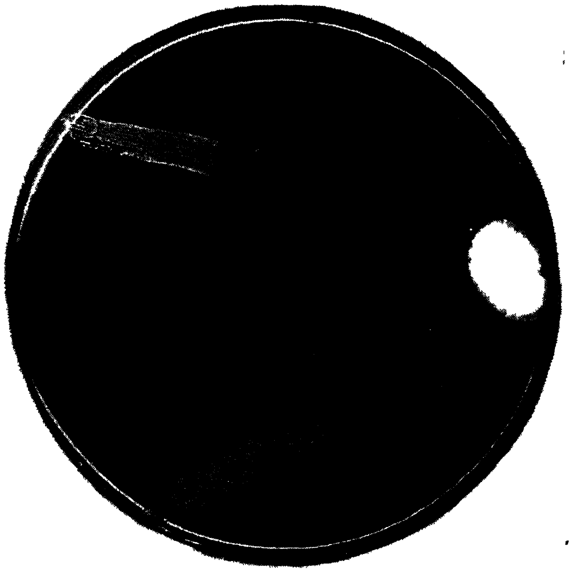
reactions, each one of which must be completed before the next one in the series can occur, is fundamental for the understanding of the principles of nutrition. It emphasizes the importance of an adequate diet, adequate not only in quantity (calories) but also in quality (containing the needed essential metabolites). It is important in another way. Suppose that we were to furnish an organism with a chemical compound closely related to an essential metabolite but a little different from it, what would happen? The situation appears to be somewhat the same as though you were to attempt to unlock a door with a key that almost fits but does not quite work. So long as the wrong key was in the keyhole, you could not insert the proper key and unlock the door. So chemical compounds which resemble but are slightly different from an essential metabolite (these are called analogues) appear, in some instances, to insert themselves in the series of reactions and interfere with the growth of the organism. The sulfa drugs, penicillin, streptomycin, and other similar substances probably work in this way. They displace essential metabolites and interfere with growth.

For example, pteroylglutamic acid (folic acid) is an essential metabolite for some organisms. It has a molecular formula of $C_{19}H_{19}O_6N_7$ and consists of a combination of glutamic acid, para-aminobenzoic acid and a pteroyl group. If an NH_2 group is substituted for OH on the pteroyl portion of the molecule, the resulting compound, aminopterin, is poisonous; one part per million in the food of such animals as mice is toxic. Yet the toxic compound differs but slightly from the beneficial one; it has one more nitrogen atom, one more hydrogen, and one less oxygen. There is a close relation between chemical structure and biological action.



Vitamins required for growth. Growth of two fungi, *A, Ceratostomella penicillata,* and *B, C. microspora,* on a mineral-dextrose medium containing asparagine and purified agar supplemented as follows: (1) no additions, (2) thiamine, (3) pyridoxine, (4) biotin, (5) thiamine and pyridoxine, (6) thiamine and biotin, (7) pyridoxine and biotin, (8) all three vitamins. Forty days old Note fungus *A* grows only when supplied with thiamine and biotin; fungus *B* requires thiamine, pyridoxine, and biotin.

The investigation of the effects of the analogues of essential metabolites upon the process of growth is an important and fruitful field of research. We may assume that the chemical steps in growth are somewhat different in different living things; otherwise all of them would look alike. We might expect to find substances which would upset the growth process in one organism and not in another, or in one type of growth and not in another. It is easy to understand why a good deal of time and money is being spent in searching for chemical compounds which may interfere more with the



Growth inhibition. Fungus colony at right has produced materials which inhibit the growth of the bacteria streaked up to the fungus colony across the food material in the dish. (Courtesy Annette Hervey, The New York Botanical Garden.)

development of a disease organism than with the host on which it lives, more with the growth of cancer tissue than with normal tissue.

The synthesis of new cell substance results not only in an increase in the total mass of the organism but in an increase in the number of cells by cell division. Cells do not keep increasing in size, they divide. In fact, the size of the cells of various organisms is relatively constant. The cells of a mouse are not very different in size from those of an elephant.

Cell division, as it usually occurs, is a complex process, particularly for the nucleus. In its essential features, it appears to be much the same for all living organisms. During cell division the nucleus fragments into a number of rods, the chromosomes, which by a regularly occurring series of events are equally distributed between the two new, or daughter, cells. The result of cell division is the formation of two cells from one, and each of the new cells has received half the substance of the chromosomes formed from the original nucleus. In ordinary cell division, there is an orderly and equal distribution of heritable material from one cell generation to the next. All the somatic cells of the multicellular organism are probably alike genetically.

This marvelous and complicated process, repeating itself time after time with wonderful precision, occurs whenever new cells are formed and takes place in the larger plants and animals millions of times during their growth from a single cell to a mature individual. An average-sized potato, such as one might eat for dinner, has between five and six billion cells. Cell division must have occurred in its growth between five and six billion times, each time without an error.

Associated with the increase in the amount of cell substance, and in the number of cells, is the development of the organs and parts of the body and of the cells that compose them. Differentiation is a name frequently applied to that phase of growth which causes us to develop into the individuals we are and prevents us from becoming gigantic slime molds, a mere mass of 100 pounds or more of quivering jelly. It follows a definite rule, or pattern, with each kind of living thing; the frog's egg always grows into a frog and not into a dog or chicken; our nose always grows on the front of our face and not between our shoulder blades. Differentiation not only results in the characteristic organization of the parts of the body but in a limitation on the size of the individual and the various organs. We don't grow forever.

What causes differentiation, any kind of differentiation, and what causes it to follow a definite rule? Why do we always grow two arms and not six or seven? Why do we grow hair on the top of our head instead of on the soles of our feet? Why don't we keep enlarging during our entire lifetime? There are a number of answers; none is entirely satisfactory.

White spots are rapidly growing mutants which have originated spontaneously from the slow-growing fungus (*Trichophyton mentagrophytes*) which covers most of the surface of food in the dish.

Abnormal growth. *Left:* gall on *Solidago* sp. caused by the solidago gall moth; *right.* gall cut open to show larva of the gall moth.

One answer is that there is a vital force or principle which guides and shapes the clay of which we are made. This force is supernatural, and, because it is supernatural, it is beyond the limits of inquiry by natural means which we, as natural and not supernatural beings, must employ. The common biological answer is heredity, which means that we received a certain kind of protoplasm from our parents that develops into the creatures we are. In other words, the particular cell from which each of us starts, using the word "us" in a broad biological sense to include every living individual, has a particular kind of living stuff which carries on a particular series of chemical reactions and physical processes which work out into the characteristic structure of the individual.

We might use an analogy to make this point of view clearer. Suppose we take a small cone of mercury sulphocyanate and set fire to the tip. The burning cone forms a long serpentlike ash one hundred times or more the volume of the original cone. This is the familiar fireworks called "Pharaoh's Serpent." The growth of the serpent depends upon the kind of material of which the cone is made. It must be mercury sulphocyanate; charcoal, sulphur, gunpowder, or sawdust will not serve. It depends also upon the kinds of chemical reactions which the mercury sulphocyanate undergoes. Only burning—that is, oxidation—will form the snake; such reactions as reduction or combination with other compounds than oxygen will not produce the same result. Even though the cone is made of mercury sulphocyanate and it is burned, a serpent will not be formed unless the cone burns from the tip. If it burns at the same rate over all its surface, some other kind of monster will grow out of it. This means that certain physical conditions also must be met.

So we may picture the growth of a cell into an adult individual as depending upon a particular kind of living stuff carrying on a particular series of chemical reactions and physical processes which work out into the characteristic structure of the individual. The chemical constituents of the living stuff in a cell are, of course, much more complex than that of mercury sulphocyanate, and the chemical reactions and the physical processes concerned in growth, even in the growth of the simplest creature, are infinitely more numerous and complicated than those which take place in the oxidation of the sulphocyanate. There are other differences also. For example, protoplasm is not used up as the sulphocyanate is; it recreates itself.

One of the most astounding things about growth is that the complex series of reactions and the numerous physical processes that are involved in the construction of new cell substance, in the divi-

sion of cells, and in the organization of those cells into the adult organism occur again and again according to the same pattern for each kind of organism. In other words, growth is usually sufficiently normal so that each of us can be recognized as members of the human race even though we may not be an Apollo or a Venus.

We should not be surprised that the series of chemical reactions involved in the construction of new cell substance or the complex process of cell division should on occasion fail to occur according to the normal pattern and result in abnormal growth. In some instances we understand fairly well what is concerned in abnormal growth. Examples of abnormal growth that are associated with too little thyroxine are cretinism or myxedema. Acromegaly, in which the bones of the skull, jaw, and chest are enlarged, is associated with the amount of substances secreted by the pituitary gland. Cancer is a localized abnormal growth, the basic causes of which are not known.

There are many kinds of abnormal growth in the plant kingdom The development of a potato tuber, a swollen engorged portion of the underground stem of the potato, is normal so far as the potato plant is concerned but abnormal if we consider the major portion of the potato stem. So-called bud sports, in which a portion of the plant differs from the balance as the result of some slip in the accuracy of the division of heritable material between daughter cells, is another type of growth abnormality. Advantage is frequently taken of such bud sports for the origin of new kinds of horticultural or agricultural varieties. Spontaneous mutants that develop in cultures of bacteria, yeast, filamentous fungi, and many other organisms may give rise to strains which evidence growth patterns quite different from those of the parent. The cause of such spontaneous mutations is, in many instances, not well understood. Insect galls are common on plants. These are characterized by an abnormal but specific growth pattern super-

Modification of growth pattern by a specific chemical. *Left:* normal *Kalanchoe* plant; *right: Kalanchoe* treated on stem with lanolin containing 3.2 mg of para-chlorophenoxyacetic acid per g lanolin. Terminal leaves of the treated plant have formed a cup-shaped organ. The pair of leaves below the cup have smooth edges and are cup-shaped at their bases. The ortho or meta form of the chemical has no such effect. (Courtesy Percy W. Zimmerman, Boyce Thompson Institute for Plant Research.)

Abnormal growth. Discs of carrot tissue grown four weeks on sucrose-mineral salt agar. *Right* and *left*: disc inoculated with bacteria (*Agrobacterium tumefaciens*). Center disc not inoculated. Note tumors on inoculated discs. (Courtesy R. S. de Ropp, The New York Botanical Garden.)

imposed on a normal tissue by a foreign living organism. It is highly probable that the abnormal growth is the result of the production of minute amounts of specific chemical compounds by the larva which inhabits the gall. It would be interesting indeed to identify these chemical substances and to learn what their effects might be on plants other than those on which the larvae feed.

Root nodules are caused by the legume bacteria; club root on cabbage and other crucifers is the result of infection by *Plasmodiophora brassicae,* a myxomycete; calluses grow from the cut ends of stems or from wounds. Fasciations, spontaneous or induced, represent still another type of abnormal growth. Treatment with specific chemicals, often in minute amounts, may induce massive tumorlike overgrowths, or profoundly modify the development of the plant.

In some respects, the most intriguing types of abnormal growth in the plant kingdom are those plant tumors which are characterized by a more or less unorganized and uncontrolled growth The overgrowths caused by *Agrobacterium tumefaciens,* and generally referred to as crown gall, have been studied for many years. Such tumors can be transplanted to normal tissue, where they will grow as tumors. The tumor type of growth is retained by the tissue after the causative bacteria have disappeared, and such bacteria-free tissue can be grown indefinitely on simple media on which the normal tissue cannot be cultivated. What do the bacteria do which upsets the normal course of growth, which causes cells that would normally grow into stem tissues and stop growing, to remain embryonic and continue to grow indefinitely in a relatively uncontrolled fashion? Why does the tumor tissue not revert to its normal pattern and become stem when the bacteria are eliminated? Is there any way in which the tumor tissue can be induced to return to the normal pattern of growth—in other words, differentiate and cease to be tumor tissue? These are questions of a fundamental character, answers to which might tell us why cells of the normal animal body at times grow wild and form benign or malignant tumors, and suggest methods of prevention or of cure.

Similar types of tumors develop on plants infected with the wound tumor virus, especially at points of injury. Tumors capable of transplantation in vitro develop also in hybrids formed by crossing *Nicotiana langsdorfii* and *Nicotiana glauca.* These tumors are not the result of infection by a virus, by bacteria, or by any other causative agent, but result from the hybridity.

Growth, then, is an extremely complex process which, in its fundamentals, is much the same in the simplest and in the most highly developed organisms. We can learn a great deal about the growth of a human being from the study of the growth of bacteria, yeasts, protozoa, or molds. We are accumulating knowledge of the substances living organisms must have so that they can grow, and the changes these substances undergo in becoming cell substance. We have learned, or are learning, much about various agents, including specific chemical compounds which stop growth or modify it, and how to use them. Our knowledge, however, is still fragmentary and incomplete. A great deal more must be learned of the physics and chemistry of growth before we can expect to solve some of the problems of growth, which include, among others, that most important one, cancer.

SPACE HEATING WITH SOLAR ENERGY*

MARIA TELKES

Dr. Telkes (Ph D., Budapest) is a native of Hungary During her high-school days she became interested in the utilization of the sun's energy, and, after doing research at the Westinghouse Research Laboratories in this country, she joined the Solar Energy Conversion Project at MIT in 1939. During the war she developed solar distillers for life rafts, producing potable water from sea water. Her interest in the heating of houses with solar energy is responsible for the construction of the first experimental house heated by sunshine alone.

SPACE heating consumes nearly 30 percent of the fuel used in the United States. The yearly value of this fuel has been estimated at 3,500 million dollars. In other parts of the world an even greater percentage of the fuel produced is burned to maintain comfortable temperatures. Scarcity of fuel depresses the standard of living, and the availability of fuel is one of the most important factors in the progress of civilization.

Is it necessary to rely on fossil fuel alone? Would it be possible to eliminate the difficult task of mining and transporting coal or collecting wood? It is well known that fossil fuel is the result of photosynthesis during geological ages, whereas wood is the product of the contemporary action of solar energy. The forests utilize less than 0.1 percent of the solar radiation incident upon them. Even under the most favorable conditions, the efficiency of photosynthesis is less than 2 percent.

The important question is: Can we compete with nature in capturing and storing solar energy with a much greater technical and economical efficiency? Is it possible to solve the task of space heating by using the omnipresent solar power, replacing—where feasible—fossil fuel with solar energy as a new fuel resource?

The available amount of solar energy. Weather Bureau records give the number of sunshine hours during each month for a large number of locations, and quantitative data have been collected at a limited number of observing stations. The amount of solar energy varies considerably; it is not a simple function of the geographical latitude. Table 1 gives a very broad estimate and serves merely to show the magnitude of the available energy.

TABLE 1

SOLAR ENERGY RECEIVED ON ONE SQUARE FOOT OF SURFACE, DURING AN AVERAGE DAY AT OPTIMUM INCIDENCE

	B T U.	Fuel Equivalent in Pounds of Coal
Tropical, yearly average	3,000	0 25
Temperate Zone:		
(Lat. 35°–40°, U S.A.)		
Summer, average	2,000	0.17
Winter, maximum	2,000	0.17
Winter, average	1,000–1,500	0.08–0 12

For the purpose of evaluating the possibilities of solar space heating, let us consider a small home, receiving 1,000–1,500 B.T.U. per square foot during an average winter day, with a heat load of 400,000 B.T.U per day, corresponding to conditions of 1,000 degree-days per month. These are typical conditions for latitude 35°–40° in the United States. If it were possible to use for space heating at least 50 percent of the average winter solar energy—that is, 500–750 B.T.U. per square foot during an average winter day—the above-mentioned home would require a solar energy collecting surface of 500–800 square feet. Such a surface cannot be regarded as excessive, and it may easily be incorporated in the roof or south wall of the house. In warmer locations the solar energy collecting area could be correspondingly smaller.

Previous experiments with solar heating. During the past twenty years solar water heaters have become increasingly popular in Florida and California. In these states clear weather occurs nearly 70 percent of the possible time, and a cloudy day is seldom followed by another cloudy day. The water heaters consist of a collector of solar energy, mounted on the roof, and an insulated storage tank large enough to store at least two days' supply

* Based on an address presented before the United Nations Scientific Conference on the Conservation and Utilization of Resources, Lake Success, New York, August 17–September 6, 1949.

of hot water. The collector is a well-insulated flat box, covered with one or two air-spaced glass panes, to transmit solar energy, which in turn is absorbed by a thin black metal plate with water-circulating pipes soldered to it. According to various reports, the efficiency of such heaters is rather high in Florida and California. If properly designed, the solar heaters can convert at least 50 percent of the incident solar energy for the pupose of heating water. The most difficult problem is the need for storing enough hot water for the inevitable cloudy days.

Experiments incorporating a large water heater in the roof of a test structure have been conducted at the Massachusetts Institute of Technology in Cambridge, Massachusetts, since 1940. It was found that the two-room structure could be heated during the winter, but that this required an excessively large storage tank. Similar experiments are now in progress in a small home located near the Institute, using a roof-type water heater with a storage tank capable of accumulating a two days' supply of heat, with additional electrical heating provided for the inevitable sequences of cloudy days. Other tests have been carried out in Switzerland.

In Colorado another home used crushed rocks as the heat-storage medium. Solar heat was collected on the roof, behind air-spaced glass panes, and the warm air was circulated through the house, or through an insulated compartment filled with heat-storing rocks. This house used a conventional fuel-burning furnace because solar heat could be stored only overnight.

South-facing windows transmit considerable amounts of the low-slanting rays of the winter sun, and the use of such large, south-facing windows has recently become popular. These architectural "solar houses" may collect a great deal of solar heat during clear winter days, often overheating the house. The gain is rapidly lost at night and on cloudy days, and consequently a true "net gain" is probably limited to warmer climates.

The efficiency of solar heat collection. The type of solar heat collector used for water heating, if properly designed, can be very efficient. The thin, black-painted metal plate absorbs solar heat and transfers part of this heat to a circulating medium. Air-spaced transparent panes serve to diminish the outward heat losses, owing to reradiation and convection. The number of such "heat-trapping" panes is limited, because there is a loss of 10 percent, due to reflection, on each transparent pane. The optimum number of panes appears to be two, transmitting about 80 percent solar energy at near optimum incidence. If reradiation and convection losses are limited to 30 percent, the balance of 50 percent is the "net gain;" that is, the collector utilizes the incident solar energy with an efficiency of 50 percent.

These figures should be considered as average figures throughout the day. Solar radiation is of variable intensity, lower during the early morning and late afternoon than the noontime peak; therefore, higher efficiencies will be observed during the noon hours. The reradiation and convection losses increase with the temperature difference between the collector plate and the outdoor air. Higher temperatures of collection—at a high enough efficiency—could only be reached with solar energy of higher intensity. It appears, therefore, that better collector efficiency can be attained only with collectors operating at a moderate temperature, preferably below 110° F.

Solar heat storage, the critical problem. The need for storing solar heat, not only overnight, but also during a sequence of cloudy days, is obviously a critical problem. If the collector temperature is limited to a 110° F. maximum for reasons of collection efficiency, the temperature of heat storage must be lower. For space heating, an average indoor temperature of 70° F. is required, and therefore the temperature of heat storage must be greater than this value; consequently, the temperature change of the heat-storage medium will be limited to a rather narrow range, possibly not more than 20° F.

Using the specific heat of water for heat-storage purposes, it is probable that not more than 20 B.T.U. can be stored effectively per pound of water during one winter day. The specific heat of other materials (rocks, etc.) is lower than that of water, and their heat-storage capacity per pound will be lower, too.

Fortunately the specific heat effect is not the only heat-storage possibility. The heat of transformation, or heat of fusion of chemical compounds, appears to offer much higher heat capacity for storage. Several chemical compounds, or mixtures, are available, with heat-storage capacities in excess of 100 B.T.U. per pound of material. Some of these compounds melt within the 90°–100° F. temperature range and are readily available at a very low cost. Typical materials are, for instance, sodium sulphate decahydrate $Na_2SO_4 \cdot 10H_2O$, melting at 90° F., or disodium phosphate dode-

cahydrate Na_2HPO_4 $12H_2O$, melting at around 95° F. There are several other suitable mixtures.

The amount of heat required to melt these compounds is stored in them; when heat is abstracted, the materials recrystallize. The process of fusion and resolidification can be repeated continuously. The chemical compound is placed in closed containers, and it never needs to be renewed. Some of these materials are capable of storing eight to ten times more heat than is possible with water when equal volumes are compared. The use of these heat-of-fusion materials therefore diminishes the heat-storage volume required for space heating with solar energy. Figure 1 shows one of the possible solutions of arranging a solar energy collector and the heat-storage material, assembled in a "heat bin."

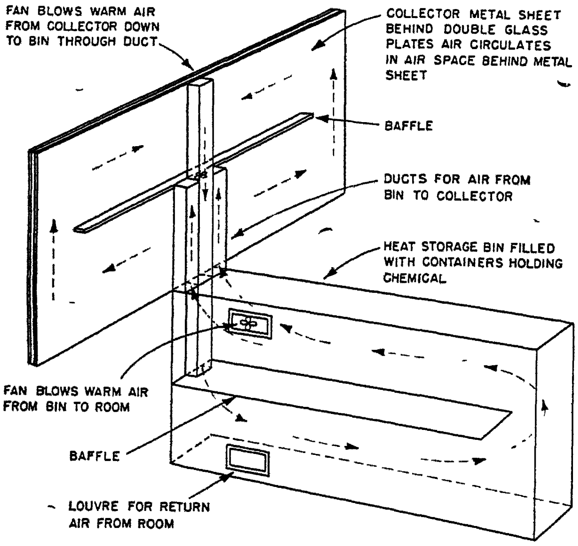


FIG. 1. Heating system, sun-heated house, Dover, Mass.

Although the heat of fusion is a definite physical constant, it is not sufficient merely to fill containers with this material. Several problems must be met, such as the prevention of corrosion of metallic parts, the promotion of recrystallization by preventing undercooling, and the most advantageous shape factors for efficient heat transfer. This form of heat storage is basically a heat-exchange problem, based upon the first law of thermodynamics.

Experimental house at Dover, Massachusetts. In December 1948, an experimental house, located in Dover, Massachusetts (15 miles from Boston), was completed. The house uses the heat-of-fusion principle for solar heat storage. The south-facing vertical collector of the 720-square-foot area is located in the attic of the house. Air warmed by solar energy is circulated by fans to the heat-storage units, "heat bins" located between the rooms. Figure 1 is a diagram of the heating system, and Figure 2 is a view of the house.

The total volume of the chemical mixture used is 470 cubic feet (3,500 gallons), and its weight is 21 tons, capable of storing about 4 million B.T.U. at 88°–90° F. The house has a volume of 10,000 cubic feet, and the average winter heating requirement is 400,000 B.T.U. per day; therefore, the completely charged "heat bins" should be capable of providing space heating for ten consecutive sunless winter days.

The heat is transferred from the storage units into the rooms partly by radiation through the walls of the bins and partly by circulating the air of the rooms through the bins. It is obvious that several other locations for the collector and the storage units are equally possible, as well as other systems of heat transfer. Designed by the architect Eleanor Raymond, the house has several south-facing double windows to make use of additional winter sunshine. This project has been sponsored by Amelia Peabody of Boston, and it is not connected with the work at Cambridge.

Preliminary data, collected during the past February, indicate that the collection efficiency during the entire month was 41 percent of the total incident solar energy. During this month there were ten days when no heat could be collected at all. The total amount of solar energy recorded during these ten cloudy days was less than the amount received during an average clear day. The collection ranged in efficiency from 45 to 60 percent during clear days, but it was lower on partly cloudy days. The longest sequence of cloudy days was five, which is in accordance with weather statistics in this vicinity.

During the summer the ducts of the collector are opened, and the cool night air is circulated through the storage system, lowering its temperature. The specific heat capacity of the solidified chemical compound is sufficient to keep the house comfortably cool during the warm summer days. In this way the winter storage system can be operated in reverse during the summer.

Future development. Compared to the efforts made in the past during the development of efficient space heaters using fossil fuel, the efforts made so far in the utilization of solar energy are infinitesimal. Considerable research and development work are probably needed before solar space

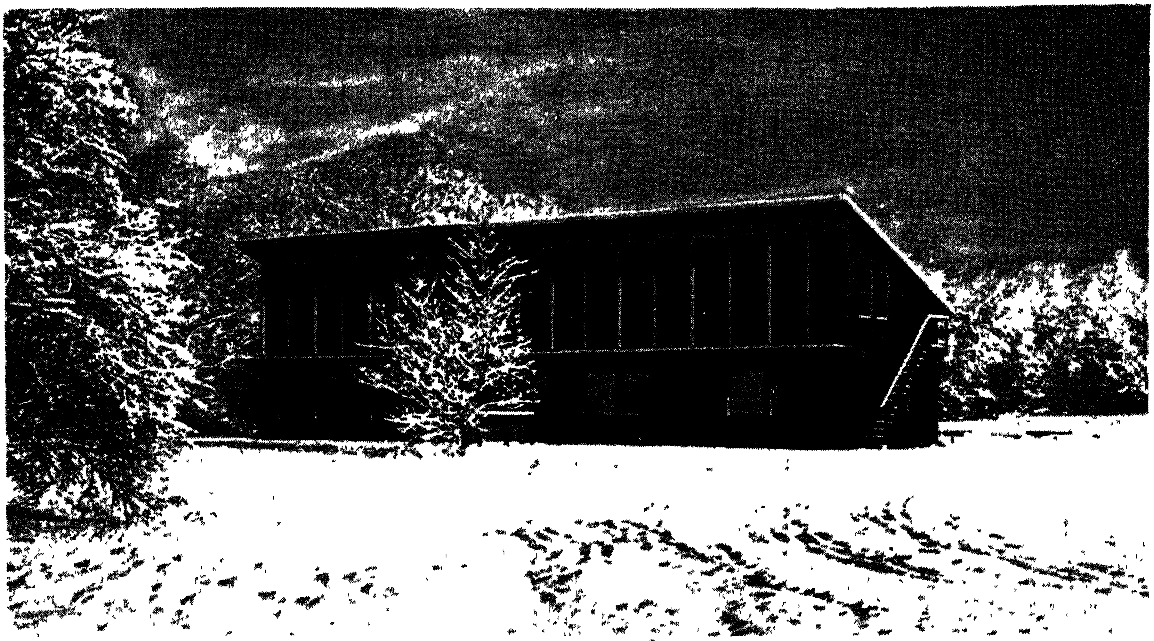


FIG. 2 Sun-heated house designed by Eleanor Raymond. (Project sponsored by Amelia Peabody.)

heaters will be readily available for general use, but the basic principles have been established, and the trend of development is clearly indicated.

The use of solar space heating offers numerous advantages. The relatively low temperatures encountered during its operation preclude any fire hazards. There are no problems due to smoke, ashes, and their disposal, and the fuel it saves can be used for other purposes. After the initial cost of installation, the upkeep of the solar heater is very low. Solar space heating should be an important economic factor in regions where coal is scarce or where it has to be imported.

In addition to space heating in homes, solar energy offers other possibilities of utilization, such as greenhouse heating, water distillation, and, ultimately, electrical power production. It is the greatest untapped energy resource of the world, and its utilization should be one of our most important and fruitful projects.

JET PROPULSION

C. R. TOSTI and J. B. TUZEN

Major Tosti has been for some time closely associated with aircraft engine problems and developments in the field of jet propulsion. At present he is assistant for control with the Guided Missiles Section of the Air Materiel Command Wright-Patterson Air Force Base. Mr. Tuzen, who left the Air Materiel Command with the rank of major in 1946, has since been in its employ as a civilian mechanical engineer while working for his Doctor's degree at Ohio State University.

THE two words "jet propulsion" invariably bring to most of us a mental image of only the latest type of military aircraft streaking across the sky at high speed. Oddly enough, you and I utilize jet propulsion in our everyday lives, and man had witnessed its operation long before he conceived the idea of applying it to aircraft. In fact, the Greek philosopher Hero dabbled in jet propulsion as far back as 130 B.C.

The humble and unspectacular operation of an ordinary lawn sprinkler; the action of a Fourth of July skyrocket; the recoil of a pistol—all illustrate the fundamental principle of jet propulsion, which is based on the principle of reaction covered in Sir Isaac Newton's third law of motion—*to every action there is an equal and opposite reaction.* For example, when an ordinary pistol is fired, the combustion resulting from the detonation of the gunpowder forms gases which expand explosively and, in trying to escape, exert pressure in all directions within the barrel of the pistol. As the slug leaves the barrel, the gases at the nozzle end immediately escape while the remaining gases are still exerting pressures against the breech end. As a result of this unbalance in force, motion occurs in the direction of the greater force, expressing itself, in the case of a pistol firing, as a "kick," or recoil. It is this reaction, in a direction opposite from that in which the gases are being expelled, that causes the pistol to recoil.

In the case of the jet-propelled engine, the gases formed as a result of combustion burst out of the rearward discharge nozzle, and the reaction drives the airplane forward. The reaction that propels a jet engine occurs within the engine and not, according to popular belief, as a result of the gases "pushing against the outside air." In fact, outside air is not necessary to achieve jet-propelled motion. A pistol fired in a vacuum would recoil with the same force as when air is present. Similarly, a jet-propelled aircraft carrying an adequate source of oxygen necessary for the combustion processes of the engine could operate in a vacuum.

Jet propulsion definitely is not a phenomenon of our present swiftly moving era of flight. In 130 B.C. Hero demonstrated his aeolipile, considered by many as the first apparatus which converted steam into mechanical power and the earliest demonstration of the principle of jet propulsion (Fig. 1). Hero mounted a hollow sphere between two supports over a closed vessel containing water, which was converted to steam by a fire under the vessel. One of the supports was hollow and served to transmit the steam from the closed vessel to the hollow sphere. Two pipes with right-angle ejecting nozzles were affixed to the sphere on opposite sides, and the reaction caused by the escape of the jets of steam from the two pipes caused it to revolve.

Hero's toylike device apparently served no better purpose than to demonstrate the theory of jet propulsion for latter-day historians. The first practical application of jet propulsion occurred through the use of the skyrocket as a signaling and illumination device. The Chinese are credited with using rockets several centuries before the birth of Christ, employing the exhaust gases of burning powder as a means of propulsion. During the defense of Pien-King, during the Mongolian seige in A.D. 1232, rockets were used in warfare, possibly for the first time. It was not long, however, before the military chiefs realized the tactical utility of rockets as weapons, and during the thirteenth, fourteenth, and fifteenth centuries they appeared on the battlefields of Europe. Disguised as floating fishes, running rabbits, and flying pigeons, they were employed to set fire to enemy fortifications.

Although Hero and the early rocket makers utilized the principle of jet propulsion, they were completely unaware of its theory. It was left for Sir Isaac Newton to explain the phenomenon in his famous laws of motion; he then applied these laws to the design of a jet-propelled vehicle (Fig. 2). A spherical boiler mounted over a fire comprised the vehicle's engine, and propulsive thrust was obtained as the steam was allowed to escape

FIG. 1. Hero's aeolipile, considered by many the earliest demonstration of the principle of jet propulsion.

through the rearwardly directed nozzle at the top of the boiler.

In the late 1700s and early 1800s, rockets were more successfully used on the battlefields of Europe and the United States. Perhaps the most far-reaching achievement of the rocket as a war weapon occurred in the Battle of Bladensburg on August 24, 1814, during the War of 1812. Use of the rocket by the British routed Stansbury's American brigade, leaving the city of Washington completely unprotected and resulting in its capture and burning by the British. During the following month, when the British attempted to capture Fort McHenry, in Baltimore Harbor, rockets again were directed at the Americans; in fact, the rockets whose "red glare" Francis Scott Key described in *The Star Spangled Banner* were warheads loaded with heavy explosives.

Near the close of the nineteenth century, thought was given to the development of thermal-jet units. The evolution of these early ideas and experiments resulted in the jet-propulsion engine as we recognize it today. Circiu of Rumania (1886), Octave Chanute of the United States (1886), Marconnet and Lorin of France (1909), Morize of France (1917), H. S. Harris of England (1917), and Gustave Eichelberg of Switzerland (early 1930s) served as pioneers in this field. The first thermal-jet unit of the modern era, illustrated by the schematic diagram (Fig. 3), was the Campini system proposed in 1932. Air was admitted through the annular opening (indicated by the arrows), which widened out to convert the kinetic energy of the air into pressure energy. The air then passed through a two-stage centrifugal compressor driven by a reciprocating engine and was compressed into the combustion chamber, where fuel was introduced and ignition occurred. The expanding gases were then discharged through the nozzle to obtain the propulsive thrust.

Among the first jet airplanes to fly were two machines constructed to utilize this principle. The first, usually referred to as the CC1 (Campini-Caproni), was built in 1940. It featured an all-metal construction and weighed 8,800 pounds. During a trial flight on August 27, 1940, at Milan, the plane remained in the air for ten minutes. Until the details of German aviation developments were learned after the defeat of Germany in World War II, this flight was recorded by historians as the first made by a jet-propelled propellerless plane incorporating a thermal-jet engine. Actually, the first flight was made in Germany exactly one year before by a Heinkel 178 (Fig. 4).

In 1941, a revised version of the CC1, designated the CC2 (Fig. 5), was flown from Milan to Rome, a distance of 168 miles, with a stop at Pisa, probably for refueling. The speed attained was reported to be 130 mph, and the fuel consumption was excessively high. The use of the internal-combustion engine to drive the compressor was inefficient and considerably increased the weight of the airplane.

A logical improvement on the Campini system was the gas turbine type of thermal-jet unit devised by Air Commodore Frank Whittle, of Great Britain. Campini used the energy of the expanding gases for propulsion only, but Whittle also employed this energy to drive the compressor, eliminating the need for a reciprocating engine, which was very inefficient for this application. In

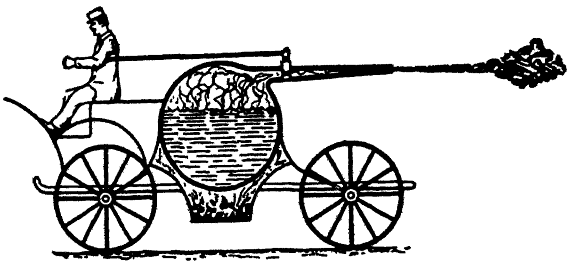


FIG. 2. Jet-propelled vehicle designed by Sir Isaac Newton.

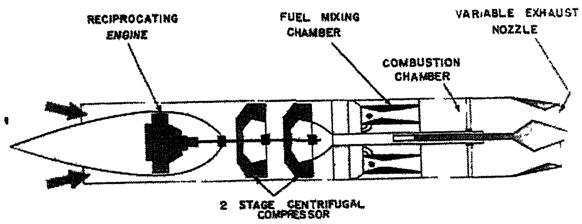


FIG. 3. The Campini system was the first thermal-jet unit of the modern era

the Whittle design, air is taken in as shown by the arrows in Figure 6. The air is compressed by the centrifugal compressor into the combustion chambers, where the fuel is injected and the gas-air mixture is ignited. The expanding gases that result from the combustion process then pass through the turbine wheel and drive it and the compressor, which is mounted on the same shaft, thus providing more air to maintain the continuous combustion process. Only a part of the energy of the exhaust gases is utilized to drive the turbine and the compressor; the rest of the energy is utilized for the propulsive thrust obtained as a result of the expanding exhaust gases being ejected through the discharge nozzle.

In January 1930, Whittle obtained his first patents on a jet-propulsion engine, but it was not until six years later that the construction of the first engine of this type was started at the Rugby Works of the British Thomas-Houston Company. After two years of development work, the Air Ministry offered the Gloster Company a contract for the first jet-propelled airplane using Whittle's engine. During taxiing tests, the airplane became air-borne, reaching an altitude of 6 feet for 100–200 yards' range. The first official take-off of the Gloster Pioneer E-27, forerunner of the Gloster Meteor, occurred at Cranwell on May 15, 1941, almost two years after the flight was made by the German HE 178. The flight lasted seventeen minutes and was successfully completed.

The U. S. Air Force inaugurated its jet-propul-

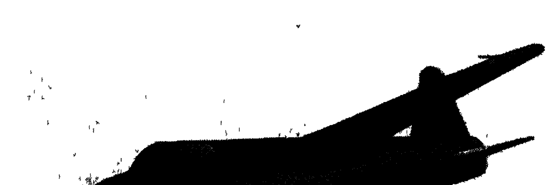


FIG. 4. The Heinkel 178, first jet-propelled propellerless plane incorporating a thermal-jet engine.

FIG. 5. In 1941, a revised version of the CC1, designated the CC2, was flown from Milan to Rome, a distance of 168 miles, with a stop at Pisa, probably for refueling.

sion program soon after the successful flights in England. In July 1941, a special American technical mission viewed the British-developed jet engine at Rugby. As a result of this investigation, arrangements were made to conduct the development of turbo-jet engines, based on the Whittle design, in the United States. At the time this decision was made, only fifteen flights and ten hours of jet engine operation had been accumulated in Great Britain.

The development of the turbo jet in the United States was entrusted to the General Electric Company because of the experience this company had acquired in the development and production of the turbosupercharger for high-altitude, high-power American reciprocating engines. Essentially, the turbo-jet engine is a large turbosupercharger, incorporating a device for the combustion of the fuel-air mixture. The General Electric Company designed, built, and statically tested its first unit in less than seven months after the first Whittle engine was brought to this country. The General Electric design followed the Whittle lines fairly closely. In October 1942, barely one year after the development of the J-31 (I-16) turbo-jet engine was undertaken, the Bell XP-59A Aircomet incorporating two such engines was flown at a secret test site in the United States. An outgrowth of the J-31 development was the J-33 (I-40) turbo-jet engine. This engine produces 150 percent more thrust than the J-31 and is, at present,

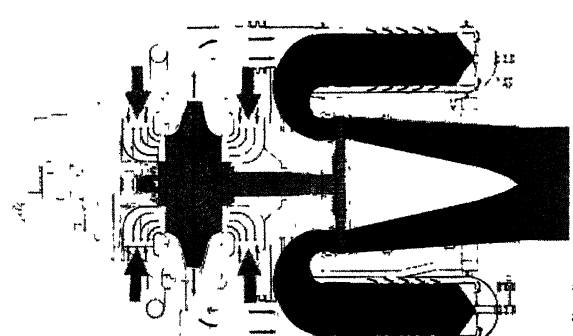


FIG. 6. The Whittle design—schematic.

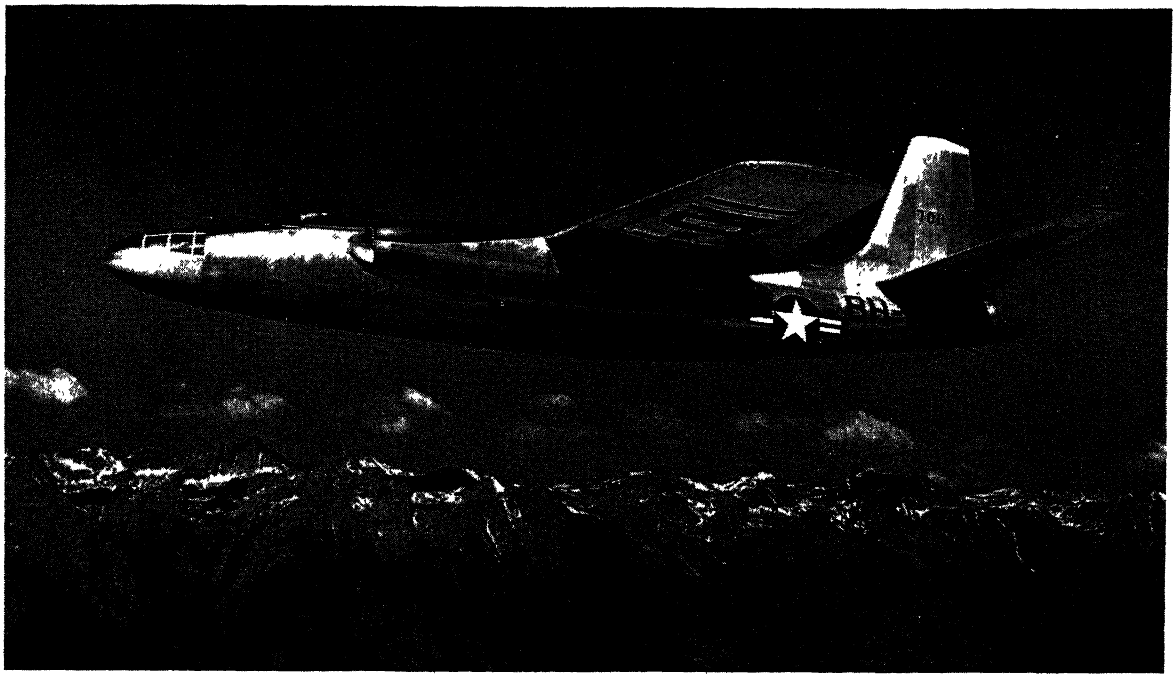


FIG. 7. The North American B-45, one of the first operational bombers to employ jet propulsion.

in operational use in the F-80 Shooting Star.

Along with the production of turbo-jet engines of the Whittle design, the development of axial flow type turbo-jet engines was conducted in Britain and this country. The principle of operation is exactly the same as that of the Whittle engine. The design of the axial flow compressor, however, consists of a series of rows, or stages, of blades rather than one large centrifugal compressor. This permits the utilization of a compressor of smaller diameter to provide the required amount of air compression and, as a consequence, an engine of smaller diameter which offers the advantage of less drag, in providing greater horsepower per unit of projected frontal area. Most of the operational and experimental jet aircraft of today utilize the turbo-jet engine of either the centrifugal compressor or the axial flow compressor type.

Although the details concerning the numerous fighter and bomber jet aircraft under development cannot be divulged in full, some information has been released on several types of jet aircraft. The first among these is the Republic F-84 Thunderjet which has a speed of more than 590 mph, an operational range of 1,000 miles, and a service ceiling of more than 40,000 feet. This aircraft is powered by the J-35 axial flow turbo-jet engine.

One of the outstanding jet fighter planes of today is North American's F-86 Sabre. This aircraft is a low wing fighter with a sweepback of 35 degrees for both the wing and tail assembly. Its service ceiling is over 40,000 feet, and its combat radius is more than 500 miles. The Sabre has established the world's speed record of 670.981 mph, carrying its normal operational load of ammunition and armament. It is powered by the J-47 turbo-jet engine.

Among the newest jet fighters of the Air Force is the Northrop XF-89 Scorpion. This aircraft is especially designed for tactical operations under unfavorable weather conditions. It has a crew of two, a pilot and a radar observer, both of whom can be "exploded" to clear the plane by pilot-ejection seats if the plane must be abandoned at high speeds. The Scorpion is in the 600-mph speed class and has a service ceiling of over 40,000 feet. Its propulsive system consists of two Allison J-35 jet engines. Another new experimental jet aircraft is the Lockheed XF-90, a penetration fighter which made its first flight test at Muroc Air Force Base, California, on June 3, 1949, staying aloft 37 minutes. It employs 35 degrees of wing sweepback and has a span of approximately 40 feet, a length of 56 feet, and a height of approximately 14 feet. It is powered by two Westinghouse J-34 axial flow turbo-jet engines.

The Republic XF-91 is designed as an interceptor fighter to be used as a local defense weapon capable of combating enemy bombers. Its wings

are swept back and are wider at the wing tips than at the wing and fuselage junction. The inverse taper of the wings will theoretically allow better control of the aircraft at low speeds. Powered by a turbo-jet engine, this interceptor also incorporates rocket motors for accelerated take-off and climb and for operations at high altitudes.

The Consolidated-Vultee XF-92A is the first American aircraft to feature the Delta wing, on which the sweepback is 60 degrees. The wing is triangular in shape and incorporates elevons in the trailing edge for aileron and elevator action. The vertical stabilizer and rudder are provided to give additional directional control and stability. The plane is powered by an Allison J-33 turbo-jet engine.

One of the first operational bombers to employ jet propulsion is the North American B-45 (Fig. 7). Its design is patterned along conventional lines, although they are highly streamlined. Its four J-47 turbo jet engines are arranged in pairs in nacelles on each wing. The airplane was designed to carry a bomb load of over 10 tons, and it has a tactical radius of over 800 miles. It is in the 550-mph speed class. The Boeing XB-47 stratojet is a six-engine bomber with wings swept back to a 35-degree angle (Fig. 8). Four of the engines are mounted in pairs beneath the inboard section of each wing. A single jet unit is positioned at each wing tip. The plane is designed for speeds exceeding 500 mph and has a tactical radius of approximately 1,000 miles. The Martin XB-48 is a light bomber powered by six J-35 jet engines. It is in the 500-mph speed class and has a combat radius of over 800 miles. It is designed to carry more than 10 tons of bombs. An unusual feature of the XB-48 is that its main landing gear is of the tandem type, with an outrigger gear under each wing. The Northrop B-49 is the jet-propelled version of the B-35 flying wing. Powered with eight J-35 engines, this aircraft, the largest USAF jet plane so far built, is in the 500-mph speed class (Fig. 9).

Although the turbo-jet engine is the major source of propulsion for the jet aircraft of today, the rocket and the ram jet will find their application on the supersonic aircraft and guided missiles of tomorrow. A detailed discussion of these two

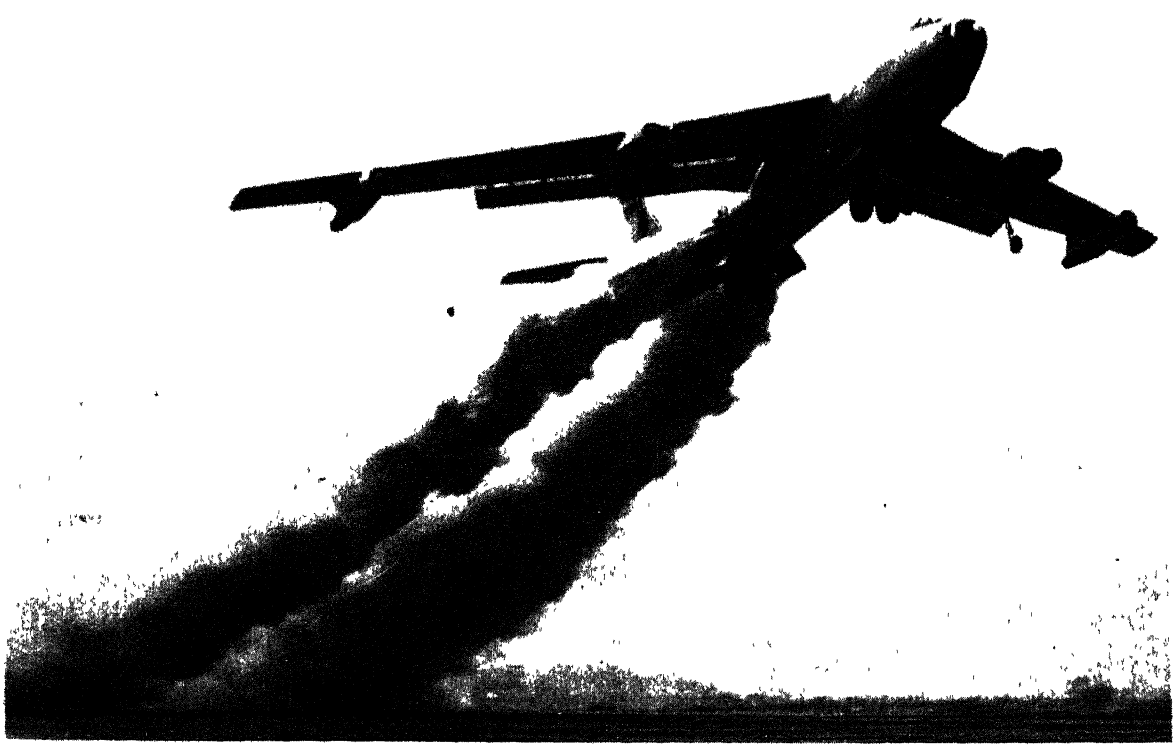


FIG. 8. The Boeing XB-47 stratojet is a six-engine bomber with wings swept back to a 35-degree angle.

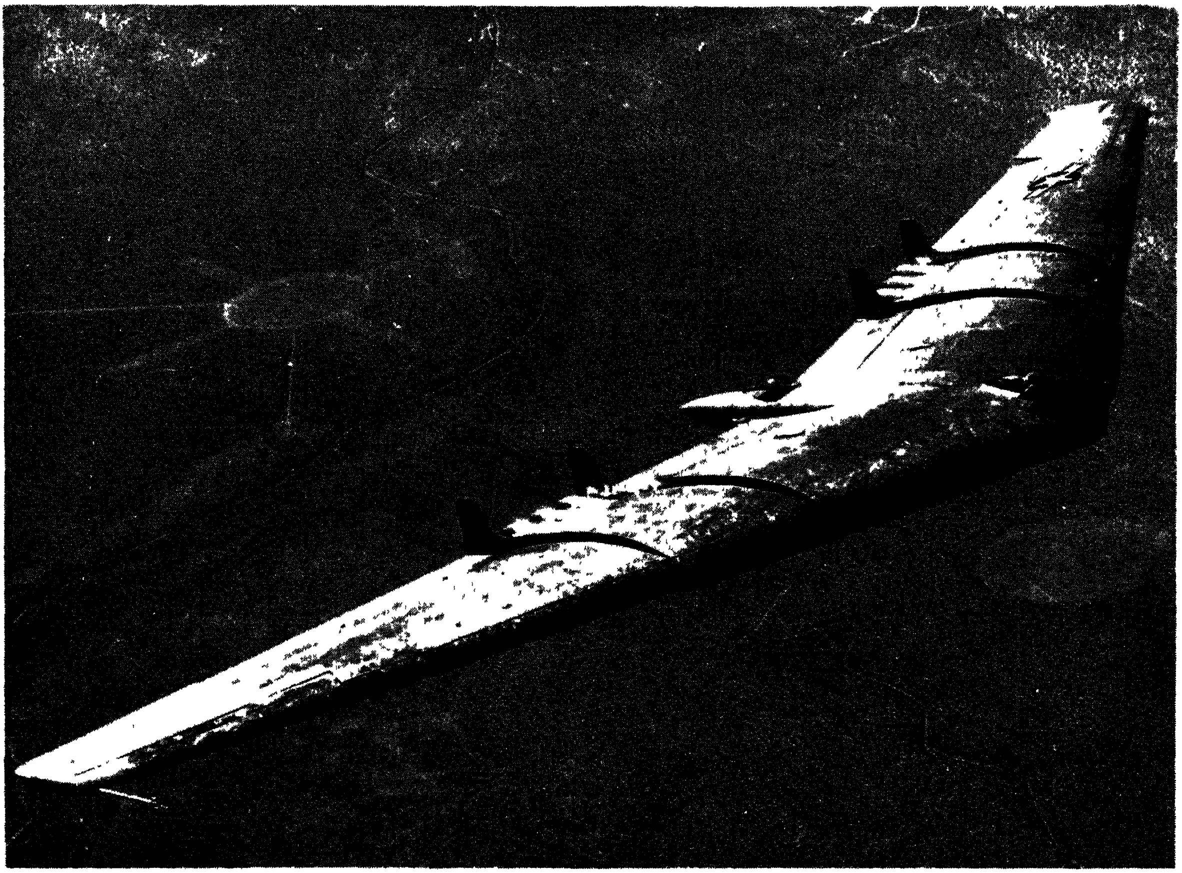


FIG. 9. The Northrop B-49, the jet-propelled version of the B-35 flying wing.

types of jet power plants is not possible within the space limitations of this article It suffices to point out that the performance being obtained with the rocket-propelled X-1 research vehicle is indicative of the strides being made in conquering the problems of supersonic flight. The phenomenal developments of the past several years have established jet propulsion as a primary type of propulsion for the operational Air Force fighters and bombers of the future.

Jet propulsion is a reality today, but we should not expect an overnight replacement of propeller-type aircraft by propellerless, or jet, aircraft. Many problems remain to be solved. For one, the problem of excessive fuel consumption severely limits the range and payload obtainable with present-day aircraft as compared with the range and payload of a propeller aircraft such as the B-50. Furthermore, the take-off characteristics of jet aircraft are unfavorable as compared to those of propeller-type craft, since the thrust horsepower of a jet is a function of its speed. Thus, extremely long runways will be required for the acceleration of heavily loaded jet aircraft to produce the thrust required for take-off. Obviously, propeller-type engines, whether the conventional reciprocating engine or the turbo-prop engine, lend themselves to application on heavy payload aircraft such as cargo aircraft and transports, which require high power outputs for take-off on comparatively short runways.

None of the problems which face us is insurmountable. The problem of high fuel consumption may be solved by the development of more effective fuels and combustion methods. The problem of take-off is being solved with the development of efficient and dependable rocket jet-assist-take-off units. There is no doubt that the new age of flight heralded by the advent and continuous improvement of jet propulsion offers glowing promise to both military and civilian aviation.

NEW PLANTS FOR OLD

P. S. HUDSON

Dr. Hudson, formerly director of the Commonwealth Bureau of Plant Breeding and Genetics, Cambridge, England, is now agricultural officer, Plant Breeding and Genetics, Food and Agriculture Organization of the United Nations, Washington, D C

EFFORTS to introduce plants from one environment to another have been made ever since man started to cultivate plants at all. Some of the plant introductions made in the past have been astonishingly successful; for example, the Brazilian rubber tree introduced into Ceylon, Malaya, and the East Indies has formed the basis of the rubber industry of the world, and anyone who likes oranges will testify to the success of the introduction of the navel orange from Brazil to California Potatoes, tomatoes, and tobacco are other examples of plants introduced from South America which have now become almost universal. In many cases, plants introduced from outside have served as a basis for plant-breeding operations; one or more of the valuable characteristics of the introduced form have been transferred to the commonly cultivated forms of the plant by a process of hybridization, followed by patient selection and testing of the resulting hybrids. In this way many of the improved plants we are familiar with in our farms and gardens, in our kitchens and on our dinner tables, have been built up; wheats resistant to rust attack, strawberries with fruits twice the size of those eaten by our grandfathers; beans free from strings; corncobs with beautifully arranged symmetrical grains, ripening at a range of conveniently graded dates, and, incidentally, yielding nearly half as much again as those our fathers grew—the examples could be continued almost indefinitely. From these few instances it may be seen, however, what an important role introduced plants have played and can play in the improvement of our cultivated plants. The possibilities of improvement would seem to be almost unlimited. Most cultivated plants have been improved in yielding capacity by breeding methods; some, such as wheat, cotton, and many horticultural plants, have been improved also in quality. But almost all forms that now exist could be improved still further by breeding; wheat varieties resistant to rust are not always sufficiently free from lodging or shattering of the ear, those free from shattering are not always sufficiently winter-hardy, and so on ad infinitum. The characters it is desired to add can, more often than not, be found in some other variety or form, though often in a form growing in some quite different part of the world. Plant breeders have gradually come to see that one of the first essentials for the success of their task is to have at their disposal the greatest possible choice of types, from as great a range of geographic and climatic areas as possible, and so containing the greatest range of desirable characters that are known to exist in the plant in question. It was this argument that led the Russian botanists in the years between the two world wars to make a survey of the crop plants of the whole world; the results of the survey, carried out by a strong team of competent botanists under the leadership of the late N. I. Vavilov, surpassed all expectations. Rich and quite unexpected sources of economic plants were discovered, and the range of variation within each plant was found to be much greater than had ever been previously suspected. Not only new plant characters or varieties but new species were discovered. Even in wheat, which was one of the most widely investigated of the cultivated plants, several new species, hitherto unknown, were found. In barley, forms were found that were adapted to extreme conditions either of moisture or of drought, others with smooth awns or naked grains, or resistance to the frit fly, and so on. Among fruit trees, apricots that will survive 72 degrees of frost, blackberries with soft spines, edible honeysuckle and mountain ash, frost-resistant vines, and disease-resistant forms of many others are among the interesting types discovered.

An especially interesting example is the potato. We now know that the potato was introduced into Europe as early as the year 1570 or even before, although it was some time before it became generally cultivated. Today it is grown nearly everywhere and is one of man's staple foods. The number of agricultural varieties recorded can be counted in the hundreds. Yet, as far as we are able to judge from the information at hand, all these innumerable varieties have arisen, ultimately, from

the first few tubers that were introduced into Europe three and a half centuries ago; at least there is no evidence to show that any later introductions from South America were made, or, if they were, that they had any material effect on potato breeding. The entire range of types known in our present-day potatoes, then, has arisen by a continuing process of recombination of the characters contained in the original material brought over in Queen Elizabeth's time. What must have been the astonishment of the Russian botanists, therefore, when they sent their expeditions to the South American Andes, to find, growing in the Cordilleras of Bolivia and Peru, hundreds of other potato types, all as different as could be from the potatoes grown in Europe and elsewhere. They found potatoes of all conceivable shapes and sizes, some long and sinuous like a serpent, some small and round, others with curious markings like the head of an animal or a man; tubers of all colors, ranging from inky black through various shades of purple and pink to pale-cream color, were found. There were many differences of flavor and consistency, too. Most of these forms are cultivated by the local Indians and are given distinctive, often highly descriptive names. That these potatoes have been used by the local inhabitants from time immemorial is shown by the discovery among the ancient Indian pottery of quaint vessels in the shape of potato tubers, eyes and all, and of others, more grotesque in shape, where the head of a man or animal is depicted with eyes resembling those of a potato.

From all this the Russians concluded that they had actually found the birthplace of the cultivated potato. Many of the primitive potatoes found by them show surprising resemblances to those depicted in the first published descriptions of the potato by the early herbalists. Their characteristic features have thus been gradually lost in many generations of breeding. Many others, however, have in fact never been known in Europe or the rest of the world, owing to the unrepresentative and random nature of the samples that served for introduction. The practical importance of this discovery lies in the fact that some of these characters that had previously been unknown are of great value to agriculture. One of these is frost resistance; the potato has never been regarded as a hardy plant, but now it is found that frosts occur almost every night in the higher areas of potato cultivation in the Andes (which in places go up to almost 14,000 feet), and that the potatoes come through undamaged. Such hardiness, if it could be transferred to our common potatoes, would enable immense northern areas in countries like Canada to grow potatoes for the first time. We have already seen what terrible consequences the ravages of such diseases as potato blight can have if left unchecked, and so far no satisfactory control for blight disease has been forthcoming. Some of the primitive species of potatoes found in Mexico, however, are immune from blight, and it only remains to transfer this immunity to the domestic races for a complete solution of the problem to be provided. The same is true for Colorado beetle and many other pests.

Enough has been said to show how important the plants collected in these many expeditions can be as a source of raw material for plant breeders to work with in improving our crops. The vast amount of material collected by the Russian investigators built up the most unique store of potential breeding material that had ever been known, and constituted the famous World Collection of Cultivated Plants at the Institute of Plant Industry, Leningrad. Later developments in genetical thought in the Soviet Union have unfortunately somewhat diverted attention from the importance of this world collection, but it served as an example to breeders throughout the world of what can and should be done to supply the plant breeder with the necessary provisions to enable him to perform his task. Unfortunately, no one country has since been in a position to attack the problem on a scale adequate enough to assemble in one spot all the forms existing of all the cultivated plants with which plant breeders may be expected to work. Nor has it been possible to find one spot in the world climatically suitable to maintain all cultivated plants. Individual efforts have been made by countries or groups of countries. The U. S. Department of Agriculture has maintained and enlarged its already admirable collections of a large range of crops; the British Commonwealth has established large collections of potatoes, cotton, cacao, and bananas; and almost every plant-breeding station throughout the world maintains a collection of some sort to serve as a starting point for its breeding operations. This system has the great defect, however, that no collection is really complete, yet many of the more popular varieties of plants are represented over and over again in different collections; lack of coordination results in both duplication and incompleteness.

How, then, can this state of affairs be im-

proved? Clearly, by cooperative effort on the part of plant breeders throughout the world. There have been repeated attempts in the past to organize some such cooperative activity, but they have mostly come to nothing, owing largely to the magnitude of the task and the absence of any centralized body that would be responsible for the organization of it. This fact was brought to the notice of the Food and Agriculture Organization of the United Nations at the organizing convention in Quebec in 1945, and the subject has been further discussed at a number of later meetings. Out of these many discussions, and after much trial and error, there has gradually emerged what appears to be a workable scheme, which has come to be known as the FAO World Catalogue of Genetic Stocks. The scheme has been devised in such a way that each country, or state, is asked to take upon itself the responsibility for maintaining the varieties, races, and breeding stocks of its own territory, and furnishing details of them to the central office of the FAO (Agriculture Division) in Washington, D. C. In this way the burden of the work is distributed between the participating countries, and every one of them is free to benefit from it. A brief description of the scheme and how it works may be of interest.

Before initiating the actual cataloguing of stocks, exploratory work was done by FAO on the most convenient method of describing the stocks, transcribing the data to permanent records, keeping the records up to date, issuing summarized information, and supplying detailed information on request with respect to particular groups of stocks. These things are more or less interrelated, and after considerable study it was decided that the method must be based on a punched-card system of recording. Description forms have been devised which enable a plant breeder to record all the main characteristics of a variety on a single form by simply placing a circle round a code number in the appropriate column. Each column corresponds to a particular character, and the characters are arranged in three main sections. The first section covers Descriptive Classes, the second, Disease and Insect Reactions, and the third, Agronomic and Quality Classes. In the second and third groups there is uniformity in the setting up of the classes in that, with the exception of the zero, *low* numbers indicate *high* desirability. This principle of classifying stocks so that desirable classes are indicated in low numbers throughout was adopted after thorough consultation with plant breeders. Another feature of the classification system is that it is based on a series of classes from 1 to 9 For example, in the case of yield, 1 = *Very High*, 3 = *High*, 5 = *Medium*, 7 = *Low*, and 9 = *Very Low*. With this method the medium class falls exactly in the middle of the range, which it would not do if the scale ranged from 1 to 10. In this way some 60 characters can be recorded without any writing at all, except the inscribing of the circles. Certain other features are recorded by the very minimum of writing.

These forms are sent to plant breeders and breeding institutions all over the world, and, when they have been completed and returned to FAO, the data recorded on them are transferred to punched cards, these cards forming the basis of the World Catalogue. They are easily manipulable, and it is possible to extract from them, by a simple mechanical operation, all or any desired portion of the information contained on them, and if necessary reproduce it. Thus, on the basis of the information supplied from the breeders and incorporated on the punched cards, a list of the catalogued stocks, classified according to their characteristics, will be compiled and circulated to all contributing breeders and, upon request, to others. From examination of these lists a plant breeder will be able to see whether any of the stocks in the catalogue possess properties of special interest, and, if so, complete available information regarding such stocks will be supplied to him by FAO on application. If the breeder desires to secure seed, he will write direct to the station holding the stock. Quarantine regulations of the countries involved will often suggest the best procedure in the exchange of seed, but FAO will give all possible assistance in case of difficulty.

Owing to the magnitude of the task, it was considered desirable that FAO should restrict the scope of its work in the beginning to wheat, extending it as soon as possible to rice, and thus embracing the two greatest food crops of the world. Then, as opportunity and facilities permit, the catalogue will be enlarged to include other self-fertilized crops, especially the other cereals and crops of importance to certain areas, such as soybeans.

An essential part of the scheme is that each breeder or institution participating in it undertakes to maintain living material of the stocks of which he furnishes the details, and to supply small quantities of seed of these stocks to other participants in the scheme. The type of material perpetuated includes not only commercial varieties but also other varieties or races that would be of possible

interest to plant breeders for use as parents, and even hybrid material known to possess one or more valuable genes or characters, such as resistance to some disease, strong straw, high baking quality, or freedom from shedding, sprouting in the ear, or other defects. Since each country is generally expected to maintain only the races of its own territory, the amount of material that falls to the lot of any one breeder to grow for the benefit of the scheme is not large. For cereals, for instance, about a pint of seed from each stock would usually be sufficient to provide a small package for each plant breeder who might request it, and the seed, if properly stored, will remain viable for at least five years. Seed supplies could be maintained, therefore, by growing one fifth of the stocks each year, or 200 short rows per year if the total number of stocks were 1,000. This would be a very small undertaking if the task were assigned to the staff of an institution engaged in plant breeding.

It is anticipated, however, that certain areas may not participate in the scheme, at the beginning, owing to insufficiency of staff or for other reasons. It may happen that some of or all the varieties of these areas are present in one of the large collections already in existence and maintained by one of the plant-breeding institutions in some other country. For this and other reasons any breeder or institution possessing a world collection of wheat or some other plant, or a large collection from other than his own area, is encouraged to maintain it. Such collections can be exceedingly valuable as a source of genetic material. By "processing" the entire collection — for example, by growing it under epidemic conditions for a particular disease organism—resistant plants may be discovered which, when crossed with susceptible but highly productive varieties, can give rise to disease-resistant strains of the latter.

Although the scheme as here described is applicable only to self-fertilized plants, it is recognized that similar treatment should be given to the cross-fertilizing species, particularly among the herbage and forage grasses and legumes, and the brassicas. These types present a number of difficult problems as far as the securing of uncontaminated seed stocks from living collections is concerned, but it is felt that these difficulties are not by any means insurmountable, and that it is desirable that the question of the cataloguing and maintenance of genetic stocks of these crops should also receive consideration by FAO in due course. Special importance is being attached at the present time to forage plants capable of growing in tropical and subtropical areas. The number of head of livestock that can be carried in these areas is more often than not governed by the availability of fodder during certain critical periods of the year. The introduction of certain exotic species such as the kudzu vine and the lespedezas has gone far toward solving this problem in some parts of the United States and elsewhere, but even a casual study of the question shows that the possibilities of development in this direction are by no means exhausted. In fact, some investigators think we are only at the very beginning. At all events, a systematic survey of the existing resources throughout the world, on the lines indicated above, followed by free and extensive interchange of material and results between the research workers and countries involved, is the next logical step and should undoubtedly lead to a more thorough utilization of the plants already available. Promoting the possibilities of interchange of plants between countries with comparable climatic conditions will probably open up new possibilities in introducing entirely new plant species in many of these areas where they are so badly needed.

SCIENCE ON THE MARCH

TREATMENT OF TREES WITH TOXIC CHEMICALS TO FACILITATE REMOVAL OF BARK AND TO REDUCE WEIGHT*

INEVITABLY, mills that make paper pulp from wood have considerable investment in mechanical apparatus for bark removal and in power required for its operation. Anything that facilitates the removal of bark has, therefore, potential interest to manufacturers of pulp and paper.

Treatment of trees with chemicals to facilitate removal of bark has recently engaged the attention of the pulpwood industry, and various tests of the use of chemicals for this purpose have been undertaken by certain companies since early in 1942, the year in which a patent on the use of chemicals for removal of bark from trees was issued. As early experiments showed that the wood of some trees to which chemicals had been applied was lighter in weight than wood of untreated trees, it seemed that chemical treatment might also be capable of effecting economies in transportation by reducing the weight of wood.

This paper presents a brief summary of work by the Forest Products Laboratories of Canada on chemical aids in bark removal and on the effects of such chemical treatment in reducing the weight of wood, together with observations made at the same time on some of the effects on the quality of the wood of treated trees.

Bark may be peeled cleanly from trees during spring or early summer when growth is active, but, when the growing season approaches its end, the bark is no longer easily peeled. In eastern Canada, where the experiments of the Forest Products Laboratory were done, the period during which the bark may be cleanly and easily peeled from freshly felled trees (known commonly as the "sap-peeling" season) ends about the middle of August. Much of the pulpwood felled during spring and early summer is sap-peeled with simple hand tools. The experiments cited show that trees may be killed with chemicals during this season of easy peeling so that the bark retains the easy peeling quality that characterized it at the time it was killed.

The fact that the bark can be easily peeled from trees for about three months between the time when growth begins in spring and ceases in midsummer, has been explained by reference to the soft condition of the cambium during the growing season. In the season when growth is active, the cells of the cambium are actively dividing and may be easily torn, so that this layer presents a region of easy cleavage that permits clean separation of the bark from the wood with a minimum of mechanical effort. Later, as the period of growth ends, the cambium toughens so as to resemble more nearly the adjacent wood and bark. It no longer presents an area of extreme weakness, with the result that it is no longer the region of easy cleavage, and peeling with the usual hand-operated tools is not feasible. Chemicals may be applied to trees during the season of easy peeling so that the cambium is killed while in its active, easily cleaved condition, with the result that the trees so treated may be easily peeled thereafter.

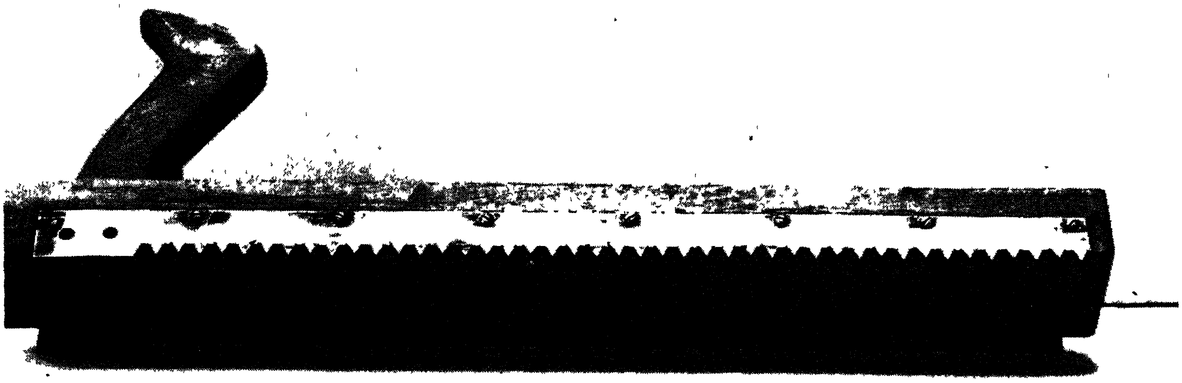
METHODS OF TREATMENT

In order to produce easy peeling by chemical treatment, it was found necessary to apply chemicals to sapwood, exposed all around the circumference of the tree, by removing a strip of bark at a convenient height near the butt. This girdling operation was performed by cutting through the bark with either a special saw or a V-shaped knife. The width of the girdle varied from about 0.25 inch to some 2.5 inches, depending on whether the knife or saw was used.

The chemicals were made into paste with a little water, and sometimes flour, to make the mixtures stick to the wood. In the earlier experiments chemicals were held in close contact with the wood by a band of crinkled kraft paper pulled tight around the tree and fixed in place with a broad-headed upholstery tack, but the most recent tests indicate that a band is not necessary to obtain easy peeling. Such use of chemicals depends on the fact that water-soluble substances can be applied in contact with the wood of living trees so as to be dissolved and absorbed into the ascending sap and taken upward in the sap stream.

Absorption of soluble chemicals so applied to trees takes place fairly quickly when growth is active unless heavy rain immediately after application should wash away materials not protected

* Based on an address presented during the United Nations Scientific Conference on the Conservation and Utilization of Resources, Lake Success, New York, August 17–September 6, 1949.



Double blade (girdling) saw for removing a strip of bark about 2½ inches wide around the tree.

by a bandage. Under favorable conditions, enough material to kill trees and to perpetuate the easy peeling of bark may be absorbed from an application of the most effective chemicals to even the narrowest girdle.

A number of different chemicals were used in experimental treatments, but the best results in effecting easy peeling were obtained with soluble forms of arsenic.† Next best was ammonium sulphamate, widely used in weed-killing compounds. With the most effective chemicals the application of amounts as small as 15 gm (dry weight) was often found to produce effective results on trees of 9-inch diameter at the girdle.

In experimental treatments in which hand methods of girdling and applying chemicals were used, and employing an inexperienced crew, it was estimated that trees comprising 9–12 cords of wood of average pulpwood size were girdled and treated per man-day (eight hours), depending upon the species and general working conditions. Undoubtedly, time studies of large-scale treating operations would indicate that a larger number of trees could be treated per man-day.

Treatments were applied to nine species. In stands where the trees were of pulpwood size, treatments were applied to aspen, *Populus tremuloides* Michx. and *P. grandidentata* Michx.; balsam poplar, *Populus balsamifera* Du Roi; white birch, *Betula papyrifera* Marsh; Balsam fir, *Abies balsamea* (L) Mill.; black spruce, *Picea mariana* (Mill) B.S.P.; white spruce, *Picea glauca* (Moench) Voss; and jack pine, *Pinus banksiana* Lamb. Treatments also were applied to cedar of sizes suitable for posts and poles (*Thuja occidentalis* L.).

† Particularly a mixture of arsenic trioxide with caustic soda. Commercial sodium arsenite also proved effective.

RESULTS

Soluble arsenic applied to trees during June and July rendered the bark easy to peel when the trees were felled after the sap-peeling season was over, but the peeling became easier if trees so treated were allowed to stand until about the middle of October.

In general, trees of the broad-leaved species treated with arsenic in June and July peeled more easily than sap-peeling when felled the following October. Trees of the coniferous species treated with arsenic in June and July, and even as late as the first week of August, generally peeled more easily than sap-peeling when felled in the following October, the bark in some instances coming off in such large pieces as to permit peeling by hands alone without the aid of tools. Chemical treatments that gave easy peeling by October were found to continue their effects the following year.

Trees to which arsenic treatment was applied too late to permit easy peeling during the following autumn improved in ease of peeling if allowed to stand for a year. With the exception of aspen and birch, the bark of trees so treated could be very easily removed in the autumn a year after treatment, and in aspen and birch could be removed about as easily as from untreated trees during the season of easy peeling.

The greatest reduction in average weight‡ of peeled wood was found in chemically treated jack pine and balsam fir; wood of black spruce was not appreciably lighter for its treatment. The small loss of weight of black spruce seems associated with the very narrow sapwood of this species,

‡ Reduction in weight of wood took place above the level where chemicals were applied. Wood below the girdle did not lose weight and was typically heavier than in untreated trees.

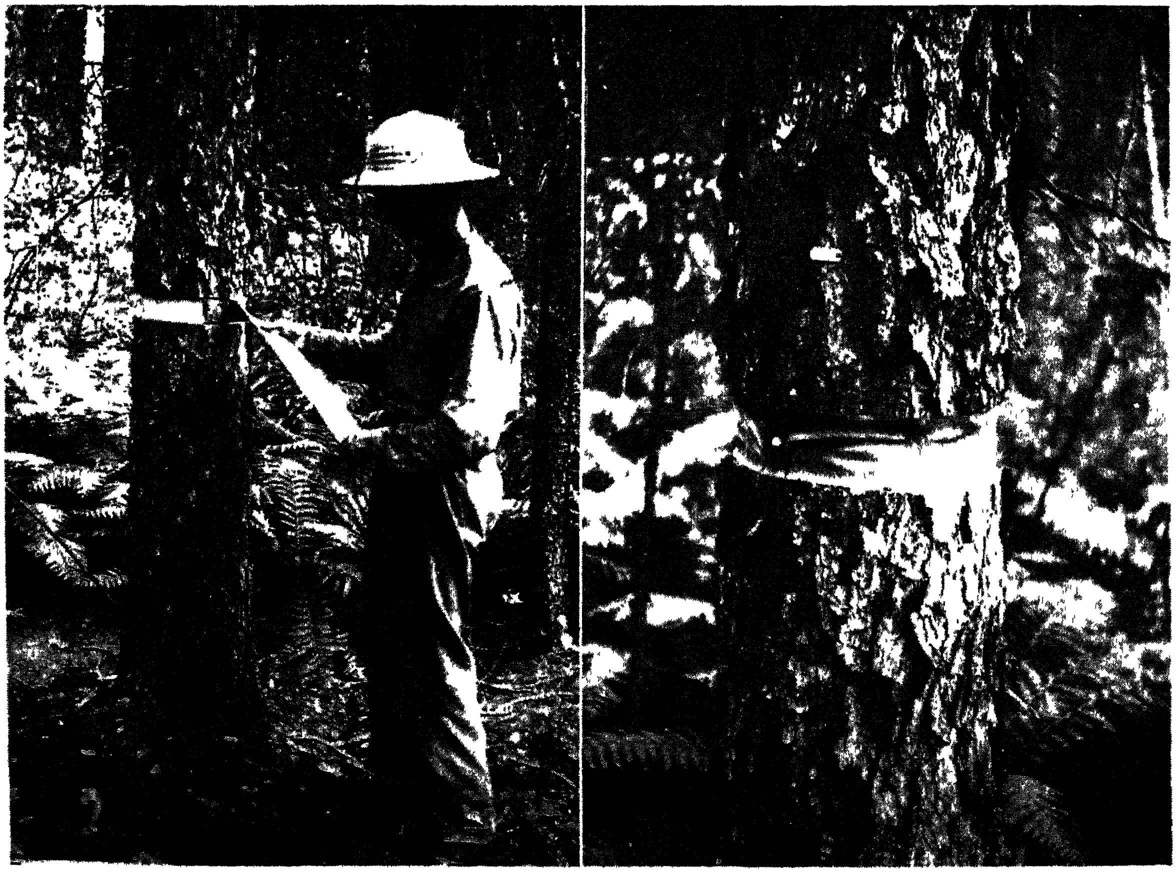
since white spruce, which has wider sapwood, showed satisfactory reduction of weight comparable to that observed in jack pine. The weight of small cedars was reduced in one summer to an amount about 15 percent less than untreated cedar. Trees of the broad-leaved species showed no reduction in weight during the summer and autumn after treatment.

June and July were found to be the best months for applying chemicals to jack pine and balsam fir in order to effect appreciable weight reduction by October of the same year. For example, peeled wood of jack pine treated in June and felled in October of the same year averaged over 10 pounds per cubic foot lighter than normal untreated trees felled during the same period. Based on an average of 88 cubic feet of solid wood per cord of jack pine, this would mean a reduction of about 900 pounds per cord effected four months after treatment. Late treatments (made during early August) were not as effective in reducing the weight of wood and trees treated in September and, sampled one month later, showed no reduction in weight. Allowed to stand for a year after treatment, however, such trees showed considerable loss of weight.

Examination of preliminary results of experiments carried out during 1947 and 1948 has shown that trees of jack pine and balsam fir treated with chemicals at any time from June to September continue to lose weight throughout the year following treatment. Trees of jack pine treated in June and July showed a loss in average weight of wood of about 17 pounds per cubic foot when felled in September of the year following treatment, and those treated in August and felled during the same period showed a reduction of about 12 pounds per cubic foot.

Trees of black spruce and broad-leaved species treated from June to September and felled during the early summer of the following year showed no appreciable reduction in weight.

Traces of bark insects were sometimes observed, particularly in the conifers (with the exception of cedar), in trees treated during June or July and felled during the first summer. Trees in which chemical treatments were most effective in reducing weight generally had the most bark insects,



A gasoline-powered girdling tool in operation.

especially in top logs where the greatest reduction in weight typically occurred. Trees treated with ammonium sulphamate were much more severely attacked than those treated with arsenic, which evidently had some toxic effect on insects. The attack by bark insects in such trees treated with sulphamate was sufficient to cause the bark to break along the insect tunnels and peel in small pieces so that removal of the bark with the usual peeling tools was more difficult than in trees treated with arsenic.

Some trees of the coniferous species, especially jack pine, treated in June or July, showed considerable attack by wood borers in top logs when felled in October of the same year. Trees treated late in the year (in September), although showing no attack by wood borers when tested a month following treatment, showed traces of attack when felled the following June, the attack becoming more severe during the warm summer, as shown by observations made when such trees were felled in September (a year after treatment). Trees of the broad-leaved species felled a year following treatment also showed some attack by wood borers.

Traces of blue stain were found in sapwood of coniferous species, treated in June or July and felled in October of the same year, the attack being more severe in the top logs of trees of jack pine where considerable drying had taken place. Sapwood of some trees permitted to stand through two summers after treatment showed signs of decay.

Trees treated with chemicals late in the sap-peeling season tended to show less attack by insects and stain when felled in the late autumn than those treated early, the amount of such attack depending upon the length of time the trees were left standing during the warm summer months. In no case was the presence of wood borers and blue stain considered so severe as to degrade the wood significantly for pulp.

The small amount of the chemicals that enter the sap stream of treated trees would seem insufficient to prevent stain or attack of insects on the sapwood. The fact that treatment with arsenic keeps trees so treated comparatively free from bark insects during the first summer after treatment is of interest since such insects were found in considerable number in trees treated with chemicals lacking the toxicity of arsenic. The effectiveness of soluble arsenic in the tests reported makes it the logical standard of comparison for other chemicals that may be tried in future for similar purposes. Arsenic has, however, the disadvantage of causing its handlers to develop painful rashes unless the most extreme care is taken to avoid its contact with the skin. Moreover, its extreme toxicity makes it a possible source of danger to animal life unless substances repellent to animals are mixed with it.

It does not seem too much to expect that other suitable substances may be found that are capable of producing as easy peeling as arsenic, that will be more effective as inhibitors of insects and staining fungi, and that can be more easily and safely applied.

Recently a gasoline-powered portable tool for girdling trees has been developed that is reported to be satisfactory. The fact that such a tool is capable of making a deep cut in girdling trees may increase the drying of wood of species that so far have not been readily susceptible to reduction in weight by application of chemicals to superficially girdled trees. This, coupled with the fact that the most recent tests show that easy peeling can be produced by applying chemicals without a covering band of paper, should accelerate treating operations. If chemical treatment is to become widely established in industry, possibilities of increased efficiency will of course be sought.

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GAS TURBINES FOR VEHICLES

IF ONLY a small part of the time and money expended by the automobile industry on the development of the reciprocating engine were to be applied to gas turbine development, a highly efficient, reliable engine, with improved performance at reduced cost, would soon be available to the American public. Development of high-temperature alloys and high-efficiency air compressors places the gas turbine on a competitive basis with the reciprocating engine for such vehicle applications.

Steady progress has been made in the development of gas turbines, and the question arises whether such turbines can be successfully applied to vehicle propulsion on a competitive basis with present reciprocating engines. The gas turbine offers several distinct advantages. Comparisons of present and future performance are illustrated in Table 1.

Although most gas turbine research and construction have been on units of 1,000 HP and larger, there is some interest in smaller units, especially in Czechoslovakia and England, where automotive gas turbines have been built. In the United States a 200-HP unit weighing 150 pounds has been developed. It has a diameter of 10 inches and a maximum combustion temperature of 1,500° F.

Application of the gas turbine to automobiles can effect a saving in weight of the engine of 50 percent. Since a reduction of one pound in engine weight will enable designers to reduce the weight of the chassis by another pound, a material saving in weight and cost of motor vehicles can thus be realized.

It is difficult to visualize a more complicated propulsion device than a reciprocating engine, in which energy from fuel is converted into mechanical work through the effort of pistons, valves, crankshaft, and flywheel, then used to drive either a complicated automatic mechanical transmission or a fluid generator which in turn drives a fluid pump and finally a speed reducer at the rear wheels. The only excuse for such a complicated mechanism is that it works with acceptable performance.

In comparison, the gas turbine for automobiles offers a simple device consisting of a prime mover and torque converter in a single unit, requiring only two rotating elements and a single speed-reducer. It is one of the few prime movers that are capable of producing high starting torque at low speed.

Figure 1 shows the performance of a nonregenerative gas turbine designed for the following conditions:

95 HP at 83 mph (same as reciprocating engine)
Temperature at design point—1,500° F.
Compressor efficiency—80 percent
Compressor-turbine efficiency—85 percent
Power-turbine efficiency—85 percent
Combustion efficiency—95 percent
Pressure losses—2.5 percent
Pressure ratio of compressor—3 4 1
Inlet air temperature—60° F.

On a level road this turbine would propel the vehicle at 83 mph with a gas temperature of 1,500° F. At 68 mph the gas temperature would be 1,200° F. and at 58 mph, 1,000° F. The torque multiplication—that is, the torque at zero speed divided by the torque at maximum speed—is 2.75. Although

TABLE 1

	Present Reciprocating Engines		Present Gas Turbines	Future Gas Turbines
	Manual Gear Shift	Automatic Transmissions	Without Regenerator	With Regenerator
Mi. per gal.	15–18	8–14	9–11	16–18
Wt. per 100 HP	500	600	150	200
Cost per 100 HP, $ (high prod.) ..	250	350	150	200
Advantages . ..	Economical, quiet, simple control	Smooth operation, quiet, low driving fatigue	Small size, light weight, low cost, smooth operation, few parts, low-grade fuel, low oil consumption, excellent torque characteristics	Economical, moderate cost, smooth operation, few parts, low-grade fuel, low oil consumption, excellent torque characteristics
Disadvantages ...	Gear shifting, plumbing difficulties; highly refined fuel, oil contamination	Poor fuel consumption, expensive, plumbing difficulties, complicated mechanism, highly refined fuel, oil contamination	High-speed reducer, noise, poor fuel consumption	High-speed reducer, noise

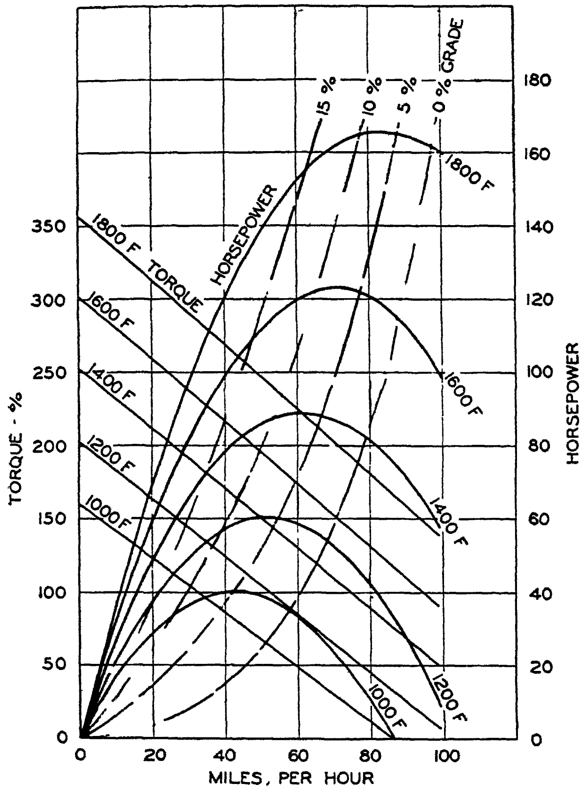

FIGURE 1

this is less than the over-all torque multiplication of the gear transmission of an automoble in low gear, it is more than adequate to negotiate a 15 percent grade.

The power turbine, which is connected directly to the axle through a gear reduction, will operate at speeds from zero to maximum. The compressor-turbine wheel, however, operates at nearly constant speed. Thus the compressor and its turbine wheel act as a producer of high-pressure, high-temperature gas. Control is effected by reducing the speed of the compressor turbine, thereby reducing the pressure ratio, temperature, and air flow through the unit. The speed reduction of the gas producer need not be large to accomplish adequate control. A change in speed of the compressor of less than 20 percent will permit operation of the power turbine over its full range of speed.

A study of Figure 1 will indicate the suitability of the gas turbine to vehicle propulsion. Turbines have characteristically parabolic horsepower curves; efficiency curves are approximately the same shape. With control by gas-produced speed variation, the grade curves fall near the loci of maximum efficiency and horsepower. If the design point is selected at the peak of a given horsepower curve, say, one corresponding to 1,500° F., the torque multiplication is 2: 1. By designing the turbine for a blade-speed/jet-velocity ratio somewhat above the ratio of maximum efficiency, the grade curves will fall reasonably close to the peaks of the horsepower curves over a range of grade curves, and the torque multiplication becomes greater than 2: 1. Shifting the design point beyond the peak of the horsepower curve causes a small increase in fuel consumption on a level grade, but improves fuel consumption considerably on steep grades. Also, a slightly larger turbine is required.

A vehicle requires maximum power and maximum torque for less than 1 percent of its operating life. To obtain high torque output for short intervals, the gas temperature may be raised above the design temperature. If the gas temperature is raised to 1,800° F., the torque multiplication becomes 3.55: 1. This procedure is analogous to supercharging aircraft reciprocating engines during take-off wherein a time limit is set during which the intake manifold pressure may be raised above normal.

With regeneration, the fuel consumption is materially improved. The mpg reach values comparable with many automobiles and light trucks now in operation. The principal advantages of the gas turbine drive are its excellent torque characteristics, eliminating automatic transmissions, freedom from plumbing difficulties, no need for antifreeze, low consumption of lubricating oil, light weight, small number of moving parts, and ability to use fuels that do not require a high degree of refining—for example, low octane number, etc.

There are many problems to be solved before the gas turbine becomes a practical machine that is competitive with the reciprocating engine for vehicle propulsion. Among these are component efficiencies when parts are made small, high-speed gearing, noise, effect of dirt deposits, and low-cost production techniques. However, progress to date on gas turbines indicates that these problems can receive adequate solutions.

Other problems involved in the successful application of gas turbines to vehicle propulsion are noise and vibration, cost, control, starting exhaust-gas dissipation, air consumption, stresses, fuel system, and weight. The noise problem is particularly troublesome, because much of the noise is air-borne and originates in the compressor, but because it is high in pitch and therefore easy to absorb, sufficient research should find a solution to this problem. Since there are only two high-speed rotating parts in perfect balance, vibration does not become objectionable, nor is it harmful.

Cost of an automotive turbine on a production basis should be low. The high cost of present turbines is due primarily to development expenses.

Since the cost of the engine is a small part of the total, it has only a small effect on the over-all cost of the vehicle.

Control of gas turbines over a range of speed and load is not easy. Adequate controls are becoming more complicated and costly. Fuel nozzle research to date has brought on complications rather than simplification. But a gas turbine for a vehicle needs a single control for the fuel, combined with a temperature limiter and an overspeed control. Small gas turbines already in operation may be brought from a starting state to full load in fifteen seconds, and after starting the compressor turbine the power turbine may be accelerated from stall to full speed in five seconds. An electric starter is required to accelerate the compressor turbine to about 25 percent of rated speed. Ignition is required for only a few seconds at starting. Starting can be made easier by opening a by-pass between the two turbine wheels so the compressor turbine can use the full pressure ratio during starting.

The gas turbine uses about seven times more air through the machine than does an equivalent reciprocating engine; therefore, there is seven times as much exhaust gas.The exhaust gas from the gas turbine is cooler, however, than the exhaust gas from a reciprocating engine, and this is especially true if a regenerator is used. Including the air requirements for engine cooling of reciprocating engines, the air requirements for the two types of engines are about equal. Hot gas flowing from the rear of a gas-turbine-propelled vehicle is no different than that from a conventional automobile.

The turbine blades are subject to high stresses, and a mechanical failure of a blade presents disastrous results. Such troubles can be avoided by adequate design and sufficient preliminary testing, with perhaps new parts installed at predetermined intervals. Alloys having 0.1 percent creep in 3,000 hours under a stress of 30,000 psi at 1,350° F. are available at present. If blade-vibration fatigue is eliminated, these alloys are adequate for the life of a vehicle.

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"ORLON" ACRYLIC FIBER

ACRYLONITRILE polymer was first recorded in the chemical literature many years ago and was described as being intractable, infusible, and insoluble, thus leading to the belief that it was three-dimensional in structure. The origin of the "ORLON"* acrylic fiber prepared from this polymer occurred in the discovery in the laboratories of the Du Pont Company that polyacrylonitrile was soluble in certain organic solvents, producing concentrated solutions amenable to conventional yarn-spinning techniques. The initial work on this new fiber disclosed that is was orientable, leading to strong structures, dimensionally stable, resistant to common solvents, acids, dilute bases, and ultraviolet light, and had commercially valuable aesthetic properties. This singular combination of properties justified an intensive search for a suitable commercial process of manufacture. The first phase of this work included a broad search for a solvent from which the polymer could be spun. This effort culminated in the issuance of several Du Pont patents,[1] disclosing a variety of materials possessing solvent and plasticizer character for polyacrylonitrile and its copolymers containing up to 15 percent modification. Investigators elsewhere were also successful in finding solvents for polyacrylonitrile, as revealed in their issued patents.[2]

The polyacrylonitrile used for spinning into Orlon yarns for industrial outlets can be prepared by polymerizing acrylonitrile with peroxygen-type catalysts. Since the polymerization reaction is sensitive to minor amounts of impurities, it is essential to control the conditions of polymerization rigorously, to produce material of uniform properties suitable for uninterrupted spinning. After polymerization and purification, the polymer is blended to level off the minor variations in properties that may occur.

Early in the Orlon acrylic fiber development, an economic analysis of the conventional methods of yarn spinning, together with consideration of product properties, favored the formation of filaments according to the principles of dry spinning. This consists in the precise metering of the concentrated polymer solution through a spinneret into a hot evaporative medium, to remove the major portion of the solvent, thereby forming the solid yarn.

The residual solvent remaining after the spinning operation is removed by extraction with water. Following extraction, the yarn is drawn several times its original length. This process of attenuation aligns or orients the polymer molecules along the filament axis, thus allowing inter-

* "ORLON" is the trade-mark used by Du Pont to designate its acrylic yarn and staple.

molecular forces to become established, increasing yarn strength. Dimensional stability is imparted to the yarn by allowing it to relax a controlled amount after the drawing operation. The strength, elongation, and other physical properties of the yarn can be altered within wide limits by proper adjustment of the extent of drawing and relaxation. After winding on a suitable package, the yarn is ready for shipment to customers' mills for textile processing.

Physical properties. Orlon acrylic fiber has a warm, dry, luxurious hand, coupled with a subdued luster and good dimensional stability. These attributes give this fiber the feel and appearance of silk. It has a high bulking power, which is partly due to the cross-sectional characteristics of the filament. As shown in Figure 1, these are "dog-bone" or "clover-leaf" in shape.

Typical physical properties for the present commercial fiber are shown in Table 1. These properties may be varied over a wide range by changes in the processing history. Similar data are listed for nylon and cellulose acetate fibers for comparative purposes.

Electrical properties. The electrical properties of this fiber are not particularly notable at less than microwave frequencies. In the frequency range 10^2–10^6 cycles per second, the dielectric constant is 5.0–4.0, and the dissipation factor is 0.09–0.03. In the microwave frequency range, the combination of low loss tangent, a low dielectric constant, and low moisture absorption is notable. The extreme resistance of this fiber to fungi and soil-organism attack is also of interest in considering electrical applications.

TABLE 1

	ORLON	NYLON	ACETATE RAYON
Specific gravity	1.17	1.14	1.33
Tenacity,* gpd (Suter)			
Dry	4.0 – 5.0	4.5 – 7 5	1.1 – 1 5
Wet	3.6 – 4.5	4.0 – 6 5	0.7 – 0.9
Loop	3 3 – 3.7	3.9 – 6.0	0.8 – 1.2
Knot	2 7 – 3.3	3 8 – 6.0	0 8 – 1.2
Elongation, % (Suter)			
Dry	16 – 21	14 – 28	20 – 27
Wet	16 – 22	17 – 30	28 – 35
Loop	11 – 17	10 – 13	13 – 20
Knot	8 – 13	9 – 12	13 – 20
Tensile strength (psi)	59,000 – 74,000	65,000 – 110,000	19,000 – 25,000
Instantaneous elastic recovery,** %:			
From 2% stretch	97	100	87
From 4% stretch	84	100	83
From 8% stretch	75	100	55
Retarded elastic recovery,*** %:			
From 2% stretch	85	100	72
From 4% stretch	66	100	44
From 8% stretch	57	99	23
Stretch resist., gpd (at 1% el.)	0.70	0 31	0 45
Moisture regain, % (at 60% R H.)	0 9 – 2.0	3.4	5.4
Shrink., % (30 min., boiling water)	2 – 4	8.0	2 0
Flex life, cycles (Masland)	316,000	380,000	200

* The tensile strength at the breaking elongation is expressed in grams per denier where the denier is the weight in grams of a 9,000-meter length of the test sample.

** Sample stretched and then allowed to recover for 60 seconds.

*** Sample stretched, held for 100 seconds, and then allowed to recover for 60 seconds.

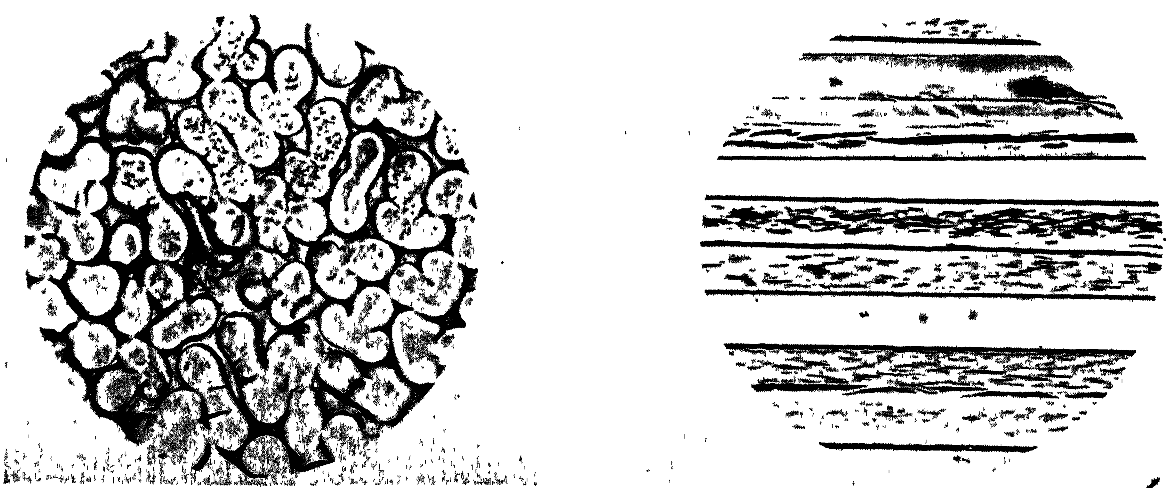

FIG. 1. Photomicrographs (× 500) of Orlon acrylic fiber. The cross-sectional view on the left shows "dog-bone" and "clover-leaf" filament shapes. Longitudinal view on the right shows characteristic rough striated surface.

Thermal properties. The resistance of Orlon to burning is between that of cellulose acetate and viscose rayon. It does not flash burn, and the combustion products are no more toxic than those from silk or wool. The softening point† and zero-strength temperature‡ are high, and the high-temperature durability is good. The sticking temperature, as measured on a copper block under 200-gram load and 15-second exposure, is 235° C, as compared with 190° C for cellulose acetate and 220° C for nylon. The temperature at which the fiber bursts into flame was determined to be less than 500° C for cellulose nitrate, 750°–800° C for viscose rayon, 800°–850° C for cellulose acetate, and 950°–1,000° C for Orlon. These may not be taken as true ignition temperatures because of the possibility of an absolute error due to the rapid heating rate employed, but their relative order is correct.

The fraction of room temperature tenacity retained at elevated temperatures is given in Table 2. These data were obtained by immersing the sample in silicone oil at the indicated temperatures and measuring the tenacity in the usual manner. They do not represent thermal degradation, but rather the reversible thermal weakening of the interchain forces which are responsible for fiber tenacity.

TABLE 2

PERCENT OF 25° C TENACITY RETAINED AT ELEVATED TEMPERATURE

Temperature (°C)	Tenacity Retained (%)
− 40	135
+ 25	100
+ 75	85
+100	70
+125	60
+150	40
+175	15
+200	10

Prolonged exposure to elevated temperatures darkens the fiber, but causes little loss in tenacity until a temperature of 150° C is exceeded, as seen in Table 3.

Durability properties. In addition to good high-temperature durability, Orlon acrylic fiber has excellent resistance to outdoor exposure, to acidic and neutral chemicals, and to insect and micro-organism attack. It is not particularly resistant to alkaline chemicals.

Representative chemical durability data are given in Table 4.

† The temperature at which viscous flow changes to plastic flow.

‡ The temperature at which the tenacity falls to some prescribed level, generally 0.1 gram per denier.

TABLE 3

HIGH TEMPERATURE DURABILITY—HOT AIR EXPOSURE*

Temp. (°C)	Exposure Time (Days)	Straight Tenacity/Elong. (gpd/%)	Loop Tenacity/Elong. (gpd/%)
Unexposed	—	4.7/15	2.95/10
100	1	5.1/17	3.15/11
	4	5.1/17	3.45/11
	11	4.8/16	3.25/11
	32	5.2/17	3.00/10
125	1	5.0/16	3.10/10
	4	5.1/17	3.20/11
	16	5.0/17	3.05/10
	32	5.1/17	2.75/10
150	1	5.0/17	3.15/11
	4	3.9/15	2.75/10
	8	2.8/11	2.40/ 9
	16	1.3/ 3	2.40/ 9
175	1	2.3/ 9	2.00/ 7

* Exposure in oxygen-free atmosphere will show even less degradation than shown in above table.

Common commercial fibers would not withstand the action of these chemicals at the concentrations employed for more than a few days. Exposure for two months at room temperature to toluene, naphtha, gasoline, ethyl acetate, ether, "Triclene," acetone, and ethanol had no effect on Orlon.

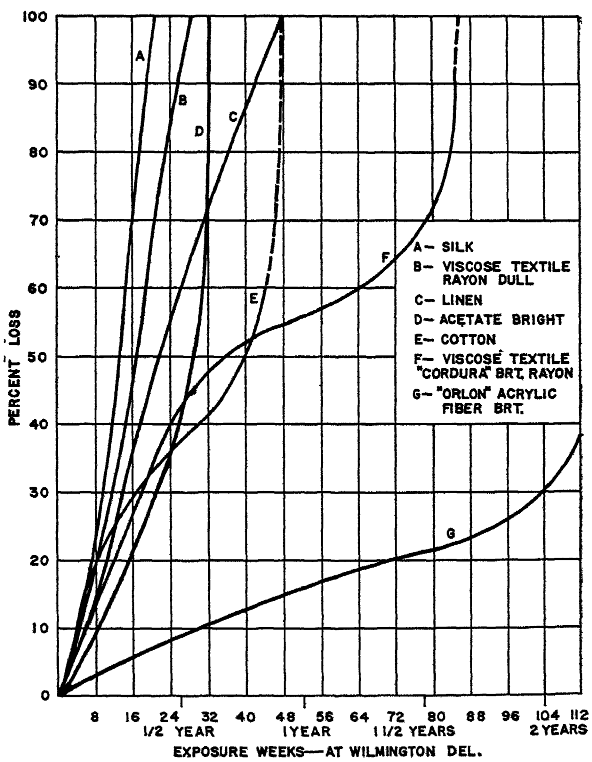

FIG. 2. Effect of outdoor exposure on the tenacity of various natural and synthetic fibers.

TABLE 4
CHEMICAL DURABILITY

Reagents	Conc. (%)	Temp. (°C)	Period of Stability* ORLON
Aqua regia	100	25	No change after 105 days
	50	75	" " " 16 "
Hydrochloric acid	37	25	" " " 20 "
	18	75	" " " 16 "
Hydrofluoric acid	48	25	" " " 56 "
Nitric acid	40	25	" " " 105 "
Sulfuric acid	50	25	" " " 30 "
	37	25	" " " 80 "
Sodium hydroxide	10	25	" " " 10 "
	5	25	" " " 30 "
Zinc chloride	50	25	" " " 105 "
	30	80	" " " 35 "

* Stability defined as less than 15 percent tenacity loss.

Good resistance of Orlon acrylic fiber to commercial bleaching treatments is shown by the data presented in Table 5. Each treatment was several times more severe than normally applied during commercial laundering.

TABLE 5
EFFECT ON TENACITY OF REPEATED BLEACHING

No. of Bleaches	% Loss in Dry Strength	% Loss in Wet Strength
30	2- 5	6-22
60	4- 6	9-26
100	7-14	11-22

This fiber has outstanding resistance to the degradative effects of ultraviolet and sunlight exposures. Fade-Ometer (ultraviolet light) exposures of 1,000–1,500 hours are required to produce a 10 percent tenacity loss in Orlon, whereas cellulose acetate (bright) shows a similar loss in 300–500 hours, and viscose (bright) in 100–300 hours. Outdoor exposure data secured at Wilmington, Delaware, are shown in Figure 2. The resistance of Orlon is unique.

Data on the resistance of Orlon to microflora, teredos, and fabric insect pests are shown in Table 6. The resistance of Orlon to these agents is seen to be outstanding.

Dyeability. Orlon acrylic fiber is hydrophobic, and consequently its response to conventional dyeing procedures is generally different from the rayons and natural fibers. Pastel shades of good washfastness can be secured with certain acetate and basic colors using orthodox dyeing procedures at the boil. Medium shades can be obtained with these same dyestuffs by using an assistant such as meta cresol in the dye bath. Heavier shades can be secured by dyeing under 15–20 pounds pressure (gauge). Acetate, basic, and certain vat colors may be printed using the usual methods followed by steam aging under 15–20 pounds pressure (gauge). Modified fibers dyeable to deep shades with a wide range of colors by orthodox techniques have been produced in experimental quantities.

Manipulation of properties. As mentioned earlier, Orlon is a completely synthetic fiber. Modification of the several processing steps between the initial volatile monomer and the final yarn provides a wide spectrum of yarns of differing physical properties. A few of the process changes which can be made to give modified yarns may be only briefly discussed here.

The polymerization may be controlled to give a polymer of almost any desired molecular weight. The molecular weight at present used has been selected to yield a desirable combination of final yarn properties and process operability.

Valuable tensile and aesthetic properties are imparted to Orlon by the drawing operation which orients the polymer chains. Unlike nylon and certain other synthetics, Orlon does not have a definite draw ratio§ beyond which breakage of the fiber always occurs. With Orlon high draw ratios are possible because of the large amount of plastic flow which occurs during drawing, in addition to the orientation process. The ten-

TABLE 6
MICROFLORA, TEREDO, AND FABRIC-INSECT-PEST RESISTANCE

Test Condition	Observations
Soil burial	
(tropical microflora, 21 days)	No tenacity loss
Soil burial	
(ropes for 18 weeks)	8% tenacity loss
Sea water immersion	
(ropes for 6 months, heavy teredo infestation)	11% tenacity loss, no teredo damage
Pest defilement:	
American cockroach (7 days)	No tenacity loss
German cockroach (69 days)	No tenacity loss
Pest subsistence:	
Firebrat (20 days)	No damage
Carpet beetle (20 days)	No damage
Clothes moth (20 days)	No damage
German roach (30 days)	No damage
Pest starvation:	
Firebrat (40 days)	No damage
Carpet beetle (50 days)	Slight damage
Clothes moth (33 days)	No damage
German roach (32 days)	No damage

§ A draw ratio of 5 produces an extension during drawing of 500 percent.

sion conditions within the fiber structure during the drawing operation are also important in determining the final yarn properties. X-ray studies indicate that the drawing tension affects the extent or degree of order within the structure, but not the amount.

After drawing, the fiber may be relaxed in order to gain additional toughness and thermal stability. Relaxation causes an increase in orientation as well as in the amount of ordered material. In addition, order is extended to other planes within the fiber. It is not surprising, therefore, that many fiber properties are affected by the extent of relaxation, the relaxing tension, the medium employed, and the temperature at which it is carried out.

Suggested markets and future development. An examination of the above properties of Orlon acrylic fiber immediately suggests certain applications. Its strength and great resistance to heat, acids, microorganisms, and outdoor exposure suggest uses in acid-resistant clothing, hydraulic and pneumatic filtration of acidic materials, automobile tops, awnings, curtains, yacht sails, and sewing thread for fertilizer bags, awnings, tents, tarpaulins, and other similar items.

The fiber's high covering power, good dimensional stability, pleasing hand, drape, and rapid drying suggest uses in suitings, rainwear, lingerie, shirtings, uniforms, and beach wear.

At the present time, continuous-filament yarn is produced on a pilot-plant scale for market development. The evaluation of experimental quantities of staple fiber has shown that such a material will also find a field of usefulness. The process for producing staple fibers differs from that of the continuous-filament yarn not only in the fact that the material is cut into short fibers of the required length, but also in the subsequent processing operations. The object in making a staple fiber is to achieve good processability from fiber to yarn by the conventional systems, and at the same time impart to the end product, be it woven cloth or a knitted structure, warmth, loftiness, pleasing hand and drape, good resilience, and other desirable properties. To accomplish this, it is necessary to impart a crimp to the fiber, either by a chemical or mechanical means.

By appropriate changes of the spinning and other processing conditions, staple fibers possessing properties ranging from silklike to wool-like can be produced. Undoubtedly, this development will assume a greater importance as the work progresses.

REFERENCES

1. U S. Patent 2,404,714, G H Latham, Du Pont.
   U S. 2,404,713, R. C. Houtz, Du Pont.
   U S 2,404,717 " " "
   U.S 2,404,718 " " "
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   U S. 2,356,767, Kropa. American Cyanamid
3. Smith, H DeW. Textile Fibers · An Engineering Approach to Their Properties and Utilization. *Am. Soc Testing Materials, Proc.*, 1944, 44.

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BOOK REVIEWS

OF HUMAN BONDAGE

The Situation in Biological Science. Anonymous. 636 pp. $5.00. International Publishers. New York.

THIS book, said to be the complete stenographic report of "The Proceedings of the Lenin Academy of Agricultural Sciences of the U.S.S.R., July 31–August 7, 1948," was printed and copyrighted in the United States in 1949. Although nowhere so stated in the volume, it is a photolith duplicate of an edition that appeared earlier in Moscow. The text constitutes the documentation for the article by Robert C. Cook entitled "Walpurgis Week in the Soviet Union" that appeared in THE SCIENTIFIC MONTHLY for June 1949. Other reports on the purge of "anti-Michurinists" appearing in the press and in the *Bulletin of the Atomic Scientists* for May 1949 were based on the less complete accounts printed in *Pravda.* This purported *procès-verbal* amplifies the Russian newspaper articles

The translation is in good English. Faults in use of technical terms or in the rendition of scientific concepts are few, and we may judge that the statements of the speakers are faithfully presented. This book will give anyone who may have thought American reviews and comments on these proceedings to be biased, overdrawn, or nonfactual full opportunity to verify from an authorized USSR publication that the situation is, indeed, as depicted.

The general meeting of the Lenin Academy of Agricultural Sciences was dominated by its president, T. D. Lysenko. He welcomed newly created members of the Academy, named the chairman of the sessions, and then gave the keynote address from which this volume takes its title. The speech was a typical Lysenko harangue, repeating views not essentially different from those of his latest pamphlet, *The Science of Biology Today,* which was reviewed in *Science,* April 22, 1949. Evidence of increased confidence in his power is shown in this speech. First, he boldly criticizes Darwinism as accepted and taught in the USSR, apparently feeling amply protected by Frederick Engels' dialectic arguments of the same tenor. Thus, "Malthusian" errors of Darwin are attacked and the teaching of natural selection belittled as "a summation of age-old practical experience of plant and animal breeders who, long before Darwin, produced varieties of plants and breeds of animals by the empirical method" (p. 12). Some future contender for his place in the sun may seize upon this outright anti-Darwinism of Lysenko to unhorse him. In the second place, Lysenko now feels secure enough to announce without equivocation his adherence to Lamarckism and his belief in the inheritance of acquired characters, matters previously not mentioned in his addresses and not admitted when challenged. These are advocated and put forward as sanctioned by Darwin and Michurin. Belief in the inheritance of acquired characters now becomes the shibboleth to identify Lysenko adherents.

The real purpose of the conference was revealed by the denunciations by Lysenko of fellow-members of the Lenin Academy—I. I. Schmalhausen, B. M. Zavadovsky, and P. M. Zhukovsky—for teaching false Darwinian doctrine (*sensu* Lysenko) and as "Mendelist-Morganists" Other prominent Russian biologists were similarly denounced, among them N. P. Dubinin, corresponding member of the USSR Academy of Sciences; I. Polyakov, Department of Darwinism, University of Kharkov; Y. Polyansky, prorector of the University of Leningrad; and A. R. Zhebrak, geneticist, Timiryazev Agricultural Academy.

In the next eight sessions, long speeches by adherents of Lysenko along the general theme of the conference only thinly cloak the main purpose of the assembly, namely, a complete purge of all so-called Mendelist-Morganists from universities, scientific institutes, and agricultural stations of the USSR. Of twenty-four speeches by members of the Lenin Academy of Agricultural Sciences, twenty were pro-Lysenko and were either denunciations of colleagues or preparations for such attacks. Four members of the Lenin Academy were denounced as "Mendelist-Morganists," and amid taunts and with heckling, in part by Lysenko himself, sought to defend themselves as not being anti-Darwin or anti-Michurin. The agricultural specialists, journalists, administrative officials, and others who took part in this conference followed the precedent of Lysenko and indulged in denunciation of their fellows. They added to the list of those on whom Lysenko had already put the finger, J. A. Rapoport, cytologist, Institute of Cytology, Histology, and Embryology, USSR Academy of Sciences; S. I. Alikhanian, geneticist, University of Moscow; and Academician V. S. Nemchinov, statistician and director, the Timiryazev Agricultural Academy. Of the thirty-two speeches by agricultural specialists and others, not members of the Lenin Academy, twenty-eight were pro-Lysenko and four were futile attempts at defense or of explanation by those attacked.

It is difficult to catalogue as to specialty those who were brought together to consider the fitness or unfitness of colleagues and to determine the biological tenets proper for the USSR. Those under attack are well-known investigators who have made important contributions to science in genetics, Darwinian theory, statistics, or cytology. The pro-Lysenkoites, for the

most part, seem characterized by a passion for anonymity Search of the *Bibliography of Agriculture* for the past ten years shows them either without citations or only as authors of popular articles on agronomy, soil science, general agriculture, and the like; some appear as signators of administrative directives; biochemists who have contributed articles on antibodies, hormones, and blood chemistry are exceptions.

From the publications available and the context of speeches, it would appear that the pro-Lysenko academicians belong in the following fields of specialization: agronomy, 4; biology, 4; plant breeding, 4; animal husbandry, 2; biochemistry, 2; and 1 each in horticulture, soils, agricultural engineering, and genetics. The pro-Lysenkoites who were not Academy members fall into the following categories: agricultural station directors (field of specialization not clear), 8; plant breeders, or Michurin-type geneticists, 7; agricultural administrators, 4; animal breeders, 2; journalists, 2; biochemists, 2; and 1 each, biologist, philosophy professor, and educator. This array of itself indicates the degree of competence of many of the individuals to debate the subject matter of genetics. Any group approval or dissent is obviously without real significance.

In general, the speeches followed a common pattern. Michurin was praised for directing biological thought into proper channels, and Lysenko was glorified for rescuing Michurinism from neglect. Much of the time commonly was devoted to extravagant praise of Lysenko's contributions to Russian agriculture. Many speakers had to rely upon quotations from Lysenko's old addresses, the meager data and specious claims from these sources being quoted and extolled. Frequently the speaker would parade his own work or that of his station and then seek to relate it to the Michurin-Lysenko doctrines. A few reported experiments of the Michurin or Lysenko type that appeared to substantiate the doctrines or were about to do so. None of these was particularly impressive. As the speeches droned on (pages 57–603), each sought to outdo his predecessor in fulsome flattery of Lysenko. No deprecatory statement or gesture on Lysenko's part is anywhere noted in the text, but at times sharp temper was displayed at faint praise or criticism.

The book reinforces the evidence that as a scientist Lysenko is a pygmy. No Haldanish gloss can cover the internal evidences in this volume of the repeated fiascoes that have attended Lysenko's projects. Always the opportunist, he has Potemkinized failures by promises of future successes. As the myth of winter wheat vernalization as a contribution to Soviet agriculture exploded, Lysenko is shown as adroitly suggesting transformation of spring wheat into winter wheat by a process of Michurinistic education to gain winter hardiness and smut resistance! But as he shrinks as a scientist, the book reveals his growth as a consummate politician. Years of scheming and preparation reach successful climax at this meeting. By years of propaganda Lysenko had sold the fettering doctrines of Michurin and had them made party policy. He had in the past ten years planted his henchmen in the agricultural stations and in other positions of power. He had packed the Lenin Academy with his satellites. These were to provide a show of scientific force if such were necessary to accomplish the purge of "Mendelist-Morganists." But decision by the motley group was unnecessary, for Lysenko, the politician, short-circuited the assembly by going directly to the Central Committee of the Communist Party and obtaining the nod from it. After all the speeches were made, Lysenko gave out what R. C. Cook in his article called the "pay-off": "The Central Committee of the Party examined my report and approved it." (*"Stormy applause. Ovation. All rise."*—page 605).

For opponents of Lysenko the jig was up. Then followed a pitiful spectacle as Academician Zhukovsky and Professors Alikhanian and Polyakov abjectly recanted and swore fealty to Lysenko. The sessions closed with the customary letter to Stalin. Lysenko, the politician, lost no time in capitalizing on his victory by drafting a set of resolutions that would force the Academy of Sciences to complete the purge.

If one could overlook the human tragedies involved in this march to power of Lysenko and could disregard the threat to our own freedom that is inherent in state control of man's thinking, then we could dismiss these happenings as medieval and this book as a farrago of nonsense. We could even place it beside *Alice in Wonderland* as high comedy. But this is a deadly serious record, and a menacing one. To the American reader it reveals that in the USSR the situation of biological science is one of utter degradation.

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ANIMALS DOWN UNDER

An Australian Animal Book. (Rev. ed.) Charles Barrett. 374 pp. Illus. $4.50. Oxford University Press. New York.

Charles Barrett, an able "bush naturalist" and writer, has gathered into one volume an account of many of the animals, birds, reptiles, and fishes of his native Australia, a signal work on an amazing fauna—this latter unfortunately thinned in spots by indiscriminate slaughter, so much so that it occurred to me in reading it that "A Study in Extermination" might have been a good subtitle for this book.

Despite the callousness with which the early settlers treated animal life, there still remains in Australia a fauna distinct from that of the rest of the world and reeking with fascinating creatures.

At the present time in Australia, natural history is to the fore. Numerous books have been published,

and this is the third printing of Barrett's. It is recommended to the naturalists of Australia, but it is of interest to naturalists anywhere. He has chosen data written by some of the earliest explorers, has brought it up to date—including the present-day work of David Fleay—and has added many of his own observations. He tells us that the laughing jackass teaches its young to laugh; that the spiny anteater develops a pouch for the purpose of hatching its one egg; that the rifle bird, passing from tree to tree, makes a noise like the shaking of a piece of new stiff silk; that the bowerbirds have been called the most wonderful birds in the world, not especially for their beauty but because one of them builds a most elaborate play hall, a hut of mosses decorated with colored flowers and brilliant beetles. The author has done a fine job in telling something about a vast field.

Some of the colored pictures, from old prints, are delightful, and there are a number of more recent photographs—excellent ones. Although these pictures are good, they are arranged in an exasperating way. Why the American gray squirrel should be in the chapter on marine and fresh-water fishes, and the Australian magpie prefacing the chapter on Australian frogs, is an editorial idea that I cannot comprehend.

It is nevertheless a readable book, full of interesting information.

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SPAIN'S SON OF SCIENCE

Explorer of the Human Brain: The Life of Santiago Ramón y Cajal. Dorothy F. Cannon. xvi + 303 pp. Illus. $4.00. Schuman. New York.

Dr. Dorothy F. Cannon, medical editor for the J. B. Lippincott Co., has performed a real service to science by writing an adequate biography of Santiago Ramón y Cajal, "the greatest scientist Spain has produced and one of the scientific leaders of the world." Though Cajal was a Nobel Prize winner, though many other honors and distinctions were heaped upon him during his lifetime (1852–1934), and though his research upon the nervous system was basic and pioneering, his name is not well known in America except among neurologists and histologists. Dr. Cannon's book, the tenth volume in Schuman's "Life of Science Library," will help correct that.

Like most great thinkers, Cajal's genius radiated in many directions. He was not merely a medical scientist: he was also a dynamic teacher, an ingenious technician, an artist of unusual talent, an investigator of color photography, a creative writer, a philosopher, an educational reformer, and a man of consuming patriotism, whose ambition above everything else was to increase "the sum of Spanish ideas floating in the world." And, withal, he was a man of consummate modesty, of infinite patience and tirelessness. He had an appealing personality. "A bit disconcertingly," says Dr. Cannon, "his weaknesses have proved to be few and are for the most part endearing."

Many of Cajal's discoveries in neural anatomy have today become a fundamental part of neurology. As Dr. Cannon says, they "constitute the basis of our present-day knowledge of the development and structure of the neuron; of the way in which the nervous impulse is transmitted from sense organ to central nervous system and thence to muscle or gland; of the process of degeneration and regeneration of nerve fibers; and of the localization of the various sensory and motor areas of the brain." His Nobel Prize discourse in 1906, *Les structures et connexions des neurones,* made medical history. The new data that he brought to light are of importance to psychologists and psychiatrists: "to a considerable degree it was Cajal's work that made modern neurosurgery and neuropsychiatry possible." Of great significance, too, were the methods he devised for microscopic study, particularly new staining methods. In 1903 he developed the reduced silver-nitrate stain, and in 1913 the gold-sublimate stain, by means of which the neurofibrils, hitherto elusive to study under the microscope, could be traced. These and other ingenious inventions in micrographic techniques opened the way to Cajal's own discoveries and to those of his followers and all future explorers of the nervous system.

Ironically enough, many of Cajal's published papers, year by year reporting upon his researches, did not at first reach a world-wide scientific public, the reason being that they appeared in the scientifically little-read Spanish language, and Cajal lived to see some of his discoveries "rediscovered" by unknowing investigators in other lands. Later most of his works were translated and published in other languages. His *Textura del sistema nerviosa del hombre y de los vertebrados,* also issued in French, is a classic and is, says Dr. Cannon, still the "most complete and accurate description ever made of the more delicate nervous structures." His *Obras literarias completas* were issued in Madrid in 1947.

Into Cajal's story there had to be woven a great deal concerning Spain's civil and political affairs, for his life saw many periods of Spanish turbulence, revolution, and war, including the disgraceful Spanish-American conflict. Cajal lived in no ivory tower, and his long life was closely identified with his country's destiny; and Spanish history from the time of the Cuban uprising in the 1870s (in which he served as medical aide) until the Spanish Civil War of the 1930s is a germane part of his biography.

The whole story of this remarkable doctor is told sympathetically and faithfully by one who knows how to condense material (often technical in nature) and make it interesting for the general reader. The book is prefaced by a memoir of Dr. Cajal by Sir Charles Scott Sherrington, the eminent British physiologist. Appropriately, the volume is published in the fiftieth

anniversary year of Cajal's only visit to the United States, in 1899, when he was one of five distinguished Europeans invited to participate in the Decennial Celebration of Clark University at Worcester.

PAUL H OEHSER

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REPUBLIC ON THE EQUATOR

Ecuador and the Galápagos Islands. Victor Wolfgang von Hagen. ix + 290 pp. Illus. $3.75. University of Oklahoma Press. Norman.

"ECUADOR," says Victor von Hagen in the Introduction to his latest book, "is a geographical paradox Here desert, jungle and snow-covered volcanoes follow each other in immediate succession." He then records his impressions of this "land of troubled topography" in a flavorful and interesting volume which is a blend of historical sketches and the quick-drawn impressions of the seasoned traveler.

When Pizarro led his men south from Panama in 1531, he marched through the land now called Ecuador to the center of Inca power in Peru. Then from Lima he sent his captains north to found Quito, the capital on the equator, in 1534. The city grew to command much of the Andes: out of the conquistadors' hunt for gold came other unexpected riches. They sought emeralds and found balsa wood—they looked for the spicy cinnamon bark and discovered the Amazon. Ecuador gave the world the first potato, rubber, quinine, and the Panama hat!

The author weaves the narrative of the development of these resources and crafts against a background of the constantly contrasting Ecuadorean terrain. It is plain the land fascinates him. He is torn between a desire to see a more modern, rational use made of its riches and an unexpressed wish that it be preserved as one vast national park. The naturalist's love for the unspoiled comes clear in his chapter on *el infierno verde*—the "verdured hell" of the Amazonian headwaters. The next chapter is easily the best in the book—a compact and sensitive analysis of the head-hunters' society—the Jivaro Indians. Another chapter explores the caste structure of Ecuadorean society—in fact, one of the underlying themes of the book is the essential neurosis of Andean dwellers—the duality of European conqueror mingled with Indian, which finds its expression in the "hyperbole and melancholy" of the typical Ecuadorean.

The section on the Galápagos stands apart from the main body of the book—like the islands to which it is devoted. The Ecuador of contrasts and color is barely sensed out here 600 miles in the Pacific. The scene is obviously a favorite one of the author's—abounding in remote enchantment and an almost giddy sense of the primeval world. The islands teem with tortoises and tall tales; with pirates, iguanas, castaways, and penguins. Through the volcanic haze are glimpsed the comings and goings of adventurers and scientists, and the reason why the rocky islands gave Darwin the first clues to his world-shaking conclusions is made crystal clear.

The book is well documented, with notes, interesting photographs by the author, a very full bibliography, and index. Mr. von Hagen calls his book an "informal history." He has adapted and refined much material from his earlier volumes, *Off With Their Heads* and *Educador the Unknown*, but this can hardly be called a reference work. Still it should be recommended as an appealing and useful introduction to Ecuador.

One minor personal note. The author makes a dispirited reference to the "much-publicized Pan-American Highway" on page 68. It is this reviewer's opinion that it is precisely lack of publicity that has delayed the completion of this project, which is of top priority for the social and economic growth of the Americas.

BEN GRAUER

National Broadcasting Company
New York City

NATIVE FLORA

American Wild Flowers. Harold N. Moldenke. xv + 453 pp. Illus. $6.95. Van Nostrand. New York.

THIS is the first popular book to describe wild flowers across the country. Fifty chapters describe about 2,000 species by families or related groups, but they are not arranged in systematic sequence. The descriptions are excellent and include distribution, habitat, and economic notes. Thirty-two color plates show eighty-eight very beautiful habitat views of wild flowers, and thirty-two black-and-white plates with excellent detail show sixty-nine species of wild flowers. An illustrated glossary of five pages explains the comparatively few technical terms used in the descriptions. A selected bibliography of sixty-five books covers the principal floras of the country, followed by a fifty-one-page index to the Latin and common names used. The spelling of the common names largely follows those of the 1942 edition of Standardized Plant Names. When the names give a false idea of the relationship of the flower, as, for example, Douglasfir, which is not a fir, and trailing-arbutus, which is not an arbutus, they are written either as a solid or a hyphenated word. Unfortunately. it was not possible for the author to include a key for the identification of the flowers without much increasing the size and cost of the book, so it will remain largely a valuable reference book for the amateur rather than one for flower identification.

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SOCIETY AND THE SCIENTIST

Controlling Factors in Economic Development. Harold G. Moulton. xii + 397 pp. $4.00. Brookings Institution. Washington.

THE frame of reference within which Dr. Moulton presents his analysis—in effect a summary of the economic studies of the Brookings Institution over the past fifteen years—is the following: There is a sharp dichotomy between "the creative and driving force inherent in the competitive system" and "the stultifying effects of the destruction of individual freedom and initiative that characterize government-controlled and dominated societies. Free enterprise, despite its imperfections, weighs well in the balance."

Interestingly enough, Moulton's optimism—based on capitalism's past performances and future expectation—represents a minority opinion among professional economists. The classical economists, from Ricardo through Cairnes, impressed by the law of diminishing returns, saw no progress open to economic society In fact, Mill, writing in 1848 and refusing to change his mind in subsequent editions of his *Principles,* expected humankind to sink into a stationary state, with the possibilities of improvement in welfare unlikely. Marx was cut from the same cloth; but, for him, revolution and communism would check the inexorable operations of the law of diminishing returns and the iron law of wages.

The fifty years following 1848 were ones of extraordinary advance: in technology, capital formation, productivity, and real wages. The deductive analyses of the academic economists again and again were proved false by engineers and business enterprisers. Despite the triumphs thus achieved, in the earlier period and in the years since 1921, the economists by and large have continued pessimistic. There have been a few exceptions; and Moulton is to be included in this honorable company.

Since 1934, Moulton and his colleagues have been pursuing their course, studying the productive and consumptive processes of the nation, as well as income distribution and capital formation. The new pessimism of Keynes has shaken their faith little. One may note the great contributions to economic analysis made by the Keynesians, notably in the area of fiscal policy; nevertheless, the palm belongs to Moulton.

The march along the path of economic progress is an unending one; technology can solve the problems of dearth; the consumptive process increases and with it go greater efficiencies, more productivity, and higher real wages; the demand for more rather than less capital is a characteristic of our society. Underdeveloped regions can absorb savings for a long time to come. Recurrent depression is a problem, and compensatory financing can get out of hand: but as long as the world remains the poverty-stricken one it is, capital formation must go on. Management of the economic processes is most effective when directed by private enterprise.

Thus Dr. Moulton in this summary volume. This reviewer concurs in these judgments. More of this type of inductive analysis will take the curse off the "dismal science": and for this the earlier classical economists (Ricardo, Mill, Marx) and the present-day classicists (the Keynesians) are responsible.

LOUIS M. HACKER

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Modern Science and its Philosophy. Philipp Frank. 324 pp. $4.50. Harvard University Press. Cambridge, Mass.

IN THIS selection of sixteen of his essays over a period of forty years (1907–1947), Professor Frank's main aim is to indicate the source of, and the cure for, the warfare and the traditional gaps between philosophy and science. In a remarkably consistent manner his writings take the reader through the maze of the differences between science and philosophy existing in three great eras of thought: Aristotelian-Thomistic, Galilean-Newtonian, and, more recently, Machian-Einsteinian. The major causes for the rifts between the two kinds of truths, the metaphysical (philosophic) and the scientific, may be found in the practical needs of society for stability and conservatism on the one hand, and the unquenchable thirst of the enlightened mind for a search beyond the accepted common sense of the times on the other. But the gaps are man-made and can therefore be smoothed out by man. The means: an application to all phenomena of the heretofore fruitful methodology of the exact sciences, namely, logical empiricism. The kernels of this cure were already envisaged by Plato, later by Hume and Comte, and more recently by Mach, Poincaré, Einstein, P. W. Bridgman, and the members of the "Vienna Circle," of which Carnap, Wittgenstein, and Frank, among others, are distinguished members. The unification of the sciences among themselves and between them and metaphysics can be accomplished by the attitude and the methodology suggested by logical empiricism. Truth for this school is the invention of propositions which can be verified by the senses and is not in the world for all time waiting merely to be discovered. Truth is the matching of a logically coherent system of symbols with large classes of phenomena empirically verifiable by intersubjective agreement. In so far as philosophy or science develops concepts which cannot meet these requirements, i.e., the "unique correspondence" of the symbolic structure of a conceptual schema with a body of verifiable data, these concepts are meaningless, lead to misunderstandings, and even to

suppression by force of one group of people over another. In so far as science and philosophy accept the attitude and methodology of logical empiricism, Frank foresees great benefits accruing to man in every phase of his society. When the social order becomes bogged down with an absolute acceptance and rigid enforcement of absolute "truths," both science and philosophy suffer in the further acquisition of new truths, and, indeed, man himself is the loser. When, however, a continuously vigilant attitude on the part of scientists, philosophers, and laymen alike to the possibility that our basic assumptions concerning ourselves and the universe may be wrong, outmoded, and may be replaced by newer, more fruitful assumptions and hypotheses, then will the gaps between philosophy and science vanish and then will it be possible, because testable, for all men to speak a language that is mutually understandable. And, through understanding—peace and progress.

Frank's aims are admirable; his exposition fine; his breadth and depth enormous; but there is one area which is crucial, and this he neglects or takes for granted, namely, the nature and processes of the human mind. In seeking a unification of the sciences through a dementalization of all concepts and "a complete disintegration of traditional philosophy," he seems to imply that the same approach which is used fruitfully in the physical sciences can yield *meaningful* data in the mental and moral sciences. This is an implication which others before Frank—and even contemporaneously—have endeavored to justify by logico-empirical theories and experiments in psychology and the social sciences but, for the most part, have achieved cold, meaningless results. If the "mechanists" and the "robotists" in psychology and sociology have failed, it may be not because they have not been scientific enough, nor because they have not been intelligent enough, but rather because the nature of the phenomena is perhaps of a different order and demands, therefore, a different approach. A dementalized psychology is not psychology at all; it may be good physiology or good physics, but as such leads us no further into an understanding of ourselves, qua human beings, than a mentalized physics or astronomy gives us an understanding of what happens in the physics lab or in the heavens. Until such time that the mentalistic approaches to the study of psychology and the social sciences generally can definitely be shown to have exhausted their usefulness, it might be hurtful to the further progress in these fields to proclaim confidently that the methods of the physical sciences must inevitably replace them But until such time that meaningful results are no longer obtained from either approach, i.e., the mentalistic-psychologic and the behavioristic-robotistic, an attitude of mutual tolerance, even mutual aid, in the respective camps can be the only attitude that will benefit the progress of all science and its philosophy.

This is an altogether sound book and is to be warmly recommended to all those in whatever fields who seek to further knowledge by whatever method Whether or not one agrees with its major premises, one can gain a great deal of stimulation and profit from it. None who reads it can remain unmoved.

PERCY BLACK

Committee on Education, Training and Research in Race Relations
The University of Chicago

Live with Lightning. Mitchell Wilson. 404 pp. $3.00 Little, Brown. Boston.

IT IS amazing that a book of interest to science should be, as well, an absorbing novel. It is due to the equally unusual fact that the author combines in himself two abilities or techniques. His work in "physics has included research in cosmic rays and high frequency phenomena . . . under such Nobel Laureates as Fermi and Rabi." This is his first novel; his short stories have appeared in our better magazines. Yet the apparently disparate talents are actually of the same nature.

As this novel indicates clearly, the very setting up of problems in physics requires imagination and creative ability of a high order. An empty laboratory at Columbia University becomes vibrant with a great machine capable of one million volts in the neutron bombardment experiment. Erik Gorin goes from that work into machine tool industry, not into production or engineering, but into the theoretical physics necessarily basic to them both. One of his problems is "the mathematical relation between the angles of a cutting tool and the breaking stress of a metal." All his previous work, his dissatisfaction with collegiate pettiness and borderline ethics in industry, lead him during the war to New Mexico and atomic energy research. The reader follows step-by-step experimentation and emerges with real knowledge of matters formerly nebulous; that is the book's genuine contribution.

Meanwhile the story of the novel concerns Erik Gorin's home life and the place of a scientist in society. Is it possible for a man wedded to his science to be a normal member of a community, to lead the life of the average husband and father? In answering this question, the author has created Savina, who is a grand girl and just right for her job as Erik's wife. There are other real characters in the book: physicists, professors, and tycoons. The result is a rounded whole—science made interesting in a spectacular novel.

MARJORIE B. SNYDER

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CORRESPONDENCE

SHORE PROCESSES

In the first two paragraphs on page 235 of Mr. Caldwell's interesting article "Beach Erosion," he briefly discusses the use of groins and points out some of the difficulties in trying to control shore erosion. Mr. Caldwell mentions only impermeable groins. I would like to call attention to another type, the permeable groin. which I have seen in use in many places on the Great Lakes and especially along the west shore of Lake Michigan.

In my studies of shore processes, I have had occasion to observe both types rather closely and carefully. As Mr. Caldwell infers but does not discuss, evidently for lack of space, the difficulty of the solid groin is that in acting as a solid wing dam it attempts to completely stop the flow of the shore current that is carrying the sediment in suspension. But, from the nature of the case, all the solid groin can really do is to deflect the current away from the shore. When this happens, of course, an area of still water, or sometimes a gentle reverse current, is present on the upstream side in the angle between the groin and the shore, and here deposition of sediment usually occurs. Unfortunately, unless a series of solid groins is so well designed as to fit the shore conditions within very close limits, this sudden checking of current flow sets up local currents and eddies having greater velocities than the original shore current Where this occurs, erosion is bound to take place. Such currents are apt to be present on the up-current side out near the ends of the groins, and again, in particularly unfortunate cases, a strong reverse eddy develops on the down-current side near the shore. Thus is developed that "sawtooth" effect so often seen in a series of solid groins.

The permeable groin which, instead of acting as a dam, allows a part of the water to pass through it, avoids most of the above difficulties and, at the same time, is equally effective in causing accretion along the shore and preventing erosion. The principles involved are the same as with the solid groin. But, instead of attempting to stop completely the flow of the shore current, the permeable groin seeks only to reduce its velocity and thus cause a partial deposition of the sediment being carried in the shore current. It is evident that complete deposition is not necessary, for, if the effect is such that deposition exceeds erosion, accretion occurs and the purpose of the structure is fulfilled. Because some of the sediment is carried through the permeable groin, deposition takes place on both its up-current and down-current sides. Thus the objectionable sawtooth effect does not develop and there is no "robbing" of the shore, as so often takes place on the down-current side of solid groins.

Of course, neither type of groin can cause accretion unless a shore current is present that carries sediment. On the other hand, on a shore along which no sediment is being transported, the permeable groin seems more protective and less likely to cause damage, since it does not result in dangerous local eddies and currents.

I believe the permeable groin has not been used much along the ocean. However, in view of its apparent advantages and its successful use on the Great Lakes, it would seem that a greater use of it along ocean shores might be desirable.

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THE ANGLEWORM UNMASKED

Dr. James Thorp, writing in the March issue of THE SCIENTIFIC MONTHLY on "Effects of Certain Animals that Live in Soils," gives all the evidence necessary to debunk the angleworm. To see the full story of the angleworm in its relation to agriculture, let us analyze his remarks.

Dr. Thorp says: " . . where vegetation is of a type that is appetizing to them, earthworms are very active in converting raw vegetable matter to humus and in mixing the humus with the mineral portion of the soil."

This remark must be modified in two points. First, the raw vegetable matter contained synthesized energy. The worms ate the vegetation and utilized a predominant portion of the synthesized energy in their own body functions, thus preventing its ever coming in contact with roots The small remainder of unused energy, which is unquestionably good plant food, is deposited in their castings. Second, these worm castings are placed on *top* of the ground where the valuable fertilizer which they contain is washed away and deposited on lowlands already covered with a greater accumulation of humus than is needed.

Thorp presents an illustration of soil that had been covered with fallen leaves, which worms had devoured, leaving small heaps of bare leaf stems (petioles). As a very successful grower of avocados, let me assure the reader that if this had happened in an avocado grove the results would have been disastrous. There are other trees that are *surface feeders*, too The successful avocado grower prizes an accumulation of mulch and would look with much disfavor upon any agency that would strip his soil of its naturally decaying tree food. He would equally dread having the surface of the ground uncovered to the drying effects of sun and wind when his greatest secret of success lies in keeping the *surface* of the soil *moist* and covered with decaying vegetation.

"It is not necessary to assume that the worms have added any new material to the soil . . . but . . . they have facilitated the conversion of raw organic matter to humus . . . ," says Thorp. Why hide the fact that the worms have actually removed the greater part of the nutritive material from the vegetation that they ate? Note that the farmer is in no special hurry to have this year's fallen leaves converted into humus at the expense of the greater part of it! He is satisfied to have the lowest stratum of ten years' supply decay into plant food, remaining on the ground instead of being washed away—remaining there under a cover of dead leaves which also conserves moisture.

The author also admits that worms do not relish pine needles, yet many readers have seen pine or giant redwoods that look fairly prosperous without recourse to angleworms. He says that "Even in desert regions they soon become abundant when moisture conditions and food supply are improved. . . ." He might have added that rats will congregate around unprotected grain.

The ancients said that the earth was flat. Darwin said that earthworms are good. Fortunately for Southern California, earthworms do not eat avocado leaves, at least to any appreciable extent.

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